Author's response to reviews

Title: Parental income is more important than parental education to children's health and wellbeing in adulthood: a cross-sectional study from Norway

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Author's response to reviews: see over
Dear Editor-in-Chief,

Please find enclosed our revised manuscript, “Parental income is more important than parental education to children’s health and wellbeing in adulthood: a cross-sectional study from Norway” by Sheikh, M.A., Abelsen, B., Olsen, J.A..

We are grateful to the two reviewers for their detailed comments and suggestions for improvements of the manuscript. Below you will find our response (referred with line numbers in the manuscript) to the reviews. The text from the reviewers is in blue.

Reviewer 1:

We agree that the subjective measures of health can be an unreliable proxy for disease outcomes, and of physical health. Therefore, we have used four different measures of subjective health and wellbeing with the aim of assessing sensitivity of the estimates. This aim has been specified in the introduction now (line 104-107).

Note that no quantitative effect estimates are provided in the abstract.

The quantitative estimates have now been provided in the revised abstract.

Line 163 indicates that the authors’ aim is to estimate direct and indirect effects, but does not clarify if these are the “controlled” variety or the “natural” variety. These have different identification requirements and different causal interpretations, and so the distinction is important to clarify. Later in the paper we find that these are indeed the natural variety, but one should say so straight away so that the reader does not need to wonder.

We have identified this in the manuscript. In the earlier manuscript, we reported only natural direct effects. However, we agree with Jay Kaufman (commented later in review) that the CDE should be reported. Therefore, CDE’s are reported in addition to NDE’s and NIE’s in the new tables (line 794-800). Other sections of the manuscript are updated accordingly.

Line 167 refers to the DAG in Figure 1, but this DAG shows no unmeasured confounders, and only one measured confounder of CSES and outcome (i.e. age). This seems somewhat unrealistic. There are also no confounders shown of CSES and education, such as parental SES, neighborhood, etc. I think that in terms of the subject matter, the DAG does not seem to be a causal DAG, in the sense that there are very many potential confounders (measured and unmeasured) that are not depicted.
We agree, and have omitted the term ‘causal’ describing DAGS.

In lines 170-180, the authors say that they fit a B&K model and tested for interactions, but they did not reveal the form of this model (e.g. linear, logistic, log-linear, etc). It is impossible to interpret the statements about interactions without knowing the scale of the model, since an interaction term is just a departure from the model form.

We have now mentioned the form of the models (line 195-196).

In line 186, the authors assert that the –paramed- command in Stata can estimate natural effects. But this is only true if these effects are identified in the DAG. This requires an absence of exposure-mediator confounding, and the absence of mediator-outcome confounders affected by exposure. Since childhood SES potentially affects many adult traits such as health, this seems unlikely. The authors should defend the claim that these effects are identified.

We agree with the limitation in reporting the NDE’s. We have now mentioned this limitation as a potential weakness of the study in the discussion section (line 437-461). In addition, we are now reporting the controlled direct effects (CDE) as well. See tables (line 794-800).

Lines 206-208: Why are the equations shown as approximate? The authors are estimating the RR directly, not approximating this by estimating an OR. So why is the equal sign not used?

We agree. It has been corrected (line 230-234).

Lines 210-214: The natural effects refer to setting the mediator to the value it would have taken under a situation that never occurred. This makes it impossible to have any public health implications, since there can never be any policy of recommending to people or assigning to people a value that is not observed. Why wouldn’t these authors prefer controlled direct effects?

We agree with Jay Kaufman that CDE’s should be reported. We have now included the CDE’s in a new table (see table 5). The reason we still report NDE’s and NIE’s is because we aim to report if there is any mediation through own education. The CDE’s alone cannot provide this answer.

Lines 254-256: “When childhood financial conditions was used as an exposure, fathers’ education, mothers’ education and spouse’s education were included in the model as potential mediator outcome confounders.” Why do the authors believe that spouse’s education is not affected by childhood financial conditions? Doesn’t a wealthy upbringing and ample parental resources predict having the resources and networks to attract a more wealthy and successful spouse?

We have now assessed whether spouse’s education can be a mediator-outcome confounder effected by the exposure, see lines 270-277.

Lines 281-287: I am a bit bothered by all of these declarations that some components of the total effect are “statistically significant”. There are no p-values shown in the Tables.
Shouldn’t the authors better report and discuss confidence intervals?

