Reviewer's report

Title: Does exposure to interparental violence increase women's risk of intimate partner violence? Evidence from Nigeria Demographic and Health Survey

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Manuscript Review

Title: Does exposure to interparental violence increase women's risk of intimate partner violence? Evidence from Nigeria Demographic and Health Survey

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This paper examines the association between exposure to interparental violence and involvement in intimate partner violence (IPV) in a sample of women from rural regions in Nigeria. Strengths of the study include the ability to examine these patterns in a large sample from Africa, a well written introduction, the inclusion of key information about the societal and cultural context, and inclusion of several covariates. The discussion is also very well-written. Despite these strengths, I have numerous concerns that focus mainly on wondering why the authors did not differentiate types of intimate partner violence but instead lump them all together (I see this as a major weakness and hindering substantial contribution to the literature). I also feel the authors have not adequately reviewed the extant literature on prospective research that already links exposure to interparental violence and involvement in IPV. It's not clear what this study is adding, beyond that it was conducted in rural, under-developed country in Africa with a large sample. The authors need to more clearly justify the ability of the study to make a novel contribution to the existing literature. These concerns and others are elaborated below.

1. In the results section of the abstract it's not clear what the differences are between models 1-3. I would suggest either clarifying this or taking out this particular section from the results. This level of detail is not necessarily needed in an abstract.

2. The first paragraph of introductions states that although IPV has been reported against men it is universally evident that they main perpetrators are men and women are the key victims of IPV. This statement is not accurate. There are several studies (perhaps
particularly from the US), which suggest that particularly in community samples, IPV tends to be reciprocal, with both men and women serving in the perpetrator and the victim role (e.g., Ehrensaft et al., 2003; Fergusson et al., 2008; Langer et al., 2008; Magdol et al., 1997). It is indeed true that more severe forms of violence such as physical and sexual battery that involves assault and injury tend to be more perpetrated by men. However, it is not the case that women are always the victims. There are several studies to suggest that women also perpetrate violence sometimes at equal rates as men. I suggest the authors reword the above statement and also include more balanced citations. Alternatively, they could make the case that SEVERE violence is more likely to be perpetrated by men, with appropriate citations that have found this.

3. On page 4, I also find that the others are missing a lot of citations for studies that have previously found support for the intergenerational transmission of violence, particularly from witnessing interparental violence to being involved in intimate partner violence. Several prospective studies have already found these patterns (e.g., Ehrensaft et al., 2003; Fite et al., 2008; Narayan et al., 2013, 2014). The authors need to more thoroughly review the literature before making their claims. Furthermore, as noted with the citations, several studies have already documented prospective associations, however, the present study is not prospective. Therefore, the authors need to justify what the present study adds to the literature. Obviously, it is very interesting that it is done in a country such as Nigeria; this is a major strength. The authors need to elaborate on this.

4. The Intro starts out with a nice definition of intimate partner violence, however, it then lumps together many different forms of intimate partner violence, such as physical and psychological and sexual. There is a wealth of literature that already suggested pathways to each of these types of IPV are different. The authors need to include the citations and review the literature more thoroughly on this topic. They also need to clearly justify why they have collapsed all three types of IPV into one outcome variable. With such a large sample here, it would make more sense to me, and it would more strongly contribute to the literature, if they were able to keep these types of IPV differentiated as separate outcomes.

5. The Introduction is lacking hypotheses.

6. Relatedly there are also a lot of covariates included in the models described in the Method, many of which would be theoretically expected to be associated with IPV, such as attitudes towards aggression, whether the partner drinks alcohol, etc. These constructs
need to be incorporated into the Introduction with justification based on past empirical work, about why these variables are included.

7. The part on Context in the first part of the Method section should be incorporated into the Introduction. This section provides key information on the context that the participants live in, and the sociopolitical climate. This is very interesting information; however, it should be in the Introduction. The Method section should start with the section on Data Source and Sample Design.

8. It's unclear on page 7 how exposure to interparental violence was measured (e.g., how many questions/items were used, what the age range of exposure included, etc.).

9. It would be helpful to have information on who administered the questions about exposure and IPV. Were the research staff who administered the questionnaires culturally matched with the participants? Do the authors feel that the participants were provided a safe and confidential environment to answer these personal questions? I wonder whether there were reporter biases operating here. Are there any cultural stigmas associated with acknowledging violence in the home that may have contributed to under-reporting of exposure or IPV?

10. Several major limitations exist, which are not acknowledged in the Discussion section. First, the authors have only relied on a few items to access the key constructs. It seems as though exposure to violence was only assessed by one item, however this is unclear. Second, it is unclear whether validated measurements of these constructs were used - and were they found to be valid/reliable for use with Nigerian women. Third, both the independent and dependent variables were drawn from the same reporter. This increases the possibility of a reporter biases. Furthermore, the authors need to explain whether or not they think that women were likely to under-report rates of exposure to violence and involvement in IPV because of social or cultural stigmas. Finally, I see the inability to differentiate the different types of IPV (physical, psychological, sexual) as a major limitation that needs to be acknowledged, and I also see the focus exclusively on male-to-female violence (both in terms of exposure - what the women saw - and involvement in IPV) as a big limitation.
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?
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Yes

Are the conclusions drawn adequately supported by the data shown?
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