Author’s response to reviews

Title: Parental academic involvement in adolescence as predictor of mental health trajectories over the life course: A prospective population-based cohort study

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Author’s response to reviews: see over
BMC Public Health  
Prof. Natalie Pafitis, Exec. Editor

Prof. Natalie Pafitis,

We thank you for the opportunity to resubmit this manuscript and apologise for the revision taking more time than expected. We would also like to express our thanks to the reviewers. We have re-analysed the data and amended the manuscripts according to the comments, and believe that this has improved the manuscript substantially. The pattern of results is the same, but inclusion of more potential confounders strengthen the argument for a real effect of parental involvement on the development of mental health.

Unfortunately, the submission system indicates that I have not chosen journal subsection, but looking further down the page, Environmental and occupational health is indicated as the subsection, something which does not seem to be possible to change (nor do I wish to).

Yours faithfully, on behalf of all the authors,

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Response to reviewers

Reviewer 1: Anu Molarius

Reviewer's report:
This is a very interesting study about parental academic involvement in adolescence and mental health trajectories over the life course using modern analysis methods (latent class trajectory analysis).

Major Compulsory Revisions

Response 1:1. Thank you for this assessment.

It is not clear from the background exactly how this study differs from the previous study of (partly) the same authors where parental academic involvement in relation to allostatic load was studied. A more specific description should be provided to justify the present study.

Response 1:2. The most important difference is that the previous paper focuses on biological wear and tear rather than on mental health, but we believe that the reviewer’s question is more about the methodology. The major difference there is that allostatic load, which is based on biological measures, could only be assessed at the latest wave of data collection, in mid adulthood, whereas we in the present paper were able to study trajectories of mental health from adolescence, through early adulthood and up to mid adulthood, giving much better opportunities to describe and understand the actual development across a significant part of the life course. We have now tried to make the distinction between the earlier and present studies by highlighting that the difference between allostatic load and mental health as well as the fact that the outcome in the earlier study was measured only in mid adulthood.

Also in the discussion the difference between the results of these two studies should be clearly stated.

Response 1:3 Thank you for pointing this out, we realise that this was mentioned relatively obliquely in the discussion. We have now tried to highlight the difference and provide a possible interpretation.

There are several open questions about the measure of parental academic interest. Parental academic interest was teacher rated here. This does not appear everywhere in the text and, more importantly, not at all in the title or in the abstract of the paper which is misleading.

Response 1:4 Thank you for pointing this out, we have now added ‘teacher-rated’ and ‘student-rated’ at strategic places throughout the manuscript to make this clear to the reader, not the least in the abstract. In order not to make the title too long, however, we did not add this in the title, but keep the generic term ‘parental academic involvement’ which covers both teacher-rated parental interest in their offspring’s studies AND student-rated availability of help with homework assignments.

The measure also appears rather weak and subjective, even the wording is “probably very small” or “probably very large”. In addition, it was measured two years after the pupils left the school (at least so it is said on p. 6) and teachers may have been affected by the social
background of the pupil as well as their grades.

Response 1:5. Thank you for noticing that the year of the teacher interview was stated as two years after the pupils left school, this was a typo, and the correct year should be 1981 and not 1983. It was thus conducted during the last year of compulsory school. We have now corrected this.

However, it is still a valid point of criticism that the teacher ratings could be influenced by knowledge about the parents’ social class and the pupils’ school performance. It is because of this risk that we adjusted for social class in Model 2 in Table 3, and additionally for final school grades in Model 3. In the revised manuscript, Model 2 is additionally adjusted for parental mental health and alcohol problems as well as family unemployment, which could also have influenced teacher ratings. We also clearly acknowledge this problem in the limitations section of the Discussion. This is, however, complicated by the fact that school performance could also be a mediator between parental interest and mental health, rather than a confounder. It is thus not clear whether Model 2 or Model 3 provides the best estimate of the, possibly causal, association between parental interest and mental health, which we now elaborate in the new, final section of the Discussion.

