Author's response to reviews

Title: Early childhood risk and resilience factors for behavioural and emotional problems in middle childhood

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Author's response to reviews: see over
RE: MS: 8503266991117168

Please find attached a revised manuscript, now retitled “Early childhood risk and resilience factors for behavioural and emotional problems in middle childhood”, which we are resubmitting for the consideration of BMC Pediatrics. We found the suggestions for improvement provided by the BMC reviewers to be valuable in improving the quality of our manuscript.

Our point-by-point responses to the individual reviewer comments are provided below and revisions in the text can be identified with the track changes tool.

We appreciate having the opportunity to re-submit this manuscript for the prospect of publication in BMC Pediatrics. On behalf of my co-authors, thank you in advance for your time and attention related to the review and consideration of this revised manuscript. We look forward to hearing from you.

Sincerely,

Jason Cabaj, MD, MSc
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Responses to the comments of Reviewer #1

Major Compulsory Revisions

1. The way that internalizing and externalizing scores were analyzed needs more justification. The authors only examined children who scored either high or low for each dimension, leaving children who scored in the middle range on internalizing and externalizing symptoms uninvestigated. Given the demographic characteristics of the current sample (i.e., children from middle- and upper-middle-class families whose mothers received routine prenatal care voluntarily and agreed to participate in several follow-ups), leaving children who scored in the middle range unexamined would decrease the power of this investigation and limit its implication. A more acceptable way of identifying risk and resilience factors might be analyzing internalizing and externalizing problems as continuous variables (as they are) and investigating potential resilience or risk factors (e.g., self-esteem, social competence) as moderators. In this way, less information would be missed. Similarly, many continuous study variables were arbitrarily dichotomized in this study, which again may lead the authors to lose important information regarding the moderating effects and associated mechanisms of risk and resilience factors.

Response: Thank you for this comment. For the analysis among the subgroup of women with either demographic and/or mental health risk, our original analysis examined bivariate associations between potential protective factors and outcomes dichotomized at the extreme ends. This ensured that we were identifying those factors that clearly distinguished children at high risk for behaviour problems from those with low risk. However, the reviewer’s comment regarding the middle range should also be addressed. To this end, we have provided an additional comment in the Discussion regarding the results when the same analysis was performed that included the middle range. The outcomes were categorized as follows: high risk (at or above the 80th percentile of the distribution) and not high risk (below the 80th percentile of the distribution) for each outcome. Of note, the results were similar and did not change the interpretation. Those factors that were identified as protective when extremes were compared were also identified when we included the middle range in the comparison group, which likely reflects the fact that the majority of children scored on the lower end of the scale (less manifestation of behavioural problems) and that the 80th percentile cut-off was not in the extreme end of the scale’s range (4; range 0-12 for internalizing, and 8; range 0-19 for externalizing).

Further, we examined the outcomes as continuous variables for the resilience analysis. However, we decided to present the categorical results given that they are more readily interpretable for the reader. Protective factors that were significant in bivariate analysis when continuous outcomes were used only provided information with respect to a minor difference in scale scores (e.g., 0.5 or 1 point on the behaviour scale), which is less meaningful. Further our approach to the resilience analysis was not testing interactions (moderators) using the whole sample, but rather associations in bivariate analysis among a subgroup of women identified at risk, as this aligned with our previous work using this data and examination of protective factors for outcomes at age 5. However, to address the reviewer’s comment, we have provided context with respect to further tests of resilience that could be undertaken to provide a comprehensive understanding of risk and resilience for behaviour problems in middle childhood.

Minor Essential Revisions

2. Many proposed risk and resilience variables examined in the Results section were not discussed or reviewed at all in the Introduction section (e.g., parenting morale, adequate good quality time, etc.). It may give the readers an impression that the authors only randomly assessed whatever information available in the questionnaires and their testing of hypotheses was not being theoretically driven. Please consider add some discussion of examined variables in the Introduction section and explain the rationale why examining these variables as risk or resilience factors.

Response: Thank you for this comment. We have added text to the Introduction section of the manuscript to address this point.
Responses to the comments of Reviewer #2

Major Revisions

1. Title: ‘This should be early childhood risk and protective factors for …’ The study does not address the life course only life up to 8 years.

Response: Thank you for this comment. The title has been changed in the manuscript to reflect the reviewer’s suggestion.

2. Results: There is a need for a supplementary table showing univariable associations in order that the reader can assess which variables were tested.

