Author's response to reviews

Title: The relationship of living arrangements and depressive symptoms among older adults in sub-Saharan Africa

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Author's response to reviews: see over
Dear Editor:

Please find attached a revised version of the manuscript, “The relationship of living arrangements and depressive symptoms among older adults in sub-Saharan Africa” (MS: 1385072848848107), and a point-by-point response to the reviewers’ comments (below).

We thank the reviewers for their helpful suggestions and we feel the manuscript has substantially improved as a result.

Yours sincerely,

Brittany McKinnon
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Response to Review 1:

Major Revisions

1. The Introduction section should provide a more informative and interesting description of the study context, motivation, and rationales. The current Introduction describes changes in living arrangements in Africa (1st para), research on depression mostly from other regions and one study from Nigeria (2nd para), and the study hypothesis (3rd para). The hypothesis statement (first sentence in the third para) sounds quite abrupt, as little has been mentioned of the conceptual framework linking living arrangements and depressive symptoms in the context of Africa. Lack of support from working-age adult children may be only one of many mechanisms of increased depressive symptoms among older adults living alone. Older adults living in skipped generations may suffer greater depressive symptoms from grand-parenting responsibilities but at the same time may benefit from interactions with young grandchildren. I am not asking the authors to provide a lengthy treatment of gerontological theories on these issues, but a context-specific conceptual framework is an essential element to motivate the study and to help interpret the study results more meaningfully than just finding statistical associations.

2. Related, the hypothesis stated (first sentence in the third para) does not correctly reflect what has actually been tested in statistical analysis. In other words, the study does not test whether “conditions of poverty, declining family support systems, and increasing role as care givers to orphan and vulnerable children may increase the risk of depressive symptoms.” Rather, it tests whether two living arrangements are associated with greater depressive symptoms. The hypothesis statement should focus on operationalized terms (that is, measurable terms and study variables). Moreover, because wealth is included in the regression models, the estimates capture the direct effect of living arrangements, after accounting for the indirect, pecuniary effect of living mediated by poverty. Overall, the study hypothesis and research questions examined should be presented more clearly and explicitly.

We have re-structured the background section and provided more rationale for the specific research questions—particularly with respect to what is known about older adults living in skipped-generation households. Furthermore, we have taken out wording that is not directly related to the specific research questions addressed in the paper.
3. The rationale of including country-level HIV/AIDS prevalence in regression models is also problematic. I agree that higher country-level HIV/AIDS prevalence may lead to higher rates of older adults living alone or in skipped generations, but the regression model already includes living arrangements as the main variable of interest, in which case what is actually captured by the country-level variable of HIV/AIDS prevalence is the direct effect of “prevalence” of older adults living alone or in skipped generations. It could be that older people have greater depressive symptoms in societies with higher HIV/AIDS prevalence, independent of their living arrangements, but the rationale provided is not described that way.

To clarify the rationale behind using HIV/AIDS prevalence as a predictor in the meta-regression analysis, we have added the sentence, “We hypothesized that older people living alone and in skipped generation households in countries with a greater burden of HIV/AIDS may be especially vulnerable to depressive symptoms because of the greater likelihood they may have reduced levels of material support for themselves and dependent children.”

In conducting the country-level meta-regression, we were simply examining whether the magnitude of the association between living arrangements and depressive symptoms was greater in countries with a higher prevalence of HIV/AIDS. We have clarified that this was the rationale for our approach in the statistical analysis section that discusses the meta-regression.

4. Another major limitation to be acknowledged is that causal interpretation is limited due to the concern of reverse causality and omitted variable bias. Certain living arrangements are likely a result of factors related to depressive symptoms of older adults. For example, adult children with a healthier older parent may be more likely to move out, whereas adult children with parents who are more vulnerable to depressive symptoms may continue to live together or even move in. I understand that the authors do not make strong causal inferences in this paper, but given that they use “estimated effects”, this issue of reverse causality (endogeneity) related to living arrangements and health should be acknowledged as a limitation. The authors also may want to mention the direction of possible bias this endogeneity issue may have caused. This issue was described in Ref. #18, Silverstein and Bengtson (2006), and also examined in a recent paper by Do and Malhotra (2012).