We agree, and we have now reported and discussed CI’s instead

Results section: The authors describe several models that failed to converge, but they do not explain how they worked to obtain convergence of these models. There are many techniques for doing so, including the use of modified Poisson regression (Zou G. Am J Epidemiol. 2004 Apr 1;159(7):702-6) and the specification of starting values (Spiegelman & Hertzmark. Am J Epidemiol. 2005 Aug 1;162(3):199-200). The authors should not simply abandon these models with no estimates reported. That would be silly.

We agree that reporting estimates is better than not reporting estimates. The limitation is that the software -paramed- does not allow incorporating other techniques (like Zou G. et al., and Spiegelman et. el ) for obtaining convergence of the models.

Earlier we used age as linear (one year increment) to adjust in the models, which caused the problem of non-convergence in some cases. We have now adjusted for age (5y groups) which solved the problem.

Lines 354-355: “This suggests that mothers’ education may have a more important role than fathers’ education in the later health of children.” Such an assertion requires some kind of formal test of heterogeneity, but the authors provide no quantitative evidence that these numbers are truly different.

We agree. We have omitted this sentence now.

Citation 36: Better to cite the published version of this in the Stata Journal.

It is corrected.

Citation 39: Careful with spelling of Hernán and Hernández-Díaz (i.e. accents).

We have fixed it now.

Reviewer 2

1. A stronger case could be made for the value of using four outcomes that measure similar dimensions of subjective health and wellbeing. Would we expect to see different results from each outcome?

We agree, and we have now provided the motivation in lines 104-107

2. Further details should be provided on how the sample of 19762 subjects were selected.

We have included it in lines 121-126.

3. The dichotomisation of the composite EQ-5D variable appears to include a large proportion of respondents in the “unhealthy” category, because few respondents could have had “no problems” on all five dimensions. What is the
justification for this cut off, rather than one where unhealthy is level 2 or 3 on, say, three or more of the domains?.

With our current dichotomization, we can say that the ‘healthy’ group really represents the healthy respondents, as they report no problems on all dimensions of EQ-5D. Furthermore, with 44.6% classified as healthy, we come close to a 50/50 dichotomization. We tried making other cut offs, as Catherine Chittleborough suggested, but we could not achieve closer to 50/50 dichotomization. Moreover, in all other attempts with dichotomization, the healthy group also became ‘relatively healthy’.

4. There are up to 11% missing data (on father’s education). Are there differences between people who provided data on these questions, and people who didn’t? Previous research has shown that respondents with missing data on parental socioeconomic position were more likely to be disadvantaged (Chittleborough et al. Missing data on retrospective recall of early-life socio-economic position in surveillance systems: An additional disadvantage? Public Health 2008; 122: 1152-66). How likely is it that these missing data bias the results? It would be useful to have some comparison between those who did and did not answer these questions, or for the authors to have used some strategy to deal with the missing data, such as multiple imputation.

The number (%) of missing in the exposures, mediator and outcomes are as follows:

<table>
<thead>
<tr>
<th>Exposure</th>
<th>Missing (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>Childhood financial conditions</td>
<td>957 (7.3)</td>
</tr>
<tr>
<td>Mothers’ education</td>
<td>1255 (9.6)</td>
</tr>
<tr>
<td>Fathers’ education</td>
<td>1404 (10.8)</td>
</tr>
<tr>
<td>Respondents’ education</td>
<td>186 (1.4)</td>
</tr>
<tr>
<td>EQ-5D</td>
<td>1371 (10.5)</td>
</tr>
<tr>
<td>Mobility</td>
<td>950 (7.3)</td>
</tr>
<tr>
<td>Self-care</td>
<td>949 (7.3)</td>
</tr>
<tr>
<td>Usual activities</td>
<td>933 (7.1)</td>
</tr>
<tr>
<td>Pain/discomfort</td>
<td>1057 (8.1)</td>
</tr>
<tr>
<td>Anxiety/depression</td>
<td>1093 (8.4)</td>
</tr>
<tr>
<td>Self-rated health</td>
<td>121 (0.9)</td>
</tr>
<tr>
<td>Age-comparative self-rated health</td>
<td>375 (2.8)</td>
</tr>
<tr>
<td>Subjective wellbeing</td>
<td>2224 (17.1)</td>
</tr>
</tbody>
</table>

