Author's response to reviews

Title: The effects of house moves during early childhood on child mental health at age 9 years

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Author's response to reviews: see over
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Re: MS 5738020162099731 - The effects of house moves during early childhood on child mental health at age 9 years

Thank you for your correspondence concerning the above manuscript, and for providing us with the opportunity to revise and resubmit our manuscript.

We have addressed each of the Reviewer’s comments in the text that follows.

Thank you for re-considering our article. We believe it has been strengthened considerably by attending to the comments. We look forward to your reply.

Yours sincerely

Alice Rumbold
on behalf of all authors
Reviewer: Tim Jelleyman

Reviewer's report:
This study examines the association between residential mobility (defined as 0, 1 or 2+ moves) and behavioral outcomes at age 9 yrs in a cohort of children from an ongoing longitudinal Australian study. Three models are considered: moves in three phases over life course, moves over whole time, and a defined change in housing status. A range of covariates are included and adjusted for in the analysis. There is therefore an attempt to consider residential mobility as an independent variable. Appropriate statistical tools are used. The paper is of interest in the more specialized area of the contextual factors of housing and neighborhood that relate to child behavior outcomes.

1. The authors comment that the sample is 'broadly representative of all women who gave birth in South Australia.' The reference [17] is a study on weaning from same cohort, but not sure if this is the evidence for statement made. For instance, what is the background rate of moving for families with young children, and how does this compare with the study population? Recommend review, and provide more support for this statement if necessary. [Discretionary revision].

Our comment regarding the sample’s representativeness referred to the characteristics of the birth cohort at the time of recruitment (but the reference should have been to Moore et al., originally [15] not Walker et al. [17], which we have corrected in the revised manuscript).

There are no directly comparable official statistics concerning house moves (i.e. focussing on families with young children). However, data from the Australian 2007-8 Survey of Income and Housing showed that among households with dependent children (i.e. all ages), 47% had moved at least once in the previous 5 years (ABS 2009). This is consistent with the data reported in our paper: in the five year interval from child age 5 – 9 years, 35% of children had moved house at least once, and 60% of study children had moved at least once between birth and age 9 years.

We have added sentences about the sample’s representativeness with respect to house moves to the paragraph concerning study strengths in the Discussion.

2. From another angle they note that at the 9 year follow up the majority of families (81.9%) had purchased their own home; is this still representative of the general population and is there sufficient representation within the study to demonstrate outcome differences for the group with whom we might have the greatest concern? [Minor revision]

There are no directly comparable official statistics available. However, data from the Australian Bureau of Statistics (2009) showed that 78% of South Australian households with dependent children were purchasing or owned their dwelling (ABS 2009), concordant with our reported figure for mortgage or outright ownership of home of 81.9%.
As shown in Table 1, 30.8% of the sample (n=124) were from families where the mother had not completed high school, and 29.5% of the sample (n=119) were from families earning < $31,199 at late pregnancy interview. A total of 178 of families (44.2%) were in either or both of the lower maternal education/household income strata. Of these families, 121 moved at least once (68%). In comparison, 119 of the 225 families who were not disadvantaged (53%) moved at least once. Thus there was adequate representation of different subgroups in our study sample. That we were able to show statistically significant associations between house moves and child behaviour demonstrates that our study had adequate power.

3. The empirical findings are adequately constructed. The difficulty is with interpreting and I think this is to some extent covered in the discussion. Certainly the drivers for moving are heterogenous in the first place and unevenly distributed across the population leading to many confounding underlying patterns being likely. The middle paragraph on p14 partly covers this, but could be strengthened. [Discretionary revision]

We agree that our statements could be stronger, and have modified the paragraph on p14 to now read:

“High levels of mobility can be a marker of a complex range of circumstances, including instability in other aspects of life and disadvantage, and are likely to be unevenly distributed across the population. We adjusted for a range of socioeconomic indicators, stressful events in the family, and relationship breakdown, which represents the most potent potential confounder due to it being the most common reason for a ‘non-aspirational’ house move (ABS 2011). The association was robust to adjustment for these variables. Although we cannot rule out that the association is due to residual confounding from other unmeasured factors these would be expected to have an at most minor influence on the observed associations.”

