Author's response to reviews

Title: Stress-related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study

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Author's response to reviews: see over
Dear BMC Public Health Editors,

Thank you for the opportunity to revise and resubmit our manuscript “Stress-related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study” (ID#1293366199945455). We are grateful to the Reviewers for the valuable comments. In our response letter, we provide a point-by-point description of the changes made to the manuscript according to the Reviewers’ suggestions.

Thank you again for assessing our manuscript. We look forward to hearing from you regarding our submission in due time. We would also be pleased to respond to any further questions and comments you may have.

We hope that you will find our manuscript suitable for publication in *BMC Public Health*.

Sincerely,

Anne Jääskeläinen
Corresponding Author
Reviewer’s report
Title: Stress-related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study

Version: 1 Date: 19 October 2013

Reviewer: Wendy H Oddy

Reviewer’s report:
Stress related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study
The aim of this study was to investigate the prevalence of stress-related eating and its association with overweight, obesity, abdominal obesity, dietary and other health behaviours at the age of 16. A further aim of this study was to examine whether stress-related eating is predicted by early life factors including birth size and maternal gestational health. The study population was 3598 girls and 3347 boys taking part in the 1986 Northern Finland Birth Cohort. Followed up from before birth, adolescents underwent a clinical examination and all behaviours were assessed by postal questionnaire. Latent class analysis and logistic regression were used to profile adolescent’s risk of obesity with behavioural traits.

Major Compulsory Revisions
The author must respond to these before a decision on publication can be reached.

There are no page numbers given with this manuscript. This makes it very difficult to review. Hence no page numbers can be given linked to comments below.

Author’s response: Page numbers are added to the document.

Data collection and study variables section.
Please define what age these BMI cut-off points were for (16 years).

Author’s response: BMI cut-off points are added in the text (page 5: “The BMI cut-off points for overweight and obesity (corresponding to adult BMI of 25.0 and 30.0 kg/m²) were 23.90 and 28.88 kg/m² for 16-year-old boys and 24.37 and 29.43 kg/m² for 16-year-old girls”.

Define NFBC.

Author’s response: The abbreviation is written out in full in the abstract and under Methods/Study population (p.4).

Under Indicators of early life stress section, please reference the definition of gestational weight gain used in this manuscript.

Author’s response: The gestational weight classification used in this paper was based on cohort-specific quartile cut-off points and derived from our previous study (Laitinen et al. Maternal weight gain during the first half of pregnancy and offspring obesity at 16 years – a prospective cohort study. BJOG 119: 716-723, 2012) which is now included in the reference list.

Under food and beverage consumption section, the authors mention that alcoholic beverages were included, but were sausages included?

Author’s response: Alcohol and sausages were included in the analyses as separate items. We describe the categorisations as follows (p.7): "Frequent intake of sausages/frankfurters was defined as twice a week or more often, for other foods and non-alcoholic beverages frequent consumption was defined as 3–5 times a week or
more often. Alcohol intake was classified as 'never', 'randomly' and 'once a month or more often'.”

The cigarettes question would be difficult to delineate what was cigarette and what was snuff.

Author’s response: There were separate questions for cigarette smoking and snuff use, and the responses were combined for the analyses; we classified also those individuals who reported using snuff but not smoking cigarettes as tobacco users. We added the exact wording of the questions concerning cigarette smoking and snuff use as requested by Reviewer #2 (p. 7).

Results
Please provide some idea of numbers included in the survey.

Author’s response: We have presented the number of survey respondents in the Study population section "Of those adolescents alive and traceable (n=9215), 7344 (80%) responded to a postal questionnaire inquiring about their health and well-being” (p. 4).

Can non-stress eating be defined?

Author’s response: Non-stress eating means here that the response to the question "When you encounter stress in life, e.g. a difficult matter, occasion or situation, do you try to make yourself feel better by eating?” was ‘never’. This is explained in the Methods section (Stress-related eating, p. 6).

In Table 3, was the stress related eating referring to the adolescents or their mothers, 16 years earlier?

Author’s response: Stress-related eating (i.e. the outcome variable throughout the paper) was measured only from the adolescents.

Was maternal stress during pregnancy accounted for?

Author’s response: Due to a lack of direct measures of in utero stress (e.g. maternal cortisol levels), we used indirect indicators of stress such as alcohol use and offspring birth size.

