Reviewer’s report

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

Version: 1 Date: 16 Mar 2016

Reviewer: Tia Palermo

Reviewer's report:

The authors have generally done a good job of addressing my previous comments, however I still have the following comments.

Major comments

* Expand discussion of the ecological model in the introduction. Authors say that the model has different levels but need to clarify/expand that in fact there are "risk factors" at each of these levels, and that interact with each other.

* Page 4 talks about how a previous study finds no associate with age, education and size of household with IPV, but this is based on only one study and authors should discuss other studies, including at the least:
  

* Page 4, line 26: cite WHO study when referring to studies with only limited regions (however it should be noted that while the current study does have more regions, it's still
not national representative and in fact the current study has a much smaller sample size than the WHO Tanzania study). Further, on line 31, authors should remove "larger sample" and change to "more geographically diverse sample".

* Measures, Page 7, line 7: list out background measures collected.

* Page 8, lines 14-19: "on the one hand" and "on the other hand" is confusing. Please clarify. Perhaps a better way to state is that they ran two different regressions, one examining association of background characteristics with experiencing IPV and the other with perpetrating IPV.

* Totals of women experiencing IPV is not presented in any of the tables. Add to either Table 1 or Table 2—the latter has IPV experience by category, but no totals—what is the total % of the sample reporting different types of IPV?

* Results section: Discussion of results should be expanded, particularly discussion of Table 3.

* Discussion:

  o Compare existing findings of associations with IPV experience to the Abramsky paper above.

  o need to add a line in limitations about small sample size and power. There is a possibility that no relationships were observed in tables 2 and 4 as hypothesized because the study is underpowered to detect these relationships, and not because they don't exist.

  o The interpretations of some of the existing literature are at times misrepresented. For example, on page 13, lines 42-43 the authors report that DHS only reports GBV and not specifically IPV. This is not true (see tables 16.9-16.12 in latest Tanzania DHS 2010).
Further, the WHO study is much larger than the current study and the authors claim that theirs is larger. The WHO study interviewed 4,021 women in Tanzania.

Minor comments

* Page 3, lines 9-11, remove "even nuisance behavior" as the term is vague.

* Page 3, lines 14-16, change "as long as these acts come from" to "perpetrated by"

* Page 3, line 51: change "suggested and recommended" to "developed"

* Page 5: remove "larger" from line 17.

* Page 8, line 14: odds ratios are not a test; remove "the odds ratio" and just keep chi-squared and logistic regression.

* Page 10, lines 22-28: this discussion of what asked and how should be moved to methods, not results.

* Page 13, line 14: change "retard" to "hinder"

* Page 13, line 48: add range of IPV rates from Tanzania from WHO study (physical IPV is 33-47% and sexual is 23-31%)

* Page 14, line 47: if authors found no statistically significant relationships, how is this "similar" to the ecological model?
Are the methods appropriate and well described?
If not, please specify what is required in your comments to the authors.

Yes

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

Yes

Are the conclusions drawn adequately supported by the data shown?
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