As for the wording of the questions, the teachers obviously cannot know for sure how interested the parents actually are - they can only observe the parents’ behaviour and what people say about this, which we acknowledge in the limitations section. Including the word “probably” in the response options for the teachers was a way to acknowledge this also to the teachers and avoid missing responses because the teachers might otherwise have skipped the question saying that they could not know for sure.

The latter seems probable since there is a high correlation between the two variables and it can also be seen in that most of the results change into non-significant when adjusted for grades. These problems have been shortly mentioned in the discussion but the authors seem to more or less ignore them in their conclusions.

Response 1:7. As we argue in the preceding response, a high correlation does not necessarily mean that grades are a confounder, and in the stratified analyses it is clear that parental interest predicts mental health trajectory also after adjusting for grades among those who had average or above mean grades, and although the confidence intervals are large, the risk of entering the worst trajectory is substantially lower among those participants whose teachers had rated parental interest as high. Moreover, adding grades to the model does not attenuate the estimated association at all in this group. Instead, we believe that the clear difference between those with above and below national mean grades in the association between parental interest and mental health trajectory indicates that parental interest operates differently (or possibly is rated differently) for students with high and low academic achievement, respectively. In the latter group, there seems to be no association between parental interest and mental health trajectory, and this is not due to attenuation caused by adding school grades, since we found no association even in the minimally adjusted Model 1. We have therefore chosen to focus the conclusion on the results found in the average and above national mean group.

In addition, teachers’ ratings can have been affected by the mental health of the pupil at the time of leaving school so reverse causality cannot be ruled out. In a prospective study those who had mental health problems at the baseline would have been
excluded from the study, but this is not the case when using trajectories.

Response 1:8. We agree that this is indeed a problem since mental health problems tend to track across time. Excluding persons with mental health problems at baseline does not completely solve this problem either, since mental health is not an either-or phenomenon, but levels can vary, possibly giving rise to residual confounding also when those with manifest mental health problems at baseline are excluded. When studying latent trajectories, this problem is handled differently as the trajectories are descriptions of the development over time. It is clear that the two trajectories against which teacher-rated parental interest seems to be the most protective are trajectories which start out with high average symptoms. This could indicate that the reviewer’s suspicion might be correct, but it could also be argued that it is likely that teachers have relatively much contact with parents of children who exhibit mental health problems, and should therefore be relatively well equipped to gauge their parents’ interest. We have now amended the limitations section to more clearly acknowledge this problem.

The conclusion that parental involvement may buffer against poor mental health in adolescence and adulthood and the title are not completely supported by the results, since this was only found among those with grades above the national mean, between a few of the studied categories and that there may be alternative explanations to these findings (see above). “Predicts” is also a too strong statement in this case since a prospective analysis excluding those with mental health problems at baseline was not performed.

Response 1:9. We have now amended the conclusion to make it somewhat more careful by highlighting that the results indicate a possible effect only among those with above average grades. Regarding buffering, we completely agree that the results do not support any definite claim, but think that the combination of the words ‘potentially’, ‘indicates’ and ‘could’ should make it clear to the reader that this suggestion is very tentative only.

As suggested we have also changed the title since we agree that that the old one suggested a more general effect that is not fully supported by the data. However, we do not agree that the word ‘predicts’ carries any specific connotation of prediction of new cases of disease, and believe that it is actually the best word to describe a prospective association.

The finding that teacher-rated parental academic interest was found statistically significantly associated with mental health problems between some of the trajectories only among those with degrees above the mean national level has not been sufficiently explained by the authors. For example, can it be due the fact that adjusting for grades does not affect anymore the results when the variable is already dichotomised into two categories by stratification?

Response 1:10. If the latter were the case, then the results should be doubly adjusted for grades, which would, if anything, strengthen the finding in the above average grade group and raise questions about over-adjustment masking associations in the below average group. However, there is abundant variation left in each group, since the Swedish grade system at the time was constructed to reflect a normal distribution. There is also a clear difference in the effect estimates between the two groups, where the significant results in the above average group have estimates rather far from 1. In contrast, the effect estimates in the below average group are not only non-significant, they are also close to 1.
The result that was somewhat more robust was the finding that help with homework was associated with higher probability of entering the lowest category of internalised mental health symptoms. This should be given more focus in the article.