4. The tables in their current form are difficult to read, partly as the column headers do not transfer across pages in the draft. Also there is a lot of information and it is difficult to quickly see where the significant results are located. This may be addressed in the edited formatting. [Discretionary]

We have now carried the column headers across pages and have more consistently aligned the text within the columns.
Reviewer: Joseph Gasper
Reviewer's report: It was a pleasure to read the manuscript “The effects of house moves during early childhood on child mental health at age 9 years.” The authors make several important contributions to the literature on the effects of residential mobility on child behavioral outcomes by considering the timing of residential mobility, the number of moves, and housing trajectories. The analyses are sound and conclusions generally recognize the limitations of the study. However, I do have some minor comments for revision.

Minor Essential Revisions
1. On p. 4, the authors claim that prior research on the effects of residential mobility on child behavior is mixed and that prior research has not used longitudinal data or rigorous controls for confounding. The authors should include the following study, which examined the effects of residential mobility on delinquency and problem behavior using longitudinal data and is one of the only studies to use controls for unobserved confounding: Gasper, Joseph M. et al. 2010. Coming and Going: Explaining the Effects of Residential and School Mobility on Adolescent Delinquency. Social Science Research 39:459-476.

Note that we stated that the majority of previous research was cross-sectional with varying attention paid to confounding. We have added a sentence describing Gasper’s study and findings to Paragraph 2 in the Introduction, and also noted differences in analytical approaches between studies in Paragraph 3 of the Introduction.

2. On p. 5, the authors say that the sample is broadly representative of all females giving birth in South Australia, but the sample does not include teenage mothers and is Caucasian. While I am unfamiliar with South Australia, it is unclear how much a sample could be described as broadly representative. The authors should show how the sample is similar to (or differs from) the population of this region and perhaps soften the claim of the representativeness of the sample.

The South Australian population is predominantly Caucasian and at the time the cohort was established, 92% of births in South Australia were to Caucasian women. Less than 5% of births to Caucasian women occurred in those aged under 18 years. A detailed discussion of the representativeness of the sample was provided in a previous publication (ref 15) and we have now made that more explicit.

3. I think one weakness of this study in terms of covariates is that it does not include measures of externalizing/internalizing behaviors prior to moving. It is therefore very difficult to know, even with longitudinal data and the covariates the authors have included, whether the move actually increased behavioral problems in cases where the authors observe significant relationships. Demographic covariates may not be (and are likely not) sufficient to control for pre-existing differences in child behavioral problems. This is probably a tall demand for a data set to have repeated measures of child behavioral problems at such an early age, but this limitation should be acknowledged in the Covariates section and/or Discussion.

We agree and have added a sentence to the paragraph concerning study limitations in the Discussion: “It is also possible that inclusion of measures of child behaviour prior to...
any moves could have changed the results and their interpretation, if families with children with difficult temperaments and/or later high internalizing behaviour problem scores were more likely to move.”

4. For the upward and downward housing trajectory measures, it is unclear whether the authors are simply using measures of housing at each survey date or have retrospective housing histories covering all periods since the last interview. Not having full histories could mean individuals are misclassified (e.g., moves are missed). Please clarify.

We have amended the ‘Residential mobility’ section in the Methods to clarify the number of house moves is based on the entire housing history to age 9 years, whereas the trajectory measure is based on changes in housing tenure between age 2, 3.5 and 9 years. The number of house moves in each interval was also used to check for stability or change in housing tenure. We have also added 3 sentences to the paragraph concerning housing trajectories in the Discussion (p 16) that our measure of housing trajectories may have missed some changes in tenure status, but its likely effect on the results is small:

“It is possible that our measure of housing trajectories did not capture all changes in housing tenure status across childhood. However, the number of house moves in each interval was also used in the derivation of the housing trajectories. The proportion of misclassified trajectories, and any effect on their association with child behaviour, is likely to be small.”

5. The authors should explain the rationale for including interactions with school moves (and there is a very good one), which they currently do not. I also think it would be helpful to say something about this in the conclusion. The finding contradicts with prior research that suggests that the reason for the link between residential mobility and negative outcomes is school mobility.

We agree there is a strong rationale for this. We have strengthened the statement in paragraph 4 of the Introduction to now read:

“For example, moves occurring when the child has commenced school may have a greater impact on behaviour than at earlier ages, due to disruption of established friendships and peer groups, which are increasingly important in later childhood and early adolescence. Also important is the potential for school moves to independently contribute to the development of behaviour problems through its impact on school achievement and coping (Boon 2011).”