Author's response to reviews

Title: A prospective study of weight development and behavior problems in toddlers: the Norwegian Mother and Child Cohort Study

Authors:

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Author's response to reviews: see over
Dear Dr Hewitt and Reviewers,

Thank you for dedicating so much attention and time to our paper. We appreciated your suggestions and tried to incorporate them in the revision.

**Referee 1’s comment:** I was a little concerned about the method by which height and weight were obtained. To what extent was this information collected in a standard fashion? Of concern is the possibility that there may be variation or poor inter-rater reliability across the various health check stations where this was collected. I was also concerned about the possibility that mothers may have made errors when copying information from charts into questionnaires. Some comment about these issues would be useful.

*Our reply:* We agree with the concern that questionnaire data on anthropometric measures are not optimal. Therefore, we added this issue as another limitation in the discussion section (page 14, lines 320 - 333). Nonetheless, as we discuss in this section, we believe we have good reasons to assume that these data is fairly reliable.

**Referee 1’s comment:** The assessment of emotional and behavioural problems is quite limited. ... The limitations of the assessment in this area, is a significant weakness for the study.

*Our reply:* We have further examined the psychometric properties of the shortened version of the CBCL. Internal consistency proved to be satisfactory for all but one subscale (see page 8, lines 182 - 186). What is more, the majority of the factor loadings were high; only a few had moderate factor loadings (page 8, lines 187 - 190). Also, demonstrating their discriminant validity, we showed in previous publications that the Internalizing and Externalizing scales correlated with other variables such as maternal smoking and child diet in a meaningful way. We also added a small section on this issue in the discussion (page 13, lines 301 – 308).

**Referee 1’s comment:** I was unclear about the meaning of the sentence at the bottom of the first paragraph on page 11, “one can speculate that the scarcity of publications on children before the age of 3 may be due to publication bias”. What is meant by this and what is the evidence to support the comment?

*Our reply:* We realize we do not have empirical evidence for this comment and have therefore deleted it.

**Referee 2’s comment:** a) This study begins with a very serious conceptual problem: namely, the argument that there is reason to believe that early behavior problems may contribute to weight gain during the first three years of life. The arguments invoked simply do not apply to children below age 6. Children lack the cognitive capacity and social understanding necessary for the presumed mechanisms to function. The authors speculate that the reason they may not have found evidence in support of their hypothesis may pertain to “the young age of our sample.” This speculation is accurate. More critically, the reason that there is a scarcity of publications on the association between obesity and psychological problems for children below age three has little (if anything) to do with “publication bias”, as they surmise, but because there is just no reason to suspect that such an association exists. The failure of this study to produce one is precisely what one would expect. A close reading of the literature provides no convincing support for such an association prior to age six.

*Our reply:* For several reasons we do not agree that the relation between behavior and weight should not be examined because there would be no reason to believe that there is such a link:
1. The literature we cited has produced mixed results, i.e., some studies found positive associations between behavior and weight. These studies were mostly small and clinical. From an epistemological point of view, research should not be self-limiting nor avoid examining what we believe cannot be true. Many major discoveries base on chance results from studies where the aims were unrelated to the discovery. Moreover, precisely because we have the possibility to examine the temperament/obesity link in this large epidemiological data set, we feel an obligation to do so.

2. Mechanisms that could explain the behavior-weight link do exist, and they begin to operate early. We previously showed that already at the age of 6 months temperament is linked to being breastfed (Niegel, 2007). With increasing age, children develop distinctive food preferences and parents in modern societies are inclined to feed children their preferred foods. Interestingly, in a new study (Vollrath, 2010) (manuscript submitted for publication) we found that already at age 18 months, children with certain temperament traits have distinct preferences for obesogenic foods, such as sweet foods and sweet drinks. Moreover, the preference for sweet foods and sweet drinks is related to exactly the same temperament/behavior traits investigated here, namely, externalizing and internalizing behavior. There are theoretical arguments, and there is emerging evidence that the link between temperament and overweight may be explained by a shared biological factor. With regard to a mechanism relating overweight to distress in the child, we propose that maternal control practices aimed at restricting the child’s food intake could play a role. However, we admit that we could have made a stronger case for the existence of these mechanisms in the introduction, and we have amended this in the manuscript.

References

Vollrath ME, Tonstad S, Hampson SE. Infant temperament is associated with a potential obesogenic diet at 18 months. Manuscript submitted for publication 2010.


b) Most unfortunately, these authors appear not to have been aware of a very large longitudinal study that traced the association between internalizing (and externalizing) problems from age 2 to early adolescence (one with much better anthropometric and psychological measures than this study) which showed that a causal connection does not emerge until after age 8. Very simply, there is no reason to examine the association between adjustment problems and obesity in early childhood as the mechanisms connecting them are not functional until essentially middle childhood.

