Reviewer’s report

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

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Reviewer: Young Kyung Do

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This study uses data on 15 sub-Saharan Africa from the World Health Surveys to examine the relationship of living arrangements and depressive symptoms among older adults (+50). The study has several strengths: 1) a welcome addition to the small existing literature on aging and health in Africa within the context of the HIV/AIDS epidemic; 2) attention to depressive symptoms with depression causing a significant, though often neglected, disease burden; and 3) a large sample size with cross-national data. The authors also examine country-level heterogeneity and correlates. Below are comments the authors could consider in revising the paper.

Comments

• Major Compulsory 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.

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.

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).


• Minor Essential 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.
Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: No, the manuscript does not need to be seen by a statistician.

Declaration of competing interests:
I declare that I have no competing interests.