Author's response to reviews

Title: Profiles of Family-focussed Adverse Experiences through Childhood and Early Adolescence: The ROOTS Project, a community investigation of adolescent mental health

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Dear Editors

Re: Profiles of Family-focussed Adverse Experiences Through Childhood and Early Adolescence: The ROOTS Project a community investigation of adolescent mental health

We would like to thank both reviewers for their helpful suggestions. I attach a revised manuscript with this letter and address their comments below.

Reviewer 1: Corina Benjet

Overview
1. ‘The greatest strength is the novel approach of dividing adversity exposure into developmental periods, but the authors do not fully exploit the questions that can be addressed with this approach.’:

We are grateful for the positive comment about the use of our new procedure to estimate timing of exposure to adversities over 3 childhood periods (approximating to under 5, 5 to 11 and 11 to 14 years). We accept that in this paper we do not go further to provide putative causal analyses of the associations between timing of experience and psychopathologies. We did so deliberately in order to emphasis the results of the timing variable for three reasons.

First as we introduce a new methodology it is important to describe the procedure and the ensuing results in full detail. The reviewer was helpful in pointing out where we had not done so with sufficient clarity and we have therefore attended to these points as detailed below. Second some ‘proof of principle’ validity has to be presented or we may not show any advantage over existing methods and we prioritised this in the paper. Hence we tested a priori theoretical predictions regarding gender differences and associations with lifetime DSM defined psychiatric diagnoses assessed at entry into the study at aged 14 years. Third since both social inquiry measures and estimates of psychiatric diagnoses are retrospective there is likely to be a degree of non independence between them preventing causal interpretation from any further detailed analyses.

In our view causal investigations will be best achieved by testing the influence of timing of exposure to early adversity classes on the subsequent trajectories of behaviour. This data will be available as we have so far completed repeated measures follow ups of this cohort on 2 occasions at 15.5 and 17 years of age to date.

Abstract

1. In the methods section of the abstract the authors write “Adversities were recorded in three time periods” This could be clarified by adding the word “retrospectively” after “recorded.”

‘Retrospectively’ has been added as suggested. We have also amended the text to reflect recent studies we did not cite and thank the reviewer for pointing this out.

Background

1. In the background section the statement “Few studies have characterized the nature and associations between adverse experiences over the childhood years” is not quite accurate.
The sentence beginning ‘Few studies …’ has been amended to ‘This study adds to the growing body of work which explores the nature and associations between adverse experiences over the childhood years’.

2. Introduction:

Page 5: this sentence specifically refers to early studies as stated in the first sentence of the paragraph which begins ‘However early studies …’. With this in mind, we have edited this section for clarity. The paragraph does progress to say (paragraph 3, page 5) that studies have moved on to adopt ‘fuller examination of the configurations…’.

3. "It should be noted that quite a few studies recently are using modeling approaches which capture the type of adversity and the number of adversities simultaneously

We did reference a number of these studies with a focus on recent papers from the Angold and Costello and Copeland group but are pleased to add further references and thank the reviewer for bringing these to our attention.

Methods

1. The description of the interview doesn’t provide enough detail to understand how family discord, maternal affection/engagement, parenting style, and child maltreatment were assessed.

We agree with the referee that there was insufficient information on the CAMEEI development and method. This section has now been amended to provide much greater detail on the method and procedure. We had however already stated that the interview is semi-structured (line 1, page 7) rather than unstructured. We have included a historical context describing the origins of our social inquiry methods. We have given details and definitions of contextual methodology, rating of negative impact on family relationships. We have also added a sample page from the CAMEEI as Additional Information. Details of inter-rater reliability are given in the results section, page 14.

2. ‘The strikingly low rates of child maltreatment as mothers are asked to report on behaviour which might incriminate them or their partners.