Response: Thank you for this comment, however given the large number of independent variables tested and the stringent criteria that we used to determine potential inclusion in multivariable analysis (i.e., p<0.01), we did not present the results of the bivariate analysis. To address the reviewer’s comment, we comment on the number and types of variables that were identified during bivariate analysis (at p<0.01) for each outcome.

3. Interpretation of data: An important problem relates to comments on the prevalence of emotional and behavioural problems in this cohort. Defining emotional and behavioural problems by the 20th centile cut point and then reporting prevalence as about 20% is tautological. To be meaningful the cut point needs to be externally referenced and benchmarked against other measures. Otherwise there is no justification for making any comment about prevalence.

Response: We agree with the reviewer’s comment and have removed the term ‘prevalence’ when discussing these outcomes. We note in the Discussion that although this cut-off has been used in other research that has examined these scales, it is not externally referenced and is dependent on the sample distribution.

4. Discussion: The lack of data on fathers needs to be addressed as a limitation. Our knowledge of predictors is profoundly biased towards those measured in mothers because the researchers starting these studies did not appreciate the importance of fathers and studies like this perpetuate that bias.

Response: Thank you for this comment. The authors fully agree and share the sentiment. We have added some discussion with respect to this limitation which heeds the call for further research that examines risk and resilience factors as they pertain to fathers.

Minor Revisions

5. Background: needs to emphasise the long term risks to mental health from childhood emotional and behavioural problems. At the moment general health, academic and social problems are mentioned but the health need is justified on the basis of spend on mental illness

Response: Thank you. We agree that the long term health impacts should be emphasized. We have modified the Introduction to provide further emphasis and clarity about this point.

6. Methods: There is room for confusion in the tables and the text about when risk was measured. For example in table 1: Demographics - when was history of demographic risk assessed; maternal characteristics: - when was history of mental health risk assessed; when was poor emotional health assessed; etc
Response: Thank you for this comment. We have clarified timing of assessment in Table 1.

7. Methods: Does maternal abuse (page 9) include abuse of the mother when she was a child; or does it only relate to partner abuse?

Response: Thank you for this comment. We have added text to clarify timing of abuse in the Methods section.

8. Interpretation of data: Protective factors are not demonstrated to be causal only associated – they should be labelled factors associated with good outcomes. Good academic performance, good self-esteem and good peer relationships are manifestations of good mental health they may or may not be causes of it.

Response: Thank you for these comments. We have addressed these issues in the Discussion section.

9. Discussion: I am concerned about the labels used to discuss the results. The term ‘social environment’ is too broad and is not often used to encompass parenting and parent-child relationships. The latter together with maternal mental wellbeing are the key independent predictors and should be labelled as such. It is noteworthy that for both outcomes two different measures of parenting quality were predictive independently of each other. These measures are likely to have a reasonably high correlation and so will share some of the variance. The fact that they are independently predictive is important to emphasis. The very different levels of ‘risk’ in the different data sweeps need to be discussed. Why is the prevalence so high in the 8-year data sweep and what does this mean for assessment of risk at other ages?

Response: Thank you for these comments as well. We have attempted to address them in the manuscript. The sentences referencing the broad social environment have been modified or removed and text emphasizing the independent influence of the two variables has been added to the Discussion section. Text providing some clarification about the maternal mental health ‘risk’ variation has been added to the Results section.

Responses to the comments of Reviewer #3

Major Revisions

1. A comparison of demographic data for respondents versus non-respondents: this will assist the reader in determining how generalizable these findings are.

Response: Thank you for this comment. We compared the characteristics of non-respondents vs. respondents in the present study based on information gathered in a previous CPC follow-up study. Respondents were more likely to have higher income and education levels. Therefore, we address the generalizability of our findings in the Discussion. The potential for selection bias remains for our resiliency analysis and we also comment on this in our limitation section.

2. Consideration of the impact of parenting and school performance on behaviour and vice versa for cross-sectional analyses as noted above.

Response: Thank you for this comment. We have added text to address the issue of temporality as per another reviewer’s comment. This is an important consideration to note.

3. Consideration of the very strong gender difference for both internalising and externalising behaviours
Response. Thank you for this comment. We agree with the reviewer that this is an unexpected finding and have made note of that in the Discussion.

Minor Revisions

4. Inclusion of some of the literature in this field from Europe.

Response: Thank you for raising this point. The bias towards North American literature was not intentional. The authors hope that the addition of several relevant European references will help partially rectify this imbalance and potentially increase the perceived generalizability of the study.