We investigated whether no response (missing) on the CSES indicators is related to the health and wellbeing indicators: A larger proportion of those who do not provide a response on CSES indicators, have low education, and are relatively unhealthy (particularly in case of parental education). We also assessed whether no response (missing) on the health and wellbeing indicators was related with CSES indicators. The pattern is the same: A larger proportion of those who do not respond to health and wellbeing questions have low CSES. This may indicate that those who do not complete the questionnaire are likely to be the most disadvantaged. Missing data clearly biases our results. However, it is likely that if we had the data on all respondents, the estimates (NDE’s/CDE’s) will show an even larger effect of childhood financial conditions on health and wellbeing, in the same direction as shown. Similarly, it seems plausible that we would observe a clear association between having low parental education and being unhealthy/low wellbeing, if we had the data on all respondents.
Since the missing data is not at random, it is likely that imputation will introduce more uncertainty, and bias in our results. Therefore, we have chosen to analyze the collected data only. We have explained this now in the paper (line 484-501).

Performing multiple imputation may not do justice with the complexity of the issue, and a separate paper is perhaps a better option to explore the problem with missing values in our data set.

5. It is not very useful to talk about associations being “statistically significant” on the basis of an arbitrary p-value cut off of 0.05. See the following reference for further information: Sterne & Davey Smith. Sifting the evidence – what’s wrong with significance tests? BMJ 2001; 322:226-31.

We agree, and have omitted the assertions about statistically significance in the manuscript.

6. In the Discussion the authors state that they cannot think of any specific reason why respondents’ recall of parental education should be subject to recall bias. However, previous research (see Chittleborough reference in point 7 above) demonstrates how socioeconomically disadvantaged people are less likely to respond to questions about CSES. Potential reasons for this are discussed in that paper.

We agree. The text has been corrected (see line 463-474).

7. What are the potential mechanisms for the direct effect of child financial conditions on adult health and wellbeing that don’t occur with parental education? (second paragraph of the Discussion)

We have given the potential explanation in line 399-407.

8. It seems a little unsatisfactory to leave the analysis at the stage of “Some estimates could not be calculated due to non-convergence”. This is not my area of expertise, but statistical advice may be worthwhile here.

We agree. Earlier we used age as linear (one year increment) to adjust in the models, which caused the problem of non-convergence in some cases. We have now adjusted for age (5y groups) and that has solved the problem.

9. Some potential explanation should be provided for the finding that the NDE effect of father’s education is in the opposite direction to the NIE.

We have mentioned this in line 416-436.

Minor Essential Revisions

10. In the second paragraph of the Results there is a sentence that I can’t quite make sense of: “In contrast to the large difference in self-rated health, there was only a small absolute difference in age comparative self-rated health”. Can a little more explanation be provided here? It is nice to see both relative and absolute
differences presented.

We have omitted that sentence now.

11. The third paragraph of the Discussion is a re-statement of the results. This could be expanded to interpret why child financial conditions are linked to different outcomes for men and women.

We have now omitted this paragraph since it is a re-statement of the results. We have realized after reading Jay Kaufman’s review that any assertion about the difference between the estimates from men and women is not possible without any sort of statistical test which shows that these differences are truly different. The magnitude of the estimate is different, but their CI’s may overlap. Unfortunately, we cannot think of a statistical software, which computes both the mediation models separately for men and women, as well as provide a test of heterogeneity between them. In lack of it, we think it is better not to highlight and discuss these differences.

Discretionary Revisions

12. In the second paragraph of the Background, the authors indicate that this study adds to the existing literature because while previous studies have used economic background, this study will use indicators of economic and social background. I think that parents’ education is used as a “social” indicator here. The rest of the paragraph goes on to explain how previous studies have examined the effect of parents’ education on later health and wellbeing. A stronger case could be made for the novelty of this study.

We agree. We have now made changes in the introduction accordingly.

13. The Krieger paper used as a reference related to the accurate recall of CSES in the “Indicators of CSES” section only examined recall of social class and father’s education. Are there other studies that have examined accuracy of recall of child financial conditions?

We did not find any study where the reliability of childhood financial conditions was studied. We have now pointed this out in the paper (line 463-474).

We look forward to hearing from you at your earliest convenience.

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