We agree that asking parents to admit directly to child maltreatment whilst they remain in care and control of their offspring is problematic and could lead to under-reporting of abuse. We are not entirely in agreement with the referee about our prevalence as our 6% lifetime rate is focussed exclusively on defined overt and formally recognised maltreatment. It is precisely for this reason that we introduce a section in the CAMEEI devoted to parenting style where we obtained detailed contextual information on parenting style where lax/indifference, very strict, cruel-to-be-kind, smacking and inconsistency components were combined to form a composite variable as the prevalence of each were low. These style
questions probed indirectly putative forms of subtle milder maltreatment. We have clarified this in the methods section in detail. In discussion we note that our atypical parenting class may mark a putative ‘maltreatment risk’ construct. We accept that subtle degrees of sexual maltreatment are particularly difficult to reveal in any respondent based instrument and have made this clear in our discussion of this novel atypical parenting class.

3. Using the MINI to obtain estimates of family psychopathology

We set high diagnostic thresholds with clear evidence of impairment to minimise over-reporting by mothers both for their own histories and for other relatives. We agree that obtaining family psychiatric history from a single maternal respondent is challenging and have now acknowledged this in the text.

Results:

1. ‘Exposure to atypical parenting (class 4) almost doubled the OR for NSSI and increased it by 1.5 for depressive disorders’ –

This has been altered in the text to read ‘suggested an increase in the OR for NSSI and depressive disorders although it should be noted that these associations were not significant by conventional statistical testing’.

2. ‘We found an inverse association between psychopathology’

We have changed this to say an ‘interesting’ association.

3. Tables:

As requested we have added a column to Table 1.

Atypical parenting has been relabelled on all tables and figures as suggested.

Discussion:

1. The third paragraph of the discussion which refers to 20% probability and 10% probability, are the authors referring to frequency or probability of what? Wording should be clarified.

The probabilities reported relate to the probabilities from the one class model, i.e. the sample average. However, as only one class is returned these reflect prevalences. It was considered preferable to report model probabilities here rather than refer to prevalences (i.e. Table 1) as we go on to discuss probabilities from specific classes in the same paragraph.

Parenting: atypical parenting is now fully described on page 7

2. The third paragraph: because the findings of atypical parenting are novel, it would be helpful to have a clearer picture as to what is meant by atypical
parenting or deficient parenting.

We agree with the referee and have clarified the information recorded and rated (see above) and the putative implications of this class of parenting adversity in the discussion.

3. The sixth paragraph: the authors’ statement that “Our atypical parenting class was associated with NSSI and depressive disorders” is not accurate as the confidence intervals include 1.00.

Additionally it should be pointed out that no adversity class was associated with higher odds of anxiety disorder and the atypical parenting.

We have redrafted and clarified this statement in the text.

4. Additionally it should be pointed out that no adversity class was associated with higher odds of anxiety disorder and the atypical parenting class was not significantly associated with any diagnosis.

We have clarified this in the discussion.

Limitations

1. The authors should acknowledge the limitation of asking mothers to report on potentially self-incriminating behaviors and that this has likely led to an underreporting of child maltreatment and possibly a minimizing of deficient parenting.

We have extended this paragraph to include our reliance on maternal report on p24. We argue however that the atypical parenting class captures a substantial degree of subtle maltreatment and parenting deficiencies and have made this inference clearer in the discussion.

Reviewer 2: Professor Ronald Kessler:

Data Analytic Strategy

1. The results are interesting, but you haven’t demonstrated that we gain anything by thinking in terms of LCA rather than more conventional multiple regression analysis either on individual CA measures or factor scores based on factor analysis or multidimensional IRT analysis of individual CA items. And, in fact, we might lose information to the extent that there is significant variation within the classes that you’re ignoring by focusing only on the classes.

