Author’s response to reviews

Title: Parental food-related behaviors and family mealtime frequency: associations in Norwegian dyads of parents and preadolescent children

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Author's response to reviews: see over
Dear editors and reviewers,  

We would like to thank you for your evaluation and appreciation of our research. The reviewers’ comments are appropriate and will be addressed below.

Reviewer # 1: Katie Loth

Reviewer's report:

Major Compulsory Revisions

Abstract

1. Very minimal information included on results and conclusions. Would suggest limiting background and adding in additional details on results and more specific study conclusions.

Response: The abstract is revised as suggested.

Introduction

2. Would define extreme weight control behaviors or offer an example to help readers to understand what these behaviors are.

Response: We have offered examples as suggested (p. 3, first paragraph).

3. More information is needed to follow-up on the first sentence in the second paragraph. What type of associations between parenting style/family cohesion and family meals have been seen? This is an important piece that does not seem to be followed up on in this paragraph.

Response: Thank you for making us aware of this. We have expanded the background section to elaborate more on these issues (p.3-4).

4. The “affective environment of the home” and “parental feeding practices” should be defined.

Response: These concepts are defined as suggested (p. 5).

5. The introduction does not make it clear why one might think that parental feeding practices would be associated with family meal frequency. It states that this has not been looked it, but it is not clear why it is being explore within this paper or what the theory is for what might be found.

Response: We have expanded the introduction (background section) to try to clarify why we want to explore these associations (p. 5-6).

6. The authors state that multiple parent reported feeding practices will be assessed as a study strength, but do not list what different feeding practices will be assessed.

Response: We have revised the manuscript to include the feeding practices measure being applied (the CFPQ) (p. 6). The measure is described in more detail in the methods section (p. 8-9).
7. Why is it important to look at this in older children (vs. younger children)? Do the authors have a reason to believe it will be similar? Or different?

Response: The rationale for looking at this in older children is included in the revised manuscript (p. 6-7).

8. Overall the authors do not do a good job of helping the reader to understand WHY this association is being examined. What is known now? What do they expect this will add? And what will it mean when the findings are known or what will next steps be?

Response: We have expanded the introduction to try to clarify the “whys” and “so whats”.

Methods

9. Were all parents included in the study? It seems that more parents responded that children, so I would assume that those parents who responded but did not have a child participate were not included? This should be more clearly stated.

Response: Thank you for making us aware of this missing information. Only parent-child dyads with complete datasets for each of the models tested were included in model analyses. The manuscript has been revised to clarify this issue (first paragraph, p. 11).

10. More information on pretesting should be included.

Response: More information on pre-testing is included as suggested (second paragraph, p. 8).

11. The authors should provide more information on the appropriateness of the CFPQ for this population. Also, do they have test retest data for this measure within this population, or other similar populations that could be included?

Response: A reference on the appropriateness of the CFPQ in this population is included in the revised manuscript (end of first paragraph, p. 9). Unfortunately, we do not have data on test-retest reliability in our sample (only internal consistency reliability which is presented in Table 1).

12. How was the “parent most involved with meals” chosen for participation (over the other parent in the home)? Was this parent’s choice or somehow chosen by researchers?

Response: The “parent most involved…” was the parents’ own choice (self-selection) as clarified in the revised manuscript p. 7.

Results

13. It would be of interest for the authors to be more clear about the direction of associations seen within the text of their results section. As the results read it is very difficult to understand if family meal frequency is higher among homes with particular feeding behaviors or lower.

Response: Because of the cross-sectional nature of our study we cannot be clear on the
direction of associations seen. Nevertheless, in the discussions section, we hint at a dynamic, bidirectional model.

14. Overall the results section is very limited and more details should be included and discussed within text.

Response: We have expanded the result section (and the methods section) by including results from t-tests (p. 11, first paragraph and p. 12, second paragraph). Our main findings are elaborated in the discussion section.

Discussion

15. It is unclear within the discussion section what the authors overall theoretical model for the study is. While the study is cross-sectional and therefore cannot assess temporality of associations it would still be of interest for the authors to speculate on the direction of associations and the potential underlying mechanisms. (Previous research can be instructive in this regard.)

Response: We have tried to clarify this in the revised discussions section.

16. It is also unclear if the authors have a good understanding of what is known on the benefits and drawbacks of particular behaviors, such as restriction and pressure-to-eat. This should be discussed more clearly.