This is a relevant limitation, which we have now mentioned in the discussion section. We also referenced the Do article in discussing how this reverse causality bias might lead to an underestimation of the effects of coresidence on depressive symptoms.

Minor Revisions

Operational definition of whether one has depressive symptoms using secondary data is generally an issue. Be clear about the rationale behind the operationalization the authors used (meeting the three criteria described p. 5) to define the indicator variable of having depressive symptoms. I would suggest moving your citation of Ref. 13 upfront to the Measures-Outcome section rather than only presenting it in the Discussion (limitation) section.
We have made reference to the large multi-country study that used the same measure of depressive symptoms in the methods section.

Review 2:

In p.3 for the Ibadan Study of Ageing in Nigeria, it is better to indicate to mention the factors of depression too.

In p.4, before the hypothesis, the research questions should be explicitly stated.

In p.4, “Data for this study comes from..” please check the grammar of this statement.

We have added information about the risk factors for depression from this study and stated the research questions more clearly at the end of the introduction section.

In p.5, when the authors indicate that “Samples are nationally representative for all countries except Congo and Cote d’Ivoire”, please indicated the proportion of the population from these two countries in relation to the total population.

We have added a sentence stating that the WHS was carried out in limited geographical regions in these two countries due to civil unrest at the time. According to the WHO, the populations were still randomly sampled probabilistically in the areas that were sampled. The issue is that our ability to generalize to the whole population in these countries is limited. However, we do not feel the proportion of the population sampled in relation to the countries total population would be a useful indicator to provide in the paper.

In p.5, the term “Exposure” is confusing with its meaning. Should it be just called types of household?

This has been changed.

In p.6, for the country-level variables, more details are needed on how the authors treated them during analysis. Did the authors assume that the value of a country-level variable is the same for every cases from the same country?

The country variables were only used in the country-level meta-regression, so are only applied in analyses at the country-level. We have added the sentence: “The meta-regression is conducted at the country-level, regressing the country-specific effect estimates on the country-level predictor variables” to make this more clear.

In p.7, the authors first “assessed the relationship between living arrangements and depressive symptoms using separate logistic regression models for each country….”. Please explain what are the specific variables included in each of the models.

In order to facilitate general readers’ understanding the specific statistical tests/tools have to be elaborated with the purpose and function of each of them (e.g. marginal effects, heterogeneity in effect estimates, random-effect meta-analysis etc).

The results section could be touched up by adding more reader-friendly statement to explain the meanings of the statistical test results.

This is a helpful suggestion and we have added further description as to the purpose of the various statistical techniques:
Marginal effects: To facilitate interpretation for all models and to assess differences on the absolute probability scale, we reported average marginal effects calculated from the logistic coefficients. For example, the marginal effect for skipped-generation living arrangement is interpreted as the estimated effect on the predicted prevalence of depressive symptoms of living in a skipped-generation household compared to living in a multi-generational household, averaged over the values of the other covariates in the model.

Random-effects meta-analysis: To obtain an overall effect estimate and because the country-specific estimates lack precision due to small sample sizes, we pooled the country-specific estimates across the 15 countries using random-effects meta-analysis. This approach estimates an overall effect by computing a weighted average of the country-specific estimates. In using the random-effects meta-analysis approach, we assume our sample of countries represents a potentially random sample of all countries in sub-Saharan Africa and does not assume there is a common, homogeneous effect of exposure.

We have changed the 7% to 0.07.

In p.11, the discussion on the potential remittance received by skipped-generation households in Africa is highly confusing. Do these households actually received financial support from government or not should be clarified.

We agree this was not clearly explained and it has been removed from the discussion. We replaced with the sentence “Unfortunately, in our study we were unable to determine the orphan status of children in skipped-generation households or whether the households were receiving material support from absent adult children.”