Response 1:11. It is true that this finding is somewhat more consistent in the whole sample due to the non-existent correlation with school grades, but the finding should still be considered with caution. Firstly, the effect seems to be to increase the chances of following the very low, as compared to low, trajectory of mental health symptoms. It is not clear what this indicates – particularly good mental health or a response pattern where minor symptoms are denied. Secondly, help with homework assignments has shown contradictory effects in the literature, which detracts from the credibility of this finding. We would therefore hesitate to emphasise it more.

There may also be other possible confounding factors than parents social class that were not taken into account in the analyses (for example, unemployment in the family or mental health of the parents).

Response 1:12. Thank you for these suggestions. We agree that those factors could be confounders, although they could also be underlying factors whose potential effect on mental health are mediated by parental academic involvement. Since these factors were available, we included both these factors in Model 2 throughout Table 3. This did in fact result in some attenuation of the effect estimates and widening of the confidence intervals, although the pattern of significant results remained exactly the same. Despite the risk of over-adjustment discussed above, we decided that the addition of these factors would strengthen the paper by giving more conservative estimates.

Minor Essential Revisions
The description of the latent class trajectory analysis includes one whole page of text and two appendices. This seems unnecessary extensive and it should be possible to provide the main message of the appendices in the text.

Response 1:13. We have shortened the description of the LCGA in the Methods and Results sections. The description of the final result of the trajectory analysis as well as main message of the appendices is provided in the text. We would, however, like to keep the appendices as e.g. an online resource to make it possible for readers specifically interested in the LCGA to assess the details of this analysis. As trajectory methods are sensitive to assumptions and subjective decisions made while searching for the optimal model, it is important to make the details available to others.

Page 4, increase in mental health problems among girls is mentioned but the reference covers only Sweden. Is the prevalence increasing in other countries as well? In that case, a relevant reference should be provided.

Response 1:14. We have now amended this sentence and added relevant references as suggested.

Page 4, how is academic achievement associated with mental health? Positively or negatively? For discussion, it might be good to comment the finding (p. 10) that for girls academic achievement seems to be detrimental for mental health
since they have better grades but more mental health symptoms than boys.

Response 1:15. We agree that this needs to be clarified, which we have now done on page 11 as suggested. It is correct that girls have higher grades and worse mental health, but we do not think that grades are an explanatory variable in this relationship. Within both sexes, higher grades are associated with better, not worse, mental health, as can be seen in Table 2.

Page 5-6, it is not altogether clear why the analysis was stratified by academic grading? A better justification is needed.

Response 1:16. The earlier findings that help with homework assignment does not seem to have the same positive effect as academic socialisation could indicate that the effects of parental involvement may be substantially different if it is focussed on compensating for poor school performance (e.g. extra help with homework which might be stressful if it highlights the academic failure) versus on the encouragement of academic interests (e.g. by possibly boosting self-confidence) in students who are already performing well. We have now tried to clarify this on page 6.

Page 8, line 204, “the best model” for ???

Response 1:17. We have rephrased the sentence and it now reads: We used several criteria to choose the optimal LCGA model for further analysis.

In the analyses (table 3) parental interest and help with homework were treated as linear variables. Were the associations really linear?

Response 1:18. We suppose that the reviewer here refers to a linear relationship between the predictors and the logit functions in the multinomial logistic regressions. An assumption of linearity would then be equal to the assumption that the change in odds is the same for each unit change in the (discrete) predictor variable. Unfortunately, with 5 categories in the outcome variable of the multiple linear regression analyses, and 5 levels each of the two main predictors, the models become too unstable for a proper examination of the linearity assumption, and there is no formal test available.