Use of the word ‘might’ would benefit by being replaced by the word ‘may’.

Author’s response: We have replaced the word ‘might’ by ‘may’ at appropriate places in the manuscript.

Layout of Tables could be improved.

Author’s response: We constructed the tables according to the journal’s instructions and modified them taking into account the Reviewers’ suggestions.

Table 1 – provide p-values to show differences.

Author’s response: In Table 1, we now provide p-values from Pearson’s X² test for categorical data. Differences of means (for continuous variables) between girls and boys are demonstrated using confidence intervals.
Table 2 – was there a gender interaction effect? If not, why were boys and girls studied separately?

Author’s response: We chose gender stratification because there were certain differences in characteristics between boys and girls as shown in Table 1 and the study population was large enough to allow for separate analysis.

Table 3 – Were these associations in the mothers of study participants?

Author’s response: No - stress-related eating refers to the adolescents in all tables in the manuscript.

Table 5: Include ‘bivariate’ in the title of the table.

Author’s response: We combined Tables 4 and 5 and modified the title of the table as follows: Table 4. Association between stress-driven eating behaviour and consumption of sweet or salty snack-type foods and beverages and Table 5. Association between stress-driven eating behaviour and other health behaviours into Table 4. Association between stress-driven eating behaviour, consumption of sweet or salty snack-type foods and beverages and other health behaviours.

Can the tables be justified as there are 8 tables which are too many? Some of the tables could be supplementary.

Author’s response: By combining Tables 4 and 5 we reduced the number of tables; we also removed Figure 1 due to redundancy, as suggested by Reviewer #2.

Table 7 – it is not clear what Table 7 shows.

Author’s response: Table 7 shows how the adolescents’ response patterns clustered in three health behaviour profiles (latent classes). We then labeled the profiles based on their content.

Table 8 – there is little difference between boys and girls. Why are they considered separately?

Author’s response: We agree that the results of the logistic regression analysis are quite similar for boys and girls; however, we decided to conduct separate analyses for the sake of consistency and clarity.

The figures are very complex and not easy to understand.

Author’s response: We have removed Figure 1, as suggested by Reviewer #2.

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

Quality of written English: Needs some language corrections before being published

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests: No competing interests are declared.
Reviewer's report

Title: Stress-related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study

Version: 1 Date: 22 October 2013

Reviewer: Georgina Sophie Alice Trapp

Reviewer's report:
Review of article titled "Stress-related eating, obesity and associated behavioural traits in adolescents: a prospective population-based cohort study"

This paper uses a large population birth cohort of Finnish adolescents to examine cross-sectional associations between a range of adiposity and health behaviour measures and stress-related eating at 16 years of age and stratifies results by gender. Prenatal and perinatal factors are also examined in relation to stress-related eating at 16 years. Latent class analysis was used to examine the risk of obesity based on behavioural characteristics. A high proportion of adolescents reporting stress-related eating was found, particularly in girls. The paper is a useful addition to the literature given the lack of studies involving an adolescent sample. It is disappointing that the authors did not include multivariate models when examining associations with stress-related eating and did not include information on or adjust for socio-economic factors.

Discretionary revisions
1. Consider dropping Figure 1? – I don’t think it adds all that much and it is very difficult to interpret.

   Author’s response: We acknowledge that the readers would probably have found Figure 1 redundant and complicated to interpret and thus, it was removed from the paper.

2. Consider creating a single scale variable out of the 7 food items.

   Author’s response: The observed differences between boys and girls would not have been possible to detect using food items as a single composite variable. Hence, our view is that one scale variable would not have rendered essential additional information on the associations.

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

3. Methods section, study population paragraph – could it be made clearer that the sample used in this study are participants who completed the 16 year follow-up. As is, only dates of 2001-2002 are presented.

   Author’s response: We edited the paragraph as suggested by the Reviewer: “The mothers and their offspring have been studied prospectively since the 12th gestational week and the most recent follow-up was carried out in 2001-2002 at offspring age 16 years. Of those adolescents alive and traceable at the 16-year follow-up (n=9215)...”