Response: We have included a discussion on these issues in the revised manuscript (p. 14-15).

17. The third paragraph of the discussion seems unrelated to study findings and should be more clearly integrated to the overall study aims or dropped from the paper.

Response: We agree, and have decided to drop this paragraph from the paper.

18. I find that the authors continued description of study findings in terms of the amount of variance explained is difficult to interpret and seems overly specific for the discussion section. It would be more appropriate for authors to use the discussion to explain findings in more detail and to discuss their clinical or practical meaning in more tangible way.

Response: We agree, and have revised the manuscript to focus on discussions of our main findings.

19. The limitations are discussed and the cross-sectional design is brought up, but I would like to see more discussion on the limitation of not being able to assess directionality of associations.

Response: We have expanded the manuscript to include more discussion on this issue.

Conclusions

20. The conclusions are very vague. What do the authors think the main message is? What would they recommend based on these findings? What are next steps?
Response: We have tried to be more clear about the conclusions in the revised manuscript (p. 17).

Reviewer # 2: Mette Rasmussen

Reviewer's report:

Parental food-related behaviors and family mealtime frequency: associations in Norwegian dyads of parents and preadolescent children.

General comments:

This manuscript focus on an interesting and relevant topic that is still studied only to limited extends. The manuscript is generally well written, but suffers from a number of major limitations especially related to abstract, methods/analyses and discussion. Please find my detailed comments below.

Major compulsory revisions:

Abstract:

The abstract needs a better balance. Too much focus is placed on the background section not leaving room for more detailed information on methods and results. The use of the CFPQ for measuring feeding practices should be mentioned in the methods section. In the result section, the most important estimates of association should be reported. In the conclusion, the wording appears misleading. It says ‘Our results indicate that especially the home environment may serve as a positive influential factor regarding the frequency of family meals’. This indicates that especially home environment is an important factor among all potential factors influencing meal frequencies. It should be clear that home environment is especially important when specifically considering feeding practices. Also it should shortly be stated what is meant by home environment.

Response: The abstract is revised as suggested.

Introduction:

As a reader of the introduction, one would like to know in more details what is meant by feeding practices. A clear understanding of what this involves does not come until the specific measures are presented in the methods section. The introduction would benefit from more focus on the definition of feeding practices. Thereby, it gets clearer what this study is about when reading the research questions.

Response: The introduction is expanded to include more details on feeding practices (p. 5).

The third research question should be deleted. First, it is overlapping with research question 1 and second, it indicates some extend of causality which cannot be studied in the applied cross-sectional data. Also, the direction of the associations between feeding practices and family meal frequencies may not be straight forward. As indicated in the discussion the two concepts may be
outcomes of a common latent variable – e.g. a general high value and positive attitude towards healthy living within the family. Therefore, the directions of the associations cannot be anticipated and indicated.

Response: We have deleted the third RQ as suggested.

Else, the structure of the introduction is sound.

Response: Thank you!

Methods:

What is meant by ‘dinner’? Supper is an evening meal and breakfast is a morning meal. If a mid-day meal is what is meant by using the term ‘dinner’, I suggest replacing it with ‘lunch’. Please specify.

Response: The dinner concept (+ other typical Norwegian meals) is clarified in the revised manuscript (p. 10, first paragraph).

Statistical analyses:

It is a major limitation of the study that no confounders are included in the analytical model and analyses. Several important confounders may exist of relevance for the association between feeding practices and meal frequencies - including family structure and parental socioeconomic

Response: Including potential confounders could of course be interesting, but as this work is quite explorative (to our knowledge the relationships between parental feeding practices and family meal frequencies have not been investigated before) we aimed to test general associations (basic models) in the population of interest – irrespective of gender, SES etc. However, as these associations/models are further studied, inclusion of potential confounders is needed to inform future family-oriented interventions in different parts of the population.

Also, the applied data are collected by cluster sampling introducing a potential risk of dependent data. Therefore, the regression model should be run as a multilevel model thereby accounting for the potential design effect introduced by the cluster sampling.

Response: We did not account for clustering in our analyses, as the aim of this work was testing of the study models as such - i.e. accounting for clustering was outside the scope of this study. However, as a comment to the clustering issue, I can mention that the participating schools were all located within the same, quite small geographic area, which according to Statistics Norway (SSB) is an area with high and still growing labor attachment and household incomes. Moreover, the Norwegian school system is based on the socio-democratic principle of equality. Thus, we did not see clustering of data within schools or classes as an important issue here – at least not when it comes to socio-demographics.

