Reviewer’s report

Title: Girl child marriage, socioeconomic status, and undernutrition: Evidence from 35 countries in sub-Saharan Africa

Version: 0 Date: 07 Jun 2018

Reviewer: Arijit Nandi

Reviewer's report:

Review of "Girl child marriage and undernutrition: Evidence from 35 countries in sub-Saharan Africa" (BMED-D-18-00048)

Thanks for the opportunity to review this work. I was requested to review the methods and statistical analyses described in the study and thus I will not comment on the theory/concept or relevance of the question. From an epidemiologic perspective, there are a several methodological decisions that should be reconsidered, summarized under major comments below, followed by point-by-point minor comments.

Major comments

1. I would not present results on the odds ratio scale. Aside from overestimating the risk, it's well known that the odds ratio is a non-collapsible measure of association.(1) This is an important caveat precisely because the authors attempt to compare estimates from models that adjust for different covariates. Because the odds ratio is non-collapsible, it is impossible to discern whether the changes we observe in measures of association across models are due to the addition of controls or the (flawed) statistical properties of the odds ratio. I would prefer to see all models re-estimated on the risk scale (differences or ratios). Though this study is based on cross-sectional data, the exposure precedes the measurement of the outcomes temporally, so I believe the measures can be interpreted as risks rather than prevalence.

2. An equally important limitation to the methods is the technique used to assess mediation (and potential mechanisms) of the relation between child marriage and underweight by early and multiple childbearing, secondary education, and poverty/wealth (the secondary outcomes described in the methods). Examining mediation and estimating the direct effect of child marriage not through these secondary outcomes was not a clear objective of the study, at least as described in the Abstract and Introduction, but it was a key component of the methodological approach. In brief, the study employed the Baron and Kenny method. However, it is well established that this approach provides unbiased results only under very specific circumstances, including the absence of exposure-mediator interactions, non-linearities, and unmeasured confounding.(2, 3) The approach is particularly poorly suited for analyses on the odds ratio scale and, presently, estimates from different models shown cannot be used to infer about potential pathways, as is done in results. If the mediation analyses are to remain a primary aim of the analyses then I
refer the authors to newer advancements in mediation analysis,(4-7) including a text devoted to this topic for epidemiologic audiences.(8)

Minor comments

1. In the Participants section the manuscript should report the initial sample size and the number and percentage excluded for each of the inclusion/exclusion criteria. An alternative would be to include a flow diagram showing the selection of the sample.

2. In several instances, including the abstract, estimates should be rounded; although the sample is relatively large, three decimals is an unreasonable level of precision to report.

3. Unless I missed it, it is unclear whether the regression results are weighted by the DHS sampling weights or not. If weights were not properly accounted for then the findings are generalizable only to the study sample, not to the populations that they were sampled from. This issue should be clarified in the paper.

4. Were heights and weights measured or reported? Please clarify.

5. I am concerned that primary school completion is being measured after child marriage for some observations, since students might be enrolled until after age 15 in some cases, given the school structure in sampled countries. In this case it shouldn't be a confounder since it might be on the pathway between child marriage and the outcome. An alternative would be to adjust for any schooling, depending on the distribution.

6. There is the strong potential for unmeasured confounding by early-life socioeconomic circumstances, early-life nutrition, religion, etc., which are not addressed by the EA fixed effects and this is not sufficiently discussed.

7. A very minor point, but I would not refer to analyses of severely underweight as a sensitivity analysis since these analyses do not test the rigor of the main results.

8. This is unclear to me (and perhaps others): "To check for measurement error, we ran analyses excluding women married at ages 18 and 19 to see if associations were present controlling for more extreme ages of child and adult marriages."

9. I was curious whether there was enough variation in child marriage and outcomes within EA's? Can this be assessed and reported? Relatedly, how much data is lost by including these fixed effects?

10. I think the benefits of this approach ("By focusing on within-EA variation, we eliminated potential biases due to differences in observable and unobservable factors common to all women in these geographic areas, including local population density, infectious disease environment, poverty levels, availability of food and nutrients....") are somewhat overstated. Though comparing within clusters, exposed and unexposed might differ in
terms of age cohort. Age was controlled in analyses, but I suspect there was substantial extrapolation given the small sample within EAs.

11. If I understood this correctly, I would advise against using dummy variables to prevent observations from dropping in analyses including age at first birth, age gap with partner, etc. Have the authors considered two-stage models as an alternative?

References cited


Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

No

Does the work include the necessary controls?
If not, please specify which controls are required in your comments to the authors.

No

Are the conclusions drawn adequately supported by the data shown?
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No
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