4. Results section, paragraph 4 – first sentence needs editing.

   Author’s response: We have modified the beginning of the sentence as follows: “As regards diet features (Table 4)…” -> “As regards the consumption of snack-type foods and beverages (Table 4)…”

5. Table 7 – include a footnote with the labels assigned to the three latent classes.

   Author’s response: Table 6 (formerly Table 7) now includes a footnote with the latent class labels.
- Major Compulsory Revisions (which the author must respond to before a decision on publication can be reached)

Participant attrition

6. Were there any significant differences between the 6945 participants included in this paper and the original cohort of 9432 (i.e., differences in terms of gender, socio-demographics, SES, prenatal and perinatal factors etc)?

Author’s response: Thank you for pointing out the important issue of potential selection bias. Previously, Kapi et al. (Acta Paediatrica 2007; 96: 1174-1179) undertook the analysis of differences between initial and follow-up study populations of the NFBC1986 and found the latter to be a representative sample of the original cohort: "When we compared the background factors of the Finnish sample in 2001 to those of the children in the initial birth cohort, there were no differences in birth weight and length. In the 2001 study there were more children whose mothers had been married when the child was born (82% vs. 72%) and less those whose mothers were divorced (0.70% vs. 1.20%; p < 0.0001) than in the comparison group. The mothers of the children in the latest study were slightly more educated and the fathers’ social class was slightly higher (p < 0.0001 for both), but all levels of education and social classes were adequately represented in the 2001 study. Like in many studies, in 2001 females were over-represented (52% in the year 2001 vs. 47% in the years 1985/6), but, because most of the outcomes were studied separately for boys and girls, this was unlikely to cause any bias in the results. In conclusion, the data collected in the year 2001 was fairly well representative of the initial northern Finnish birth cohort. Besides, the sample size was large, the response rates were high, and, therefore, this could be considered a representative sample of adolescents living in northern Finland.”

Definition, validity and reliability of items

Methods section

7. Gestational weight gain – Can it be made clearer how this was calculated/collection...was pre-pregnancy weight self-reported via questionnaire at the first antenatal visit and then weight objectively measured at the 20th week of pregnancy? A comment on the validity of this measure should be included in the discussion.

Author’s response: We have previously used the weight gain classification and discussed it in more detail in Laitinen et al. Maternal weight gain during the first half of pregnancy and offspring obesity at 16 years – a prospective cohort study. BJOG 119: 716-723, 2012. This paper is now included in the reference list.

8. Food and drink consumption variables – information relating to the reliability and validity of the FFQ used needs to be included here.

Author’s response: Thank you for this essential remark on the reliability and validity of the FFQ. The questionnaire was specially constructed for the 16-year follow-up data collection. However, the associations between snack-type food consumption and stress-related eating behaviour were already shown in adults from the same geographic area (Laitinen et al. Stress-related eating behavior and body mass index and predictors of this behavior. Prev Med 2002, 34: 29-39). We consider the similar associations as an indication of the validity of food consumption measurement; this approach for validity assessment has also been presented in the literature by Willett: “The use of a questionnaire to demonstrate established relationships between a dietary factor and disease can be interpreted as qualitative support for questionnaire validity” (Reproducibility and validity of food-frequency questionnaires. In: Willett WC. Nutritional Epidemiology, 2nd Edition, New York: Oxford University Press, 1998, p.130).

9. Gestational alcohol and smoking variables – include the exact wording of the
questions used.

Author’s response: We included the exact wording of the questions (with response categories) concerning maternal gestational alcohol use and cigarette smoking (p. 6): “Maternal alcohol use and smoking were assessed using the following questions: ‘Have you drank alcohol during this pregnancy (yes / no)?’ and ‘The number of cigarettes after the 2nd month of pregnancy (none / 1-9 / >9)?’”

10. Other health behaviour variables – exact wording of the questions used and information relating to the sources/reliability/validity of each of these items needs to be included.

Author’s response: We have added the exact wording of the questions concerning adolescents’ health behaviours (tobacco use, physical activity, sleeping habits, weight control practices, eating with family; page 7).

Analyses & Results

11. Given that poor socio-economic circumstances have been shown to predict mental health problems in childhood and adulthood, data on socio-economic factors such as family income, parental employment, parental education, neighbourhood characteristics need to be included in analyses and in the description of the sample.

Author’s response: We included maternal basic education level (self-reported by parents) and parental interest in their child’s hobbies and education (self-reported by adolescents) in the analyses (p.8, Tables 1 and 7).

Results tables

- 12. Table titles should state that the data is from the 16 year follow-up and include the years of data collection.

