Reviewer's report

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

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Reviewer: Henrik Ohlsson

Reviewer's report:

The authors have answered to the previous comments I made but there are still aspects in the manuscript that needs to be considered. All aspects below are Major Compulsory Revisions.

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.

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 aspect shows that the proofreading has not been done carefully and thereby the accessibility of the manuscript is limited.

I also pointed out that the phrasing statistically significant is obsolete and often misinterpreted and I suggested that the authors should focus more on the question of the size of the estimate and the range of the 95 % confidence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.

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)?

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.

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

**Quality of written English:** Not suitable for publication unless extensively edited

**Statistical review:** Yes, and I have assessed the statistics in my report.

**Declaration of competing interests:**

I declare that I have no competing interests