Author's response to reviews

Title: Food Consumption Frequency and Perceived Stress and Depressive Symptoms among Students in Three European Countries

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Author's response to reviews: see over
Response to reviewers’ comments

General comment: We agree with the criticism of the factor analytic approach by the second reviewer and have now replaced the analysis by grouping of the food items based on theoretical considerations. Consequently some of the results changed and the discussion was adapted accordingly. See specific comments below:

Response to specific comments
[Responses in bold face, interspaced]

Reviewer: Chunhong Liu
Reviewer's report:
- Major Compulsory Revisions
  1) In section sample of method, it will help the reader to know the sampling procedure. How to sample the university? How to sample the students from each of the three areas.

Explanations about the sampling have now been added [Page 5, first paragraph].

2) How about the Cronbach's alpha of the PSS and M-BDI in this study?

This information has now been added [page 6, end of the second and third paragraph]

3) In the section “variables”. Has the food frequency questionnaire been previously validated?

The version of the food frequency questionnaire that we used was not validated against more precise food intake measures. However, validity of a similar food frequency questionnaires was demonstrated by comparison with a food consumption history interview. We have now clarified these points in the methods section (bottom of page 5 and following) and also included the lack of formal validation among limitations (page 11). See also further comments below.

4) In Table 1. The title is “food consumption by country and gender”, but the last two lines were the mean values for scale scores.

Title of the table has now been modified.

5) In Table 1. The values should be expressed as Mean ± SD.

SD has now been added, both mean and SD are now reported with one decimal place only
6) In Table 1. There are two factors across the six groups. One-way ANOVA is simply not appropriate. Two-way ANOVA should be used to analyze the effects of gender and country on the food consumption.

We followed the recommendation of the third reviewer and dropped the significance tests from Table 1, since it is only a crude presentation of the data before the final analysis.

7) In the section “discussion”.
   a) Concerning the association between fruit/vegetables intake and depression, the authors may need to give one interpretation at least.

   We have now provided an interpretation that the observed association is likely to be the effect of behavioural consequences of depression [see page 9, last paragraph].
   b) The discussion focuses on meat/fish and depression. What about the possible mechanisms? Authors may attempt to discuss further mechanisms relating to omega-3 fatty acids and depression.

   The new analysis produced no significant findings for fish consumption. We have now modified the discussion accordingly.

- Minor Essential Revisions
  1) In the section “variables”. The author said that there were 10 indicator variables, but 12 food items were found in Table 1.

   This has now been corrected

  2) ANOVA was used in Table 1, but the author didn’t illuminate it in section “statistical analysis”.

   ANOVA has been removed from Table 1.

Reviewer: Craig Hadley
Reviewer’s report:
This is a study of ~1250 students from three European countries that examines the association between patterns of food consumption and mental health. The key finding of this survey-based and cross sectional study is that for women, perceived stress was associated with greater consumption of sweets and fast foods, and reduced consumption of fruits/vegetables and meat/fish. Depression was associated, in women, with reduced consumption of FVG and meat/fish. No associations were observed for males. The authors suggest that these patterns are consistent with a model that links distress and depression to the consumption of sweet foods because these foods reduce stress. As such they should cite the important work of Dallmann (nicely reviewed in: Dallmann et al. Chronic stress and obesity: A new view of “comfort food”. PNAS. Vol 100 no. pp11696-11701). There are a number of strengths to the study, including the large sample size, the high response rates, the cross-country sample, and the innovative focus on the link between diet and mental health.

Major Compulsory Revisions:
I was more concerned with the following issues which could either be corrected or more fully addressed in the limitations and discussion section. The authors say but do not show that the FFQ has been validated – can they provide more evidence that these is a valid tool?

We have now modified the description of the FFQ and discuss in more detail the validity issue in the methods and limitations sections (see also above). The validity of simple questions asking about the frequency of consumption of fish for example seems obvious. But this FFQ did not measure the portion sizes and this might be more crucial, especially in international research, where additional variation in the sizes of serving (portions) can be expected.

Why use factor analysis? The authors say this groups foods of a similar type, this does not seem to be the case since Table 2 shows that soft drinks load high on the meat and fish subscale and milk and cereal load high on factor 4. Moreover, factors 3 & 4 have unacceptable internal consistency (and factor 1 is just below the standard cutoff of .70).

We agree with the reviewer in relation to factor analysis and have now replaced the analysis by grouping the foods based on prior knowledge.

The authors find that sweets consumption is not associated with depression and suggest that consumption of sweets may protect individuals from depression. They show that sweets are associated with stress but not depression – why not push this further and carry out an analysis with of depression that includes both sweets and stress and the interaction. If the hypothesis is correct, then those who consume low sweets but have high stress will be more likely to be depressed compared to those who consume high sweets and have high stress.

Such interaction existed but was not significant at the 0.05 level. Another more substantial problem is related to the theory of mental health indicators: not only they are strongly statistically correlated, but also there is no such concept as perceived stress after removal of depressive symptoms and vice-versa. Therefore, we do not analyse one as predictor of the other.

Minor revisions:
Abstract: Methods: Change ‘liner’ to “linear”

Changed

Table 3 is unreadable

We modified the table (table 2 in the revised manuscript) to improve readability. Since, our interpretation is only based on significance and effect estimates, we have now also dropped the Confidence intervals.

Reviewer: Craig Gundersen
Reviewer's report:
The methods being used in this paper are appropriate for the questions being asked. The writing is excellent. I just have a few comments in the order in which they appear in the paper. These are by page and paragraph number where a paragraph that began on the previous page is denoted with a “0”.
We agree with the reviewer and have modified the wording without overstating the conclusion, given the cross-sectional nature of the study.

We were only able to review the first two papers recommended by the reviewer. Both examine associations between psychological stress and obesity in children and we have now cited them (page 3, paragraph 2).

Please refer to our response to the first remark from this reviewer above. We have changed the wording in the abstract (page 3, paragraph 3) and in the conclusion.

Following this suggestion, and also in line with the suggestions of the other reviewer, we have now replaced factor analysis by grouping food groups based on a priori criteria.

(7,1) Since male is the opposite of female, both percentages are not needed in the opening sentence. (Plus, there is a typo in the sentence.)
Male percentage has now been removed and all typos corrected.

(8,3) The final sentence is a source of measurement error and should be noted under limitations.

**We have now removed the sentence.**

(9,1; 9,2) There is nothing wrong with these paragraphs but they are not germane to the central points being made in the paper. In other words, these paragraphs deal with the levels of depression/stress and the levels of nutrition, not with the connection between them.

**We agree, the corresponding sections have now been removed.**

(11,1) The second sentence is definitely understated – without a doubt, this data is not representative.

**We now highlight that our data is not considered to be representative for the studied countries. However, for the studied question the internal validity is of importance, which is supported by high response rate in each of the sites.**

(Table 1) The p-values for differences between the columns are not particularly interesting insofar as they are conflating information about, say, differences by gender in a country and across countries. I would encourage the authors to drop this column. But, if they are going to keep p-values, I would think more carefully about what exactly sorts of comparisons should be made.

**We have now deleted the p-values from the Table.**

Other changes

In the light of the constructive comments by the reviewers, we have now added an acknowledgement section thanking the reviewers for their comments that helped strengthen the manuscript significantly.