Author's response to reviews

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

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

Again, we would like to thank you for your comments on our research!

Reviewer #1: Katie Loth

Reviewer's report:
This is my second review of this article and I would recommend publication at this time based on an appropriate level of revisions to the article. This article will be of great interest to your readers and is a pleasure to read.

Response: We are grateful for your appreciation of our work!

Reviewer #2: Mette Rasmussen

Reviewer's report:
General comment:
The manuscript has improved in several aspects – especially the introduction is now more informative. However, the analyses still suffers from a number of severe limitations which should be corrected. If this is not possible (due to data not available) this should be stated and the potential consequences should be critically reflected upon in the discussion.

Major compulsory revisions:

Confounder control:
In response to the comment raised regarding whether to adjust for potential confounders or not, the authors argue that the study is explorative by nature. I do not agree that this is an argument for leaving out adjustment for confounding. It is highly potential that e.g. family structure and socioeconomic position are confounding the associations studied. It is unclear what variables are measured in the study beyond parental feeding practices and family meal frequencies. If it is the case that e.g. data on family structure and socioeconomic position are not available this should simply be stated – and discussed in directions for future research that the association should be studied in datasets with such data available. If data on socioeconomic position and family structure are available I don’t see the purpose of not adjusting the analyses – despite being explorative by nature.

Response: We have data on potential confounders such as parental educational level and household income, and the manuscript is revised as requested: the analytical models now include these potential confounders (see text p. 9, and note below Table 4).

Additional information: Re-running the multiple regression models and including the potential confounders, gave results similar to the results in our
original (unadjusted) models. However, our re-analyses led to the discovery of a significant association between the home environment variable and family dinner frequency (model 2) that was missed in the previous analyses. However, the “story” is still the same; parent-reported feeding practices, including parental provision of a healthy home environment, are associated with child-reported frequencies of family meals.

In the section on statistical methods it is stated that multiple regression models are conducted. What is meant by ‘multiple’? – normally it implies that the model includes other variables than the dependent and independent variable in focus, e.g. confounders. Here, does it mean that all independent variables (parental feeding practices) are included simultaneously? Please elaborate. This information should also be available in the title (or notes) for table 4.

Response: Yes, multiple regression here means that all independent variables (including potential confounders) are included simultaneously for each of the 3 study models. We have revised the title for Table 4 as requested.

Evaluation of non-response:
A sound scientific study always reflects on the potential existence and consequences of non-respondents. Such reflections should be included in the discussion of limitations. The authors in their response to my comment on non-response give a number of arguments why the non-response may not have caused a selection bias. This argumentation should be included (in a revised form) in the manuscript. However, I do not agree that there is no risk of selection bias. The authors mention that some students were not at school at the time of data collection and some parents did not fill in the parent questionnaire. Potentially, these children and parents may represent families with certain characteristics that relate to the dependent and/or independent variables studied in the analyses. This cannot be ignored and should be discussed in the limitations.

Response: We see your point, and have included a discussion on these issues in the revised manuscript (see p. 16-17).

Minor essential revisions:

External validity
The authors generally argue that Norway is a very homogeneous country. However it is difficult to imagine that no compositional variations exist across schools in enrolled children and families – e.g. in terms of social background and eating patterns. I therefore suggest that the authors are more cautious when they conclude that the identified associations may be generalized to Norway generally.

Response: We have revised the manuscript according to your concerns (see p. 17).

Thank you!