Author’s response: This information has been added to the title of Table 1.

- 13. Why are 95% CI’s presented for the means and not standard deviations?

Author’s response: Confidence intervals instead of standard deviations are commonly used in epidemiologic studies; they replace p-values and thus counteract the problem of multiple testing; 95% CI correlates to 1.96 SD.

- 14. Why aren’t p values provided for continuous variables (i.e., why were independent t tests not performed?)

Author’s response: We consider p-values unnecessary when confidence intervals are presented (for continuous variables in Tables 1 and 2).

- 15. Variable names are not consistent

Author’s response: We have checked the consistency of variable names and made corrections accordingly. However, e.g. stress-related eating / stress-induced eating and stress-driven eaters / stress-eaters and bingeing / binge eating behaviour / devouring large amounts of food are used interchangeably.

- 16. Given that mostly all independent variables have been coded as categorical or binary rather than as continuous variables, I worry that some important associations with stress-related eating may have been missed. I would suggest the authors use continuous variables where possible and run the results again – this would also save a lot of table space as the results in Tables 2,3, 4 and 5
should really be presented together in one table.

Author’s response: Most of the data were questionnaire-based and categorical to begin with and then further categorized for the analyses.

- 17. After univariate associations with stress-related eating were identified, why weren’t multivariate models constructed and confounding variables (i.e., socio-economic factors) adjusted for? This would have taken the analyses to the next level and would have greatly enhanced the paper.

Author’s response: According to the Reviewer’s suggestion, we included maternal education level and parental attentiveness in the logistic regression analysis and present both unadjusted and adjusted odds ratios with 95% CIs in Table 7.

- 18. Table 7 & 8 – socio-economic factors need to be included.

Author’s response: The odds ratios in Table 7 (originally Table 8) Unadjusted and adjusted associations between the three identified health behaviour clusters and obesity measures are adjusted for maternal basic education level, i.e. a proxy for family socio-economic status. However, we did not add socio-economic factors to health behaviours included in Table 6 (originally Table 7) Clustering of study population into three latent classes based on health behaviours.

Discussion section
19. The limitations section needs to be expanded on – i.e., use caution when interpreting results given the (lack of) validity of measures used, most associations were only cross-sectional, list of factors associated with stress-driven eaters not exhaustive, no adjustment for co-existing mental health problems, no inclusion of socioeconomic factors etc.

Author’s response: We discuss study limitations as suggested by the Reviewer (p. 14): “Given the lack of validity of the measures and the use of cross-sectional data, the results should be interpreted with caution. --- It is probable that other factors that were not included in the analyses, e.g. psychological traits including restrained and emotional eating behaviour, are associated with stress-related eating behaviour.”

20. Paragraph 2 – Unless the authors performed a validity assessment on the stress-related eating measure, it is incorrect to summarise that “the results indicate that a single item used in this study to assess stress-related eating is a valid instrument for use in health examinations at school”.

Author’s response: As shown in Table 2, stress-related eating is associated with body mass index (as both a continuous and categorical variable). In nutrition research, a such linear association between eating behaviour and a physiological variable is an indication of measurement validity. Furthermore, the item has been found to be a valid tool to identify high-risk individuals among adults (Laitinen et al. Stress-related eating behavior and body mass index and predictors of this behavior. Prev Med 2002, 34: 29-39): “Although it may seem to be arbitrary to classify people as stress-driven eaters and drinkers on the basis of only one item, our results showed that such persons could be distinguished from others by this item. It is easy, cheap, and fast to use this single item to identify a high-risk group of stress-driven eaters and drinkers during health examinations for young adults provided by occupational health care services and to focus an intervention to prevent the development of addictive behavior and obesity, for example”.

21. Overall, the discussion is not clearly organised - I suggest the standard topics should be covered ... quick summary of main findings, how these results fit/don’t fit with prior literature, limitations, suggestions for future research, implications of the findings for health, policy.
Author’s response: The discussion has been modified according to the Reviewer’s request. The order of the content is: Short summary of main findings, Findings in light of prior literature, Findings related to early-life factors, Role of behavioural factors related to stress-related eating, Interpretation of latent cluster analysis, Role of restrained eating and other psychological factors, Strengths/limitations, Suggestions for future research, Implications for health policy.

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

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests: 'I declare that I have no competing interests'