Author's response to reviews

Title: A three generation study of the mental health relationships between grandparents, parents and children

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Version: 2 Date: 21 October 2013

Author's response to reviews: see over
Dear Editorial Office,

Manuscript ID: 5364699069688672

We thank the reviewers for their constructive comments and feedback on our paper titled “A three generation study of the mental health relationships between grandparents, parents and children”.

We have revised our analysis as suggested by the reviewers and made subsequent modifications to the paper, which are detailed below. The revisions to the manuscript are also referenced below. We believe the revisions have improved the manuscript, and hope that the revised paper will be of interest to you readership.

Yours sincerely,

Kirsten Hancock, for the authors, Francis Mitrou, Megan Shipley, David Lawrence and Stephen R. Zubrick.

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**Reviewer 1: Virginia Warner**

**Major Compulsory Revisions**

1. *The authors need to demonstrate with the data that they have why the results are not solely due to informant bias. Was there other data that was collected that was not subject to potential bias that supports the findings?*

   We recognise that informant bias, particularly in relation to mental health problems in grandparents, is a limitation to the study that is difficult to quantify. We cannot say for certain that informant bias did not occur at all, however there is some data that suggests the patterns we observed were not due *solely* to informant bias.

   The most relevant data pertain to father report of mental health problems. In the study, mothers completed the questionnaire that asked about the mental wellbeing of the child (SDQ), the mother (K6) and the maternal grandparents. Fathers completed a separated questionnaire that asked about the mental wellbeing of fathers and the paternal grandparents. In our analyses, we show that elevated psychological distress in both mothers and fathers is associated with higher SDQ scores for the child (see Table 3), even though fathers did not complete the SDQ for the study child. This particular relationship cannot be attributed to respondent bias. Similarly, there was a significant relationship between a history of mental health problems in the paternal grandparents and higher child SDQ scores, though we do note a stronger relationship was found for maternal grandparents.

   In other work completed using the LSAC sample, similar patterns have been shown for the intergenerational impacts of joblessness and separation on children (*Hancock, Edwards & Zubrick, 2013*). In that study, joblessness and separation in the grandparent generation were associated with social and emotional wellbeing, literacy and numeracy achievement in grandchildren, similar patterns to the unadjusted analyses we reported in this study. We believe that questions relating to specific events – family separation and periods of unemployment in this case – are examples of less biased or subjective reports. Parental separation, for example, is an event that most children would remember throughout their lives. As we find similar patterns in this study, even though the measure is less objective, we have some confidence that the results are not simply due to informant bias.

   Finally, we note that while we use Wave 3 SDQ scores for child wellbeing and parent mental health, questions relating to grandparent mental health were collected at Wave 2. Thus, if there is any bias relating to state of mind on the day the questionnaires were completed, this would only affect the results relating to the relationship between grandparent and parent mental health.

   We have added some further discussion to the paragraph where we highlight informant bias as a limitation to the study (Discussion, commencing page 19, paragraph 2).

2. *Additional analyses may need to be done to support the findings.*

   We’re unsure if this comment relates to the issue of informant bias, or the analyses more broadly. We have substantially modified our analyses in response to comments from the other reviewers, we hope that taking this action addresses the concerns regarding
analyses. These changes include changing the SDQ from a categorical to a continuous measure, separating all analyses by child cohort, and by including more detailed variables relating to the mental health of parents and grandparents in our multivariate analyses.

3. Why was only 1 child chosen and how does that effect/bias the findings?

The study was a cohort design, and only children who were within the desired age range at the time of sampling were selected for the sample. We do not believe that this design impacted or biased our findings in any way.

According to the sampling report for the LSAC (Soloff, Lawrence & Johnstone, 2005), around 1.5% of families with children in the target population have multiple births, and about 5% have both infants and children aged 4 years. There was a significant respondent burden associated with completing the survey for one child. When designing the study, it was decided that including two or more children from one family would further increase this burden, and therefore attrition. Also, survey instruments would need to be tailored for these specific families, resulting in significant survey design work for only a very small proportion of families. These processes would have required a reasonable amount of development and processing resources. Therefore it was decided that only one child per family should be selected.

