Author's response to reviews

Title: The development of socio-economic health differences in childhood: results of the Dutch longitudinal PIAMA birth cohort.

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Author's response to reviews: see over
Cover letter on the second revision of the manuscript ‘The development of socio-economic health differences in childhood: results of the Dutch longitudinal PIAMA birth cohort’.

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Dear Editor,

I hereby send you our second response to the comments of the reviewer. We have addressed the comments point by point, and highlighted the alterations in the manuscript (we did not highlight all the textual alterations made by the copyediting service, but focused on alterations concerning the content). Below are the comments of the reviewer (italic) followed by our response to each comment.

With kind regards,

Annemarie Ruijsbroek

**Reviewer 2: Henrik Ohlsson**

*First of all I suggested that the manuscript should be proofread. The authors replied that they had done this. However, I still found the paper overall difficult to read due to poor word choice and confusing sentence structure, particularly in the new parts that have been included. I do not think that the quality of written English is acceptable for publication.*

As advised by the associate editor, we sent the manuscript to a professional copyediting service this time. We are confident this has improved the manuscript’s readability, and hope the reviewer and associate editor are satisfied with the result.

*Moreover, I wrote that the table headings and the figure legends needed much more information. However, this has also been very hastily done – there are no figure legends, there are two figure 1, the table headings are still limited and difficult to understand. For example the heading for table 5 reads “prevalence of potential predictors of worse health in childhood by educational level of the mother” – but among the figures in the table we found the value 3,456 and 3,507 – are those figures percentages or?*

*Furthermore there are several small disturbing mistakes in the manuscript. For example: the first time the authors use the term odds ratio, they only write the abbreviation OR, without any explanation, but the second and third time they write odds ratio. Additionally, the authors are not consistent on how they start a new paragraph. These aspects show that the proofreading has not been done carefully and thereby the accessibility of the manuscript is limited.*

We reread and edited the manuscript thoroughly in order to correct the mistakes in the text and to complete the table headings. Unfortunately, we did not do this carefully enough the first time. We could not trace the comment that there are two figures labelled Figure 1. All other points have been taken into account and the manuscript has been adjusted accordingly.
I also pointed out that the phrasing statistically significant is obsolete and often misinterpreted and I suggested that the authors should focus more on the question of the size of the estimate and the range of the 95% confidence interval (CI). The authors have done so, but they have also included a new table (table 4) without any 95% CIs and a new interpretation based on statistical significance.

We thank the reviewer for his advice to focus more on the size of the estimate and range of the 95% confidence intervals (CIs) and not only on the statistical significance of the association. As a result of this advice, we adjusted our paper accordingly in the first revision. Furthermore, we agree with the reviewer that it is inconsistent to show the p-values in Table 4 rather than the CIs. We added CIs to Table 4 and removed the p-values, as advised by the reviewer.

Concerning the interpretation based on statistical significance, we agree that the effect size of an association is very important for interpreting the (clinical) relevance of a determinant for a health outcome. In this case it would not have been enough to look only at statistical significance. However, in our case we used statistical significance as a criterion for model selection. If a determinant in Table 4 was statistically significant with regard to the health outcome, we selected this variable for our next model, and otherwise we did not. We discussed this method with our statistician, and based on his advice we chose this criterion of statistical significance for selecting the determinants for our model. We tried to be precise when making the selection. For instance, for the determinant ‘Smoking in the home’ (which is borderline significantly associated with asthma symptoms, poor general health, and frequent respiratory infections) (Table 4), we investigated the effect of this determinant on the relationship between maternal educational level and the previously mentioned health outcomes. The explanatory effect of this determinant was indeed marginal, and therefore we did not select this determinant for further analyses of these three health outcomes in Table 6.

The authors also included a new part of their aim – “For those determinants significantly related to one or more health problems, we studied the distribution of categories of the predictor by educational level of the mother” – This aim is met by studying the prevalence/mean value for each predictor in each category of educational level. This is not enough. There are no uncertainty intervals – why have not the authors included this aspect in a model (interaction terms)?

We agree with the reviewer, and have included uncertainty intervals (Table 5). In our manuscript, we investigate the relationship between the educational level of the mother and the health of the child, and we try to identify intermediate variables in this relationship. To do so, we examined whether the determinants are related to the health of the child (Table 4), and next, whether the determinants are related to the educational level of the mother. This is presented in Table 5. If this was the case, the determinants were selected for the final model (Table 6). Our research question focuses on finding these intermediate variables, not on finding effect modifiers. If this had been the case, we should have included interaction terms. As mentioned above, we agree with the reviewer that uncertainty intervals should be included in Table 5.

In table 6 only ‘low education’ is included for some of the health outcomes, why? The At page 8, row 8 the authors state that “Table 6 shows the contribution of predictors of health outcome related to maternal education level, to the explanation of the relationship between educational level of the mother and childhood health”. However, the table shows the odds ratios for low education when several predictors are included in the model. Even though
The authors have cut ‘percentage explained’ by different predictors in the table, they still interpret this figure in the text. This is very confusing. As the manuscript is devoted to a large part to mechanisms explaining differences in health it is for me strange that the authors have not tried a more sophisticated analysis.

In our initial manuscript, we included in Table 6 only the educational levels that showed a statistically significant increase in odds ratio (OR) for the health outcome in Table 3. However, in the revised manuscript we followed the reviewer’s advice to consider the size of the estimate and the range of the 95% CIs instead of just the statistical significance. As a result, we came to different conclusions and a different description for Table 3. We should have done the same for Table 6, because this table is based on Table 3. Therefore, in this revision we adjusted Table 6, and also included the intermediate educational level for asthma symptoms, poor general health, and overweight.

Concerning the second part of this comment (about page 8, line 8), we rearranged the text related to Table 6 and now start by describing the reductions in ORs when all predictors are included in the model. Next, we mention the predictors that contribute most to the relationship between maternal educational level and children’s health (when included one by one), and we give a straightforward description of the effect of the predictor on the OR. For instance, ‘the OR reduces from OR 1.17 to OR 1.12 when breastfeeding is included’. We hope this satisfies the reviewer.