Our reply: We apologize for having overlooked the important longitudinal study conducted by Bradley et al. Naturally, in the revised version of our paper we now mention this study and its results (page 5, lines 105 – 106 and 110 - 113). Consequently, we have also corrected the claim that our study was the first to investigate this issue (pages 12 and 14). In addition, we toned down our expectations to find positive associations between behavior problems and weight (pages 6, 12 and 15).

Referee 2’s comment: a) The measures of behavioral maladjustment used in the study are inadequate for purposes of this study – the authors recognize this possibility. Most critically, the measures are too brief. ... Critically, no evidence is presented verifying that the brief scales were valid (selecting items from the CBCL is not sufficient in this regard).
Our reply: As mentioned above, we acknowledge that weakness in our study. However, we believe we have demonstrated the satisfactory psychometric properties of the short version of the CBCL.

b) Moreover, the stability estimates are only moderate in size ... there is concern that they may not be capturing the same true underlying dimensions at the two time points.

Our reply: Given the small number of items, we deem the stability estimates in our study as actually quite acceptable. Compared with Bradley’s study, where the full CBCL was administered, the discrepancy of the stability estimates between the two studies is in our opinion not too big. Moreover, on page 9, lines 203 - 205 we have shown that the subscale factors were equivalent at child ages 18 and 36 months (test of measurement invariance).

c) There is particular worry for children age 18 months. This is the lower bound extension for the measure; and there remain real questions about whether one can meaningfully capture externalizing behavior in children so young.

Our reply: We refer to the fact that the CBCL is validated and commonly used for children age 18 months.

Referee 2's comment: A smaller concern is the adequacy of the anthropometric measures. ... The fact that so many cases had to be omitted due to worries about these measures calls into question the overall accuracy for a study such as this.

Our reply: As mentioned further above, we have added a section on this topic on page 14, lines 320 – 333.

Referee 2's comment: Address the issue of why somatic problems might lead to low weight.

Our reply: We have now added another sentence on that issue on page 12, lines 280 - 282.

Referee 3’s comment: a) The literature review would benefit from some type of conceptual framework or model whereby the prospective association of overweight and subsequent (or precursor) behavioral problems could be outlined to accompany text. Though you make this reference later in the conclusions, I would put more discussion of the limitations of these hypotheses in younger samples. Further, what is the role of parenting/family eating behaviors and attitudes? That would seem to make more a difference for the younger set who are reliant on moms more. Thus the hypotheses framework may have holes upon which your later null findings can be explained.

Our reply: We have now extended our literature review and conceptual framework considerably (page 4, lines 73 - 94).

b) Also, what is the rate of overweight among this age group. Is the argument that based on lifespan trajectories or is there a concern about infant overweight? If so, state that and provide numbers.

Our reply: On page 3, lines 51 - 63 we have now added some information about the prevalence of overweight in children and this age group in particular. What is more, we have elaborated on the negative health consequences that mainly pertain to long-term health consequences.
**Referee 3’s comment:** In the methods explanation, I may have missed it but what family or maternal demographics and BMI characteristics were included in models? I see maternal BMI was collected at each time point, but what other variables beyond educations? Could it not be that there other factors that explain an association between infant BMI and behavioral problems? More explanation of inclusion or reasons for exclusion are useful here.

**Our reply:** As we did not find any noteworthy associations in the first place, we did not see much reason for controlling for further background variables, as these are usually included in order to make sure that the observed association is not merely due to third variables (confounders). We have, however, inserted the word “relevant” on page 9, line 209 to underline that the most relevant control variables are included.

**Referee 3’s comment:** How accurate is mother copied information on weight and height from charts? Any concerns there?

**Our reply:** As noted earlier, a new section has been added addressing this issue (page 14, lines 320 - 333). As stated in this section, we believe this information to be fairly accurate.

**Referee 3’s comment:** What was the correlation between mother’s BMI and child’s BMI?

**Our reply:** In fact, the correlation between mother’s and child’s BMI was rather low. We have now added the exact numbers on page 10, lines 221 - 222.

**Referee 3’s comment:** In the discussion, more consideration about other outcomes (not measured) that could relate to infant or young child BMI is useful. I am not convinced that these symptoms would emerge so young primarily when eating behavior is mother/family defined. Please expand more on this beyond simply stating the lack of hypothesis fit.

**Our reply:** We have elaborated more on this in the discussion, putting more emphasis on the family context (page 12, lines 270 – 274, lines 285 – 290 and pages 12/13 lines 294 - 298). At the same time, we believe that in today’s small families, children’s food preferences are very much taken into account by their mothers.

As a result of the changes due to the reviewers’ comments we have added a number of new references. Like the other changes, they are highlighted in the manuscript. In addition, on page 6, lines 140 - 143 we have now inserted a statement on the ethical approval and written informed consent we obtained for this study. As suggested by two reviewers, we also had a professional proofread our manuscript for language and style corrections.

We are certain that the paper was substantially improved thanks to the reviewers’ efforts and hope that this revised version is satisfactory.

Yours truly,

Susan Garthus-Niegel