Minor Essential Revisions

1. Page 8, the word enrollment is misspelled.

This error has been corrected.

2. Page 13, the first sentence is unclear, should where be replaced to when? Should it be clarified which 2 groups are being compared?

We have changed ‘where’ to ‘when’, and have also included more text in the sentence to make the comparisons more clear.

3. Page 13, were confounders controlled for?

These particular analyses were unadjusted analyses. We have added a note to the relevant tables and in the manuscript text to make this more clear. The adjusted analyses are provided in Table 5.

4. Page 14, next to last sentence, starting with ‘Children with at least one parent…’ needs to be clearer.

The analyses have been updated and this paragraph has now been deleted.

Discretionary Revisions

1. I think the paper could be much stronger if additional findings could be included.

As noted, the analyses have been substantially modified in response to reviewer concerns.
Reviewer 2: Thomas Olino

Major Revisions

1. Although the authors do present strong arguments for relying on cut-points and dichotomizing maternal and paternal K6 scores and child SDQ scores, I have a strong preference for relying on continuous variables.

   We have now modified the analytic approach to examine the SDQ as a continuous measure.

   We still prefer to use the maternal and paternal K6 scores as a dichotomous measure to keep these variables consistent with the measures of grandparent mental health history and to assist with interpretation. For example, the ‘effect’ of mother’s continuous K6 score in the multivariate analysis resulted in a regression coefficient of approximately 0.4 (i.e. for every 1-point increase in maternal K6, SDQ score would increase by 0.4 points). When this value is considered alongside the beta values for the grandparent mental health history variables (most in the vicinity of 0.2 to 0.4 points), a reader not paying close attention may mistakenly believe that the effect of grandparent mental health is similar to parent mental health. When maternal K6 scores are dichotomised, we see that SDQ scores are nearly 2-points higher for mothers above the K6 cut-point. We feel this approach better reflects the magnitude of the effect when comparing the impacts of parent and grandparent mental health.

   The method and results section have been substantially modified as a result of the new approach. The new material in the Method section can be found on page 12 (Statistical analysis), and on pages 13-15 in the Results.

2. I like the sequence of the models presented. Linking G3 to G2 and G2 to G1 and G3 to G1 is nicely laid out. When the authors focus on combining these pairs into a single model, however, I would prefer that the authors do so by examining interaction terms (i.e. G3*G2). The basic request is to be able to see which ORs differ for the combinations of G3 and G2.

   We have retained this approach to the analysis, though the initial tables linking parents to child (G2 to G3) and grandparent to grandchild (G1 to G3) have been adjusted to reflect the SDQ being used as a continuous variable, and separated by cohort.

   The multivariate model now examines the separate effects for maternal and paternal grandparents and grandfathers, mothers and fathers, and the interactions between the grandparent and parent generation (Table 5). We report the interaction between mother and father mental health, however none of the intergenerational interactions were significant, so these have not been included in Table 5. We note in the figure legend of Table 5 the specific interactions that were tested.

3. Were there any differences in associations found between the cohorts? There could be differences in manifestations or processes related to manifestations of psychopathology between 4-5 and 8-9 year old children, particularly if G3 were caretakers for 4-5 year old children.
As suggested, we have modified the analytic approach, and all analyses are now stratified by cohort. Table 2 is not stratified by cohort as there is no difference in rate of elevated psychological distress in parents according to history of mental health problems in grandparents. We would not necessarily expect cohort differences for this analysis as we are only examining parent outcomes, not child outcomes.

We note that with the new analysis we find stronger effects of grandparent mental health for the 8-9 year old children. It is difficult to comment on developmental reasons for why this might occur. The simplest explanation would be that mental health problems do not emerge until the later years, however if this were the case we would also see stronger parent-child mental health relationships for the older cohort than the younger cohort. It could be that there is a cumulative exposure over time, where older children have had more opportunity to interact with their grandparents, or perhaps older children interact with their grandparents in different ways. However, for both cohorts the effect of grandparent mental health was the same irrespective of the amount of face-to-face contact children had with their grandparents.

