Reviewer’s report

Title: Magnitude and factors associated with Intimate Partner Violence in Mainland Tanzania

Version: 0 Date: 20 Oct 2015

Reviewer: Tia Palermo

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Review of "Magnitude and factors associated with Gender Based Violence in Mainland Tanzania"

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The authors use baseline data from an impact evaluation of the study CHAMPION'S MAP (Men as Partners) to examine prevalence, perpetration, and risk factors of intimate partner violence (IPV) among women. The innovation of the study is limited, as estimates already exist on prevalence of intimate partner violence in Tanzania, including from the WHO Multi-Country study on Women's Health and Domestic Violence (conducted in Dar es Salaam and Mbeya), as well as the most recent (2010) Demographic and Health Survey, which is nationally representative and the current study is not. However, I see two strengths of the current paper which may warrant publication: 1) the current study has a great number of districts surveyed as compared to the WHO study, from which most existing peer-reviewed, published estimates are drawn and 2) the current study reports on women's perpetration of IPV.

Main concerns:

1. I find the language used to refer to violence to be problematic. The authors are only examining violence perpetrated by partners and should thus use the term intimate partner violence (IPV) instead of their current language, gender-based violence (GBV), which may represent violence experienced from a multitude of perpetrators.
2. The introduction should be expanded, and particularly, the third paragraph should have a less abrupt transition, and start with discussion of the ecological model of intimate partner risk (Heise, 1998), then drill down to individual-level risk factors.

3. Some of the interpretations of existing literature are problematic.

   a. For example, citation of the global prevalence of GBV as 70% (reference #2 in the manuscript) is really the upper bound found in the WHO study, and global prevalence of IPV is generally agreed to be approximately 30% (Devries et al., 2013).

   b. Also, the interpretation of the DHS estimates are also incorrect. The estimates of physical IPV (39%) is "since age 15" and not "by the time they were 15 years" as reported by the authors.

   c. In addition, in reference to the DHS estimates, I do not know where the authors obtained the 33% for sexual GBV. My interpretation of the DHS report is that 20% of women reported sexual violence (from any perpetrator, not just IPV—page 274 of DHS report, row "Total").

   d. Study design: the authors do not make clear why this is referred to as a "quasi-experimental" study. The existence of treatment and control arms, if randomized, would indicate a "randomized control trial", or cluster randomized control trial if randomized at the village level. Further, if the arms were not randomized, then indeed the study does not have "controls," but rather a "comparison group." In any case, they should clarify the study design—a matched design may indicate quasi-experimental.

4. The regression methodology is not satisfactory. The authors should control for age, education, and marital status in their regressions simultaneously.

5. P5, lines 56-58: I do not understand what the authors mean when they say that emotional violence was not "statistically significant different from other forms."
6. P6, line 20: The wording related to women's perpetration is too strong. In fact, 7.6% is low prevalence of perpetration, and they should reword to say that "There is some evidence to suggest that women may also perpetrate violence against their partners, but rates are low." The discussion should compare these findings with those from Uganda and Ghana on women's perpetration (Kishor & Bradley, 2012).

7. Table 3 should be re-worked as a tabulation to examine prevalence of perpetration by exposure to different types of violence, but experience should not be used as a predictor. Then authors can provide a Table 4 which examines all risk factors together in one multivariate logistic regression (i.e., controlling for age, marital status, education).

8. Controlling for IPV experience in a regression with IPV perpetration as the outcome is problematic due to endogeneity and simultaneity bias. That is, many of the same factors that drive experience of IPV may also predict perpetration (and are not controlled for here), and furthermore, experience of violence may influence one's acceptance of violence and thus likelihood to perpetrate in retaliation. Thus estimates of the coefficient on IPV experience in the IPV perpetration regression will be biased, and the control variable should be removed from this regression.

9. There is no discussion of whether standard errors were adjusted for cluster sampling at village level. They should be adjusted.

10. P7: The discussion should situate these findings within the context of existing estimates (DHS and WHO study) from Tanzania.

Minor points:

1. Why are there no page numbers? This makes it very difficult to reference text in my review.

2. P1, line 50: the authors' meaning of "disregards age, education…" is unclear. Do they mean the relationship is not significant?
3. P1, line 56: language "controlling GBV" is awkward. Do the authors mean "preventing" or "reducing"?

4. P2, line 56: wording of "should have been partnered" is awkward. Change to "eligibility criteria included having been ever-partnered."

5. P3, line 28: wording "it was put clear" is awkward.

6. P4, line 4: change "even before the partner abusing them" to "when the partner was not already abusing them."

7. P4, line 48: remove "binary" before logistic; logistic regressions are assumed to have binary outcomes unless the authors are referring to "bivariate" regression, where they only control for one right-hand side variable at a time.

8. P4, lines 57-58: Change sentence "The strength of the association…" to "Level of significance used was alpha = 0.05."

9. P5: the sections on ethical consideration should be combined with "Process" on Page 3.

10. P5, line 51: Change "reported to have" to "reported having".

11. P6, lines 15-17: change wording of "border-line".

12. P6, line 59: This sentence is unclear "GBV being a public health problem"

13. P8, line 56: reword "give the best data" to "minimize underreporting".

References


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?
If not, please explain in your comments to the authors.

Yes

Are you able to assess any statistics in the manuscript or would you recommend an additional statistical review?
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I am able to assess the statistics

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Please indicate the quality of language in the manuscript:

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