Page 14, the differences in results between men and women are discussed. These were however not statistically significant after adjusting for grade.

Response 1:19. It is correct that the effects of parental interest in the sex stratified analyses were rendered non-significant by adjustment for grade. However, as we have argued, adjustment for grade could quite likely be over-adjustment since academic achievement is a likely mediator between parental interest and later health outcomes. In addition, some effects of homework assistance were actually significant also after adjustment by grades. Unfortunately, we did not have enough statistical power to formally test the interaction between sex and the two studies exposure, which is why we discuss the sex difference only in very cautious terms, calling the difference ‘apparent’.

Page 14, the authors seem to think that it was an advantage that the parental interest was rated by the teacher. As mentioned above this can also be the opposite. Are there any information available of parents’ or pupils’ ratings that
could be used to compare these ratings?

Response 1:19. We agree that it is certainly impossible for the teachers to actually know how interested the parents are, but we would still argue that the teachers are probably the best possible source of such information. Asking the parents themselves would likely be completely meaningless, both because of the strong stigma attached to not being interested in your children, and because it would be very difficult for parents to compare their own level of interest with something meaningful. Teachers, on the other hand, can compare the behaviour of a relatively large number of parents and make comparisons, and do not have the same motivation to bias their ratings. Asking the students themselves might be a better option, and there are two possible candidate questions, one asking if someone cares about them (not their studies) with parents being one of the options, the other being the question about availability of homework assistance which is reported in the paper (with almost no correlation with teacher-rated parental interest). None of these directly ask about parental interest, however, and it is far from clear if pupils can rate their parents’ interest with any accuracy (as opposed to their actual behaviour). The question about someone caring has the added problem that there was no rating scale, only a tick box in a set of multiple choice answers. Thus we are unfortunately not able to validate the teacher ratings against student or parent ratings, but we would still argue that teachers are probably the least biased, albeit far from perfect, source of information about parental interest.

Level of interest: An article whose findings are important to those with closely related research interests
Quality of written English: Acceptable
Statistical review: No, the manuscript does not need to be seen by a statistician.
Declaration of competing interests:
I declare that I have no competing interests.

Reviewer 2
Reviewer: Ingibjörg Thorisdottir
Reviewer's report:
I do not have any recommendations for improvement of the manuscript.
Level of interest: An article of outstanding merit and interest in its field
Quality of written English: Acceptable
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
I declare that I have no competing interests

Response 2:1. Thank you for this assessment.

Reviewer 3
Reviewer: Hrefna Palsdottir
1. Discretionary Revisions
• It could be clearer to have the abstract more limited or separate it a little from the main text - also have it shorter - only few line on each topic.
Response 3:1. There is a page break between the abstract and the main text in the original, submitted file, and unfortunately we do not know why this was not retained in the version that the reviewer got, but must have something to do with the submission system. Regarding the length, we have followed the journal style.

• Line 80: Is Background better?

Response 3:2. Our mistake, we have now changed to Background according to journal style.

2. Minor Essential Revisions:
• Lines: 120 – 122: These statements need a stronger and better arguments or stronger reference.

Response 3:3. We agree that the fact that teacher rated parental interest attenuated the inverse relationship between IQ and adult mortality does not necessarily mean that parental interest ‘explains’ part of the association – it could rather be a marker of some other underling factor. Jokela’s paper furthermore argues for delinquency as a potential causal link which may be modified by parental interest, and since delinquency is more related to externalised than internalised mental health problems, Jokela’s findings may not be directly applicable to the present paper. Being unable to provide a stronger argument or reference, we have removed this statement altogether.

• You need to have the reference like this: [8,17,25].

Response 3:3. Thank you for pointing this out. Our mistake, we have now corrected this throughout.

3. Major Compulsory Revisions
• Line 131: It would be clearer to describe the “aim of the study” better.

Response 3:4. We have now tried to describe the aims better.

• The research question is missing but you mention “your main hypothesis” (line 132). Is there another one? You should mention that.