It is also possible that the SDQ has greater sensitivity in detecting problems in older children than younger children. For example, in Table 3, mean SDQ scores are higher overall for the B Cohort than the K Cohort. It is unlikely that the younger group of children have more problems than the older children. Rather, there are some behaviours that while common amongst younger children represent more problematic behaviours in older children. This would make the SDQ instrument more sensitive for the 8-9 year old children than the younger children. We have included a discussion on these findings and possible explanations for them (see Discussion, commencing page 16, paragraph 4).

**Minor Essential Revisions**

1. *I think it would be helpful for the authors to be more explicit in their assessment that “...there are wider variations in methodologies and in diagnostic tools...”*. I presume this refers to the completeness of the pedigrees that are assessed, direct or indirect assessments, and questionnaires or interview methods. It could be nice to describe these differences in sum, or when presenting each study. *In addition, what are the authors’ thoughts on common findings across these studies?*

   Yes, these are the variations that we refer to. Some further detail has been added to the introduction (see page 4, paragraph 2). We also note on pages 6 and 7 that despite all of the methodological differences amongst the studies, the common theme is that mental health problems in grandparents is associated with poorer social and emotional wellbeing in their grandchildren, though it is difficult to understand how these relationships operate.

2. *Relatedly, I think the authors have an opportunity to examine a question that has not been explored in the published studies. I do not think that any published studies have looked at specific mediational pathways within maternal and paternal G3-G2 lines in the same cohort. That is, does maternal G2 psychopathology mediate the relationship between maternal grandmother and grandfather psychopathology and G3 problems and does paternal G2 psychopathology mediate the relationship between paternal grandmother and grandfather psychopathology and G3 problems? This would be a relatively straight-forward but complicated path model, but it would be very much estimable. If informative, the tests of moderation could also be examined, albeit in a slightly different form.*
There are a number of ways in which to approach the analysis, and a path model being one of them. However, we feel is outside the scope of this study. The papers that have already been published on the topic gave us a good start on outlining the mental health relationships that might exist between grandparents, parents and children. Ideally, the papers might have come to a common finding regarding the role of grandparent mental health, however, as we outlined in the introduction, a consistent pattern of findings has not been achieved. The aim of our study was to determine which of the previous pattern of results our data and analyses would support, and to provide information that might help to clarify the role of grandparent mental health on children’s social and emotional wellbeing.

In saying this, by laying out the separate grandparent-parent and parent-child relationship, and then combining them into a single model, the relationship between grandparent mental health and child social and emotional wellbeing becomes attenuated. This would suggest that although there are still independent grandparent-grandchild effects, the majority of the relationship is mediated through mental health problems in the parent generation.

Discretionary Revisions

1. The authors description of their “second notable finding” highlights an important future research direction. What are the altered processes in G2 that support the link between G3 and G1 when G2 is not affected by psychopathology?

This particular result is no longer apparent with the modified analysis, though we do find that there are grandparent mental health effects independent of parent mental health. As we note in the previous comment, the question is interesting, and we will consider examining this question in more detail in future work.

2. In terms of exposure to psychopathology in G3 – I wonder if there might be other crude ways of examining total exposure to G3 psychopathology for both maternal and paternal grandparents together. Might that differentiate between youth with and without problems?

We considered this approach, for example, looking at the number of grandparents with a mental health history to determine if there was a dose-response type relationship. We found that average SDQ scores consistently increased for each additional grandparent with a mental health history. We considered including this analysis in the current paper, but felt the manuscript already had a lot of information to consider and interpret without the inclusion of further analysis.

If you refer to exposure to psychopathology in terms of the amount of contact the child has with maternal and paternal grandparents, the relationships are less straightforward. In our data (not tabled) grandparents, paternal grandparents in particular, are less likely to have contact with their grandchildren if they had a history of mental health problems. This may relate, on one hand, to circumstances where parents protect their children from unwanted influence. It could also reflect increased levels of stress or anxiety in parents if they do not have family support available, for example when grandparents do not live close to the family.
Despite the sample size, we simply do not have powerful enough data to examine all of these questions within a single model, and we would prefer to avoid publishing a paper with too many analyses as the key findings tend to get lost.
1. As the introduction reads, one of the main selling points of the current manuscript is the availability of data on how much time the grandchild spent with his or her grandparents. However, in my opinion, this information is not optimally utilized: the measure of child contact with their grandparents is dichotomized into two very broad categories ‘frequent contact’ and ‘infrequent contact’.Maybe the authors can elaborate more about the original report form (amount of categories) and create more categories to optimize the variability of the measure.

