Reviewer's report

Title: Intimate partner violence against women in rural Vietnam: different socio-demographic factors are associated with different forms of violence. Need for new intervention guidelines?

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Reviewer: Amy Bonomi

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Amy E. Bonomi
Re-review of: Nguyen Dang Vung, Per-Olof Ostergren and Gunilla Krantz
BMC Public Health
Intimate partner violence against women in rural Vietnam: different socio-demographic factors are associated with different forms of violence. Need for new intervention guidelines?

I have had a chance to review this manuscript and feel the authors were responsive to most of my concerns; thank you. I still have the following concerns:

1) The manuscript would still benefit from editing. Perhaps this can be done at the level of BMC’s copy editor.

2) In the introduction, you mention U.S. studies which have examined IPV prevalence and overlapping IPV types, but you do not provide references. In my previous review, I gave you a couple of references which you should consider citing, along with others that may appear in the literature. Also, in your discussion, you compare your results to studies from South Africa, but not the U.S. studies. You might want to include a line or two about the U.S. studies since you mention them in your introduction.

3) In the abstract, please give the prevalence estimate for psychological abuse.

4) I believe it would be most helpful if the information about the Bavi district (currently appearing in the Intro) was moved to the methods section. I like the discussion of changes occurring in Vietnam as it currently appears in the Introduction, but the Bavi piece seems to come out of nowhere. Alternatively, you might consider keeping your discussion of the changes in Vietnam in the Intro, and once you state your study objective, mention that the study was done in Bavi and very briefly include the description of Bavi there.

5) I appreciated your addition of language addressing power calculations. I didn’t see where you added information on your response rate, as I previously requested. (Apologies if I missed this.) If you are unable to estimate a response rate because of the nature of your sampling method, please say so in your paper.

6) I did not see where you addressed my previous concern that women who experience less severe forms of IPV may be more likely to participate in your
survey than women who experience more severe forms. I provided you with a reference to Michael Johnson’s article, suggesting that women who experience more severe violence may be less likely to participate in surveys than women who experience less severe types. Also, in your discussion section, you mention the issue that the interviewers could have experienced violence, which seems less concerning than the potential participation bias that could result based varying severity levels of violence.

7) I am still concerned with your suggestion that an “accurate” estimate of IPV is possible. If you mean to state that recall bias may be less of an issue if you are asking women about past year violence compared to violence that may have occurred before the past year, then please provide references to studies that suggest that recall of experiences in the distant past is problematic and give the reasons why. There is literature in this area that should be cited.

What next?: Accept after minor essential revisions

Level of interest: An article of importance in its field

Quality of written English: Needs some language corrections before being published

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.