Response 3:5. As the secondary hypothesis we counted the assumption that parental involvement could have a different impact depending on the pupil’s academic performance, but we realise that this was not a clearly formulated hypothesis. We have therefore removed the word ‘main’ and reformulated the text about differential impact to make it clearer.

• The Method chapter is unclear to me. In line 140: It could be clearer to call it: Sample instead of Population?

Response 3:6. Thank you, we have now changed the heading to Sample as suggested to make this clearer.

• It is mentioned that this study is a “prospective study” (line: 141) – Can you describe what that is og why you use it? You also mention an interview with teachers the year 1983 (line 147). Did you not interview any teachers again?
Response 3:7. Thank you for pointing this out. The mention of the year 1983 in relation to teacher interviews was a typo, the correct year is 1981, i.e. the baseline year. There was thus no prospective element in the teacher ratings per se, although the new form teachers were interviewed 1983 for those who went on to the optional upper secondary (gymnasium) education. The word prospective refers to the study as a whole, where data on exposures and health in adolescence were collected when the participants were in their teens, with data on later exposures and outcomes measured at several later time points.

Why did you just do it once and not again? Do you think of your study as just a quantitative one, or is it also a qualitative one?

Response 3:8. The Northern Sweden Cohort is primarily a quantitative cohort study, but there are indeed also qualitative elements, where more open interviews were performed with some of the participants at different time points. The present paper, however, is a purely quantitative study, as the parts of the teacher interviews mentioned and used in the paper consisted of the interviewer filling out a form with discrete response options to each set question, in this case about the teacher’s assessment of each pupil’s parents’ interest in their offspring’s studies. The reason that we did not use the later interviews with the teachers in upper secondary school was that such data were available only for those who continued with further education, which is a self-selected group, and that the question about parental interest had not been asked at that occasion (since it was deemed less relevant at age 18).

• You should also have the Procedure chapter clearer. You could mention: Where did the students fill out the survey questionnaires? Did a teacher or somebody else assist the students when filling it out? What did the students do with the questionnaires after they had filled them out? Was it anonymous questionnaire?

Response 3:9. At age 16 and 18, the questionnaires were filled in during school hours, The PI was present on all occasions and could assist the pupils when filling in their answers. After completing the questionnaire it was handled directly to the PI. In order to be able to follow each participant, the questionnaires were not anonymous. After leaving school (i.e. at ages 21, 30 and 43) the participants were invited to reunions with their former classmates in order to get information about the study by the PI and to answer the questionnaires. Those who could not attend these reunions (and those at age 18 who had finished school) received a mailed questionnaire. If data were missing, the participants were contacted by phone for supplementary information.

• In the Measurement chapter or the Measures: You could describe better The latent class growth analysis or the (LCGA) and how you use it and why?

Response 3:10. We have revised the description of the LCGA to make it more accessible for all readers. However, we feel we need to keep part of the more technical description (distributional assumptions, handling of missing data, criteria for choice of the model etc) in order to allow readers more familiar with trajectory methods to evaluate our analyses. As trajectory methods are sensitive to assumptions made, it is important to articulate those.

• Lines: 196-214 are very unclear and confusing to me. This needs a clearer explanation.
Response 3:11. We have revised this part of description.

• In the Results chapter: Line: 257-265. Like I mentioned before, this is a little confusing and needs a better explanation.

Response 3:12. We have revised this paragraph to make it easier to understand. We hope that the text, together with Figure 1 gives a good description of the results of trajectory analysis: the shapes of the five typical developmental trajectories of IMHS identified in the cohort, as well as the proportion of individuals following each of the trajectories (i.e. the prevalence of the different trajectories).

Strengths:
• The present study is time serial study or a prospective study.
• The participants are representative
• The sample is rather big.
Weaknesses:
• The statistical analysis or the measures/models are unclear and confusing to me.

Level of interest: An article of importance in its field
Quality of written English: Acceptable
Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.
Declaration of competing interests:
'I declare that I have no competing interests'

Response 3:13. Thank you for this assessment.