As noted in Methods, the categories regarding frequency of contact included ‘no contact’, ‘rarely’, ‘a few times a year’, ‘at least every month’, ‘at least every week’, ‘every day’, and ‘don’t have’. Even at this level, there was very little variability in responses as most parents indicated the study child saw their grandparents at least every month, if not every week (see Table 1). The variables included in the multivariate analyses (Table 5) include the categories “No contact or no grandparents”, “Infrequent contact” and “Frequent contact” to provide adequate power for analyses. Further refinement in the categories results in very small cell sizes, leading to unreliable results, particularly when we start interacting the variable with other categorical variables (such as grandparent mental health history). The definition of frequent and infrequent contact has been provided in the notes for Table 5.

2. The study sample included children, parents and information on grandparents from two cohorts: children aged 4-5 years, and children aged 8-9 years. For the analyses, the data on the two cohorts are put together and a cohort-variable is added as a covariate. As it reads from Table 4, the effect of cohort, and therefore the child’s age, is statistically significant. What happens to the reported effects once the cohorts are tested separately? Also, children aged 4 to 5 are in a different phase of development (transition to school) than children aged 8-9, maybe parental or grandparental psychiatric problems have different effects on different ages. Maybe the authors can comment on this?

With the exception of Table 2, all analyses are now stratified by cohort. We opted for stratification given the number of other interactions that could be tested for. The differences between the cohorts are now documented in the Results on pages 14-16, and summarised in the next paragraph. Table 2 is not stratified by cohort as there is no difference in rate of elevated psychological distress in parents according to history of mental health problems in grandparents. We would not necessarily expect cohort differences for this analysis as we are only examining parent outcomes, not child outcomes.

In the unadjusted analyses, the mental health of both maternal and paternal grandparents was related to higher SDQ scores in grandchildren for both cohorts (though we note the marginal significance of the role of paternal grandparents for the B cohort). In the adjusted model, only the maternal grandparents mental health was associated with SDQ scores, and then only for the older cohort of children. As we note in earlier comments (Reviewer 2, Comment 3), the differences for each group of children may reflect developmental differences, or simply that the SDQ has greater sensitivity amongst the 8-9 year old cohort.
3. The measure of child outcome, the Strengths and Difficulties Questionnaire was assessed at Wave 3. As I understand from the manuscript, there was also data collected at two earlier waves. Did these waves also include data on behavioral outcome? If so, would it then be possible to look at time/age effects using repeated data?

With the availability of longitudinal data this is certainly possible but tangential to the central aim of this paper. As more waves of data become available, it will be worthwhile revisiting this question.

4. As for the measure of child outcome, the authors choose to dichotomize the data. However, with such a large sample size, isn’t it possible to analyse the data continuously? This would increase power. Similarly, the measure of parental psychiatric problems could also be assessed continuously.

We have now modified the analytic approach to examine the SDQ as a continuous measure.

We still prefer to use the maternal and paternal K6 scores as a dichotomous measure to keep these variables consistent with the measures of grandparent mental health history and to assist with interpretation. For example, the ‘effect’ of mother’s continuous K6 score in the multivariate analysis resulted in a regression coefficient of approximately 0.4 (i.e. for every 1-point increase in maternal K6, SDQ score would increase by 0.4 points). When this value is considered alongside the beta values for the grandparent mental health history variables (most in the vicinity of 0.2 to 0.4 points), a reader not paying close attention may mistakenly believe that the effect of grandparent mental health is similar to parent mental health. When maternal K6 scores are dichotomised, we see that SDQ scores are nearly 2-points higher for mothers above the K6 cut-point. We feel this approach better reflects the magnitude of the effect when comparing the impacts of parent and grandparent mental health.