It should be stated that a linear model (regression) is applied. As stated above, potential
Response: The manuscript is revised to state that a linear regression is applied. However, we still choose to omit confounders in our models.

Discussion

Several important elements are lacking in the discussion. These involve a critical discussion of non-respondents – both at school level and at student level. Almost every third school declined to participate and around half of all students enrolled at the participating schools did not respond. The potential consequences for the results and conclusions drawn on the presented analyses should be discussed. Also a discussion of residual confounding should be included. When including potential confounders available in the dataset a discussion of potential additional but not available confounders should still be included.

Response: Thank you for giving us the opportunity to clarify our views on this matter: Undoubtedly, non-respondents will always be an issue, and the lower the response rate the greater the likelihood of non-response bias. Thus, we do see your concern. However, we consider our response rate pretty good: 72% for schools (the schools that chose not to participate said the reason they did not participate was that it would be too time consuming because they were already participating in other projects at the time – e.g. the Respect Program, which is a Norwegian school based development program intended to prevent and reduce problem behavior such as bullying), 66% for parents and 92% for children with written consent from their parents. We regard the low number of child respondents (n=796) compared to parent respondents (n=963) the most important limitation, as it reduces the number of dyads included in our model analyses. However, there are plausible explanations for the relatively lower number of child respondents: The data collection procedure went through several steps (researchers - school – home - school – researchers), and some of these steps were out of the researchers’ control. Thus, information may have been lost on the way. First, the parents survey package, including a consent form for child participation, was administered to the children at school with instructions to bring them home to be completed by their parents. Some children were not at school the day of distribution, some parents filled in the parent questionnaire, but forgot to or refused to fill in the consent form for child participation, other forms were forgotten at home or got “lost” on the way, and therefore never got back to the school, and to the researchers. However, as already mentioned, the rationale for omitting these issues is that the present study aimed to test the models as such, irrespective of potential confounders, clustering and non-response bias. And, again, there is 1) the principle of equality of the Norwegian school system (there is no such thing as high SES or low SES schools in Norway) and 2) the fact that the area from which participants were invited is a relatively homogenous area with high labor attachment and household incomes.

The validity of the applied measures should generally be reported (in the methods section). One strength of the study that is highlighted in the discussion is the use of two different sources. An assumption for this approach to be a strength is that the applied measures are valid – if students are not able to report frequency of family meals, it is not a strength of the study to collect such
data from the students. Therefore, information on the validity of the applied measure on frequency of family meals collected among students should be reported. I am aware that not many validation studies exist on students’ reports of meal habits, but any experiences gained from the present study – e.g. quantitative and/or qualitative results from pilot studies/the pre-tests – should be reported.

Response: Information on validity is included in the revised manuscript (p. 9, the last sentence of the first paragraph, and p. 10, first paragraph)

Conclusion The purpose of a conclusion is to specifically relate to the research questions posted at the beginning of the manuscript. In the present manuscript, conclusions are only drawn for the first and third research question. Conclusions on the second research question are not included. Please revise.

Response: The conclusion is revised to fit the research questions of the revised manuscript.

Minor essential revisions:

1. In the introduction varying terms are used for family meal – family meal frequency and family mealtime frequency. Please be consistent.

Response: Thank you for making us aware of this. The use of these terms is consistent in the revised manuscript.

2. Page 3/last sentence of the first paragraph: ‘.....mealtime frequency has been associated with increased discussion and knowledge....’ Please be a bit more specific about what is meant by discussion and knowledge. Discussion within the family? At the table? And parent or child knowledge about nutrition?

Response: We have tried to clarify this in the revised manuscript (p. 3, last sentence of the first paragraph).

3. Page 3/second (and last) paragraph: here the term ‘home environment’ is used for the first time. However, here it is not the same concept as ‘home environment’ used to define a specific aspect of feeding practices. This is confusing for the reader. Please revise.

Response: We have revised the manuscript to clarify this.

4. Page 9/second paragraph: the midsection (starting with: These time constrains....) of this paragraph is difficult to follow – please revise.

Response: We have removed this paragraph as suggested by reviewer no. 1.

5. Page 9/second paragraph: the term ‘within-meal carry-over habits’ needs some explanation.

Response: We have removed this paragraph as suggested by reviewer no. 1.

Thank you again for your useful comments!