Reviewer’s report

Title: Intimate partner sexual violence and risk for femicide, suicidality and substance use among women in antenatal care and general out-patients in Thailand

Version: 0 Date: 25 Nov 2016

Reviewer: Tina Bloom

Reviewer’s report:

Overall, this manuscript is an interesting contribution to the research literature, and the authors have done very good work - they are off to a great start writing up these results. We think this project has many strengths and that the paper will be of great interest to this journal's readership! Most of our comments and suggestions are relatively minor, however, we do have some thoughts on how to better contextualize the study and its findings within the extant IPV literature. We hope our comments are helpful to strengthen the manuscript.

Title: the title seems unclear about other types of IPV. However, if you use "Sexual assault and intimate partner violence among women in antenatal care and general outpatients in Thailand" would be fine.

Abstract is largely complete, succinct, and clear, but a few notes:

* should include the study design; it would be cross sectional design?

* And please include the measures in the abstract (AAS, SVAWS, Danger assessment, Suicidal behavior).

* In addition, for the word of "intimate partner violence" should be consistency because sometimes the authors used "Intimate violence partner relationships".

* Please check typo in word "fiftyseven" - needs a hyphen to separate the two word parts.

Literature review: A bit thin; it needs some more information.

* For example, it is not insufficient information about IPV in worldwide and the important of IPV and sexual assault study in Thailand and to women's health. The authors showed the information and the prevalence of IPV in the previous studies that conducted in Thailand, but they did not present the literature gap and why this study is important.

* Furthermore, the aim of study focus to sexual assault in women, but the information need more clarify - why this is important or useful.

* Finally, the second paragraph of introduction at "Globally" can move it to the first paragraph.
However, the previous results in Thailand about the prevalence of IPV are concrete and helpful.

Methods: The method part needs some revises.

* It lacks a description of study design (cross sectional study?)

* Were healthcare providers trained to do the screening? Do they screen all women? Was there a standard protocol for the screening? Was there any assurance that providers were actually doing all the screening? Were women always screened in private?

* Secondly, the inclusion criteria for choosing the subjects are not clear: the women that chose from antenatal clinic are pregnant women or any women? (any age parameters) Any exclusion criteria? (do they choose some participants that they have psychological disorder or any problem in mental health).

* Measures are generally well-chosen and translation procedures are described - however, was there any validation or pilot work done with the translated measures? (If not, this should be described later in the discussion as a limitation).

* Not clear why Alcohol Use Disorder Identification Test-Consumption was included - please describe the rationale.

* Are the measures of socioeconomic status previously validated? (i.e., the measure which has response options like "We have most of the important……and luxury goods")

* Really appreciate that the authors used the Danger Assessment instrument in this study - the importance of this type of risk assessment cannot be understated in my opinion - and it has clinical and safety planning implications. That said - for future work - the authors will want to know that the 15-item version of the DA that they used is quite old. The more recent and better-validated version of the DA has 20 items. There is also a 5-item brief version of the DA developed in 2015, for use in clinical settings (see https://www.dangerassessment.org/uploads/DA-5_2.26.15.pdf). I think perhaps the best way to handle this issue is in the discussion -- to acknowledge that there are newer versions of the DA, but that the 15-item version was previously well-validated, and that it demonstrated that some of these women were at significant risk - and perhaps to refer the readers to the 5-item version for use in clinical settings.

* In data analysis part, the data is normal distributed or not because you wrote some sentences that for "non-normal distribution, non parametric test were used", please clarify more about because for the table 3 you wrote Mann-Whitney U test which is non parametric test. For the multiple regression that you used for predicting the associated, could you check it again for the assumptions of regression? Although normality is not the major assumption to test for multiple regression, it has to concern because your data is not normally distributed. However, if it is not normally, sometimes you can claim it from the size of sample.
* In addition, it is confusing that how sexual assault and danger come topic come from because looked at the Table 3, it seems like descriptive analysis, how the authors got Odds ratio and CI, this comes from regression analysis?

* Finally, it would be great to analysis and clear if the authors can show the results that separate from participants from antenatal clinic and general out-patient. We realize the numbers are too small to present statistical subanalyses by these clusters, but it would be useful if the text in the results began with some descriptive results of where participants came from (e.g., how many from which settings).

Discussion: In discussion part, it needs more discussion about the results.

* For the first paragraph that discussed about the lower rate of sexual assault of current study than previous study, it would be informative and stronger if the authors can find more studies to discuss, and another variable or any factor in demographic data that can explain about this such as socio economic that seems high.

* In second paragraph, for the last sentence that " this finding highlights the important of mental health management…. Of sexual assault" It seems not clear for who is managing or manage to who.

The most problematic issue related to interpreting the findings of this study is the extremely low prevalence of IPV (0.02%) - but this is an important finding in and of itself.

* This should be talked about in the discussion.

* Because the prevalence found in this study was so, so low, and so very different from other research studies, it is most likely that the actual prevalence rate is much higher than 0.02% -- and this should be clearly stated. Since the healthcare providers were in essence the "gatekeepers" doing all the screening -- it seems likely that what was happening here that resulted in low prevalence is that women were not disclosing IPV to the providers - and this should also be clearly stated.

* I think that it also makes it very hard to assume that the women that DID disclose IPV were typical of abused Thai women - it is a skewed sample. This should also be clearly stated and should be addressed in the discussion section as a limitation.

* BUT it does suggest that these providers were not doing the greatest job screening patients and/or that there are significant barriers to Thai women to disclosing IPV when asked in a healthcare setting (it's probably both). This is actually a very important finding in and of itself.

* There is a large body of research that demonstrates that healthcare providers don't routinely screen for IPV, are not comfortable asking about it, are not well-trained about IPV, and often have negative attitudes and stereotypes about IPV. These findings should be grounded in this literature.
There is also relevant research regarding women’s barriers to disclosing IPV to healthcare providers and others - which should also be worked into the discussion.

It is not clear if - when the authors talk about Buddhist principles/Thai cultural factors at the start of the discussion - do they mean that these factors might have kept women from disclosing violence? Or from experiencing IPV in the first place? Please include recommendations for future research, policy, and practice based on the findings.

Conclusion: The conclusion needs revise again by including the rational problems and important results and future research. The authors wrote about "the study found the moderate rate of sexual assault in intimate partner violence, but it has not seen where the results show the moderate rate of IPV. The authors also wrote the helpful of study but lack of the future of research.

Minor/grammatical: The consistency of terms related to intimate partner violence is needed because sometimes the authors used intimate violence partner relationships in study.

English is quite good overall, but there are some typographical errors here and there.

Another thing to revise and update is the references in discussion.

Sincerely,

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Are the methods appropriate and well described?  
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