The method and results section have been substantially modified as a result of the new approach, therefore we do not detail specific changes here. The new material in the Method section can be found on page 12 (Statistical analysis), and on pages 13-15 in the Results.

5. If I understand correctly, information on child and parental psychiatric history is assessed at the same wave and is therefore assessed cross-sectionally. It is therefore more difficult to draw causal conclusions from the current study, and also reporting bias is an important issue as the same parent also reported on child outcome. Authors should comment on this and also state this as an important limitation of their study.

As with any observational study, we cannot draw any causal conclusions from these data and did not intend to imply otherwise in our discussion. The presence of mental health problems in both parents and children may reflect aspects of their shared environment that we haven’t accounted for, like a death in the family in the weeks preceding the interview, rather than a parent’s mental health causing problems for their child. However, even if we used parent mental health from Wave 2, and used that to predict wellbeing at Wave 3, we still couldn’t conclude that this is a causal relationship. It may be that whatever impacted upon the parents mental health at Wave 2 simply took longer to influence the child.
We have noted this as a limitation in the discussion (last sentence on page 18, paragraph 2).

6. **What I find a major drawback of the current study is that information on grandparental psychiatric history is assessed retrospectively by asking the parent ‘whether or not his/her father/mother suffered from nervous, emotional trouble, or depression’ yes or no.** Besides the fact that this information is not assessed by using a standardized method, it is a very broad measure, and also very likely to be subject to reporter bias. **Is there any information available about the parent-grandparent relationship or the state of mind of the parent at the moment of reporting about grandparent psychiatric history, so that an effort can be made to adjust analyses and try to diminish reporter bias?**

The authors did not have input or control over the question design, and we agree that the retrospective recall is a significant limitation to the study. There is no information available about the quality of the parent-grandparent relationship, or about the state of mind of the parent whilst making the report, besides of course the K6 measure.

We note that in surveys by the Australian Bureau of Statistics that around 20% of the population have ever been treated for a mental health problem during their lifetime (Lawrence, Mitrou, & Zubrick, 2011 for example). This figure is broadly comparable to what is reported for the proportion of grandparents with a history of mental health problems.

There is other information available to indicate that while there is likely to be some informant bias, the data are still valuable and informative. We point to our detailed response to the first comment from Reviewer 1, and note that we have added further discussion on page 19.

7. **Why isn’t maternal age at birth of her first child used continuously?**

   The relationship between maternal age and both K6 scores and SDQ outcomes is non-linear. As a result, we used categories to reflect the age groups that reflect transitions to adulthood and parenthood; teen mothers (19 years and under), younger mothers (20-24 years) and older first-time mothers (25 years and higher). We included this variable as a covariate to account for the disadvantage typically experienced by teenaged mothers.

8. **How are ‘ongoing medical conditions of parent or child’ assessed? What does this measure include, and what is the rationale for including medical conditions as a covariate?**

   Information on medical problems for each of the family members was collected at Wave 3 and provided by Parent 1 (typically the mother). For each family member, Parent 1 was asked “Does (child) have any medical conditions or disabilities that have lasted, or are likely to last, for six months or more”. Examples included sight or hearing problems, learning difficulties, disfigurement or deformity, head injury, amongst others.

   The co-morbidity associated with physical health and mental health conditions is well known. We included the covariate to account more for the parent-child relationship than the grandparent-grandchild relationship, and wanted to account for situations where mental health results from ongoing medical conditions. For example, the presence of a
chronic health condition in the child may cause excess stress and anxiety in the parent. Though we acknowledge we cannot make causal conclusions with these data, we feel it is still appropriate to account for factors that may relate to inverse causation.

9. I miss a table with descriptive statistics including all variables, also the covariates.

A descriptive table has now been included (Table 1).

10. The presentation of the descriptive of psychiatric problems in the grandparent-parent and parent-child generations are not very clear; from Table 1 and Table 2, statistically significant differences cannot be read, no p-values are reported. Also, in the text and above the tables it should be stated that values are based on crude analyses.