The aim of our analytic strategy was to develop a summary measure of adversities which captured the relationships between a set of family-focused negative experiences in the most parsimonious way at operationally defined time points and longitudinally over the first 15 years of the probands life. We demonstrated that this is best achieved by using the LCA method. We have clarified our data analytic aims in the text and noted that we are not at this stage
attempting to make prognostic predictions of the contributions of different adversities on the subsequent emergence of mental disorders or the course of those indentified in this wave of assessment.

In relation to other model based approaches, we initially explored a factor analytic approach using categorical data factor analysis in Mplus. However, we did not get a good fit to a unidimensional model. A two factor model, with the parenting items loading onto the second factor provided a better fit to the data. Overall however we determined there were insufficient items for a sustainable 2nd factor. We have added our Mplus findings into the exploratory analysis section on page 11 to improve the clarity of the incremental steps we took in our data analytic strategy. We preferred the latent class approach where atypical parenting (with lower levels of other adversities than the moderate or severe adversity classes) formed a separate and distinct class.

Our simple correlation matrix showed that the indicators did not occur in isolation, so knowing that one indicator has more predictive power than another (in terms of psychopathology) from a multiple regression, would not necessarily aid our understanding of the relationship between the adversity indicators nor would not have provided us with such a measure for use with other outcomes. We do not feel that adding each indicator and the LCA classes into one regression model is a viable approach, since there are problems with multi-collinearity.

2. The whole purpose of the paper is to provide a better way of characterizing CA effects and you fail to give us any evidence that the approach you take really is a better way.

The investigations we carried out and reported in this paper were summary and descriptive not predictive in nature. We provided discriminant validity data for lifetime diagnostic clinical groups, gender, social class (via UK ACORN categories) and show difference with maternal age at birth. We also show that our class model provided a better fit to the data than a more complex 5 or 6 class model suggesting that further disaggregation of individuals did not improve representation of the data.

We certainly do agree however that there is a wealth of variation within and between classes which may not be captured entirely by our analyses. It is our intention to determine which quantitative methods will best reveal predictive validity on the prospective data that we now have at our disposal. We will report these when we have collated the repeated measures assessments on mental illness and wellbeing together with other measures we recorded at 2 further time points at 15.5 and 17 years respectively. We have added a paragraph in the discussion noting alternative data analytic strategies may be required when undertaking prognostic modeling with repeat measures data.

3. Consider the use of discrete-time survival analysis to characterize onset of the outcomes.
The comments on use these approaches are appreciated. The data on diagnoses and adversity presented here were collected retrospectively and precise timings of onset are often difficult to establish with any certainty. Thus we do not have the required level of dating onsets and offsets of experiences or psychopathology. We agree that discrete-time survival analysis could be an approach when we consider the associations between early adversity and the emergence of psychopathologies in later adolescence and young adulthood.

Editorial requests:

Authors’ contributions: manuscript revised as requested

Ethics and consent: manuscript revised as requested

General Comments:

Many suggestions, from both reviewers, concern important aspects of the work that will be addressed in future papers. This manuscript is key in describing our methods and the details of the early experiences measure in this prospective cohort investigation. By so doing we provide the platform and context for subsequent reports detailing the relative contribution made by differential exposure to early adversities on the subsequent emergence of complex behavioural phenotypes and mental disorders. Our primary aim in this paper was to delineate profiles and patterns of adversity using a measure specifically designed for the ROOTS study but based on substantial prior social evaluation research by ourselves and other groups in the UK. In this manuscript we provide data to demonstrate the necessary first step in validating our new measure and to introduce the use of timeline methods and display associations between timing of exposure and gender of probands. Future papers will address the complexities of timing on exposure with the developmental trajectories of behavioural phenotypes and diagnoses. The latter are themselves not straightforward as we are sure the referees appreciate and could not be fully dealt with in this report. We have taken cognisance of the referee’s desire for clarity over measurement details and of course where we omitted to adequately reflect the current science in the field.

We trust that the reviewers and the editorial committee find our responses acceptable and look forward to a decision on publication. Do please get back to me if you have any further queries.

Yours sincerely

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