With the change to using the SDQ as a continuous variable, these tables (now Table 2 and 3) have also been amended. We now report mean SDQ scores and provide t-tests and p-values to make significant differences more clear. We have also amended the titles so it is more clear that these are unadjusted analyses.

11. As for the analyses including face-to-face contact with the grandparents (page 13), I do not understand why the analyses are stratified? Why not perform adjusted regressions? Similarly for Table 3, as for Tables 1-2, the format is not clear; it is hard to read from the table which differences are statistically significant and which are not. It follows that children with maternal grandparents with a history of psychiatric problems, and not children with paternal grandparents with a history of psychiatric problems, have more behavioral problems. Then, why not assess the association (as presented in Table 4) separately for maternal and paternal grandparents? Do the authors have an explanation for this finding? It would be interesting to elaborate some more on this in the discussion.

This particular analysis has been removed in the modification to the Results section and other tables have been modified to be more clear.

Stratified analyses were an alternative to including a large number of interaction terms in the model. Simply controlling for these variables in the model would not answer the question regarding whether frequent contact with grandparents would result in stronger relationships between grandparent and grandchild mental health. In the new multivariate analysis we have included the separate face-to-face contact variables in Table 5 and tested the interactions separately. No significant interactions were found.

12. As the face-to-face contact variable is a selling point of the current article, why not include it in all analyses. It was hypothesized beforehand that it is an important covariate...

We initially conducted the analyses this way, and found that including the contact variable as a covariate made little difference to the patterns we observed. After doing this, we also realised that simply including the variable as a covariate does not address the reason why it’s an interesting variable. The point of including face-to-face contact, we believe, might provide context around the “transmission” of mental health. If a child has never met a grandparent, then the only way that grandparent can logically influence the child is through the parent generation, be that through inherited genes or environment. In this respect, the contact variable should be included as an interaction
term in the model – that is, if grandparents have a direct influence on grandchildren, then we would expect that the role of grandparents on grandchildren would be stronger in families that are in frequent contact compared to those with infrequent contact. These interactions were tested and found not to be significant, likewise, stratifying the results by grandparent contact (a proxy for examining interactions, allowing us to identify a significant relationship for one group but not another) also made little difference to the results. Hence the final model that we presented in the original manuscript excluded grandparent contact. As noted above, this approach has now been modified.

There are other problems with the contact variable that have not as yet been discussed. We do not know the reasons behind frequent or infrequent contact. In some cases, grandparents were deceased. They may live a long distance away, or there may be relationship difficulties – which may or may not be related to any mental health conditions – that mean parents feel it is in the best interests of their children that they not have contact. We did find that grandparents were less likely to have frequent face-to-face contact with grandchildren if parents reported that they had ever had a mental health problem (not tabled). These may be reasons why we did not find a relationship. We have added some thoughts along these lines to the discussion (commencing last sentence of page 17).

13. **In the introduction as well as in the discussion, the authors talk about direct and indirect mechanisms of transmission of risk, and about genetic and environmental mechanisms. However, they give little background on how they hypothesize the different mechanisms work. A bit more elaboration would help the reader to better understand the hypothesized pathways.**

   We have added some further information to the introduction and discussion that elaborate on possible pathways (Introduction - pages 6 and 7, paragraphs 2 and 3; Discussion - page 17, paragraph 2).

14. **Do the authors have information on whether or not the parents or the grandparents are the biological (grand)parents of the child?**

   We mention in the methods that for the majority of families parents were biological mothers (97%) and fathers (92-96%). There is no information available about the biological heritage of grandparents in the survey. In fact, the survey questions relating to family of origin ask parents about their mother, or *mother figure* (and same for fathers). We have added a sentence about the uncertainty of the biological heritage of grandparents to the method section to clarify (page 9, paragraph 3).

**Minor points**

1. **The grammar of the first sentences of the results section of the abstract is incorrect.**

   With the new analysis, the abstract has been rewritten.

2. **References should be included in the introduction where the authors discuss limitations of previous studies (second paragraph, page 6).**

   References have now been included in this paragraph.