Author’s response to reviews

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

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Reviewer reports:

Tina Bloom (Reviewer 1): Title:

The authors said they changed the title per reviewer feedback, but it does not appear actually changed.

Response:

The previous comment was “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”

Therefore, the original title “Sexual assault and other types of intimate partner violence in women in antenatal care and general out-patients, Thailand”

Was changed to what was recommended: “Sexual assault and intimate partner violence among women in antenatal care and general out-patients in Thailand”

However, we feel, as below, should be a better title:

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

Abstract

Appears fine
Background

Would be OK to use abbreviation of IPV represent Intimate Partner Violence (IPV) after the first usage.

R: Corrected

It would be easy to follow if the authors will reorganize the outline of background. For example, firstly, we can start IPV refers to ……. Secondly, in studies in high income countries….. Thirdly, According to WHO in Thailand….. Fourthly, there is lack of studies….. Finally, the aim of study.

R: Changed accordingly

Method

Measures description would be consistency if the authors can interpret the level of score in each tool, some tools interpreted, some tools not.

R: This is added

In Data analysis part, authors use 95% confidence interval, but they wrote in abstract and table is 99%.

R: There is no 99% in the abstract and there is 95% confidence in the tables

The justification for using the outdated 15-item DA is not particularly strong, but it will do.

R: OK

Discussion

Is improved

Table

Table 1,2,3,4, it will make it easy and strong table if the authors can put P<0.005 and put * to indicate significant.

R: We disagree, the actual P-values are sufficient

Conclusion

It is fine.
Sheila Sprague, PhD (Reviewer 2): Overall this is an important area of research that is relevant to BMC Women's Health. However, the extremely low prevalence rate identified amongst participants compared to rates identified in other studies challenge the validity of study results.

Most of the peer review comments have been addressed, however, a few were not addressed. There are still inconsistencies in the language used for sexual violence and intimate partner violence. I strongly suggest that the authors use consistent terminology throughout the manuscript for clarity. Additionally, there are still numerous spelling and grammatical errors throughout the manuscript. The authors may wish to consider enlisting the services of a copy editor or academic writer to review the manuscript for spelling and grammar, as well as scientific writing style.

R: Corrected

Additional Comments:

Abstract - The background section of the abstract does not include background information and only includes the objective of the study.

R: Below is added

Background: Little is known about the occurrence and health consequences of intimate partner sexual assault. The aim of this study was to assess the prevalence and correlates of sexual assault in the context of intimate partner violence (IPV) in Thailand.

This is also a run on sentence that should be made into multiple sentences for clarity.

R: Corrected

Background - What are the separate rates of physical and sexual IPV reported in references 9 and 10?

R: References [9] and [10] refer to the method section.

If the reviewer is referring to below

“Globally, in studies (as reviewed in [7]) that measured sexual assault separately from physical violence, a high proportion of women (40-68%) had experienced both physical and sexual violence.”

This is to show that a high proportion of women who experience physical assault also experience sexual assault.

Methods - How were participants systematically screened? Was every potentially eligible participant screened? The authors should address these questions in the manuscript.
All women who presented to a study site for a health care visit were approached by a recruiter to determine eligibility.

Willingness to provide informed consent should be listed as a separate 4th inclusion criterion.

Was the pilot study that you mentioned to assess the face validity of questionnaires used published? If so this should be referenced, or if not please state that this is unpublished data.

The SVAWS has been pilot tested for face validity in Thailand before [12].

Please specify what item was deleted from the DAS scale and why.

One item dealing with sexual assault was deleted from this study, since this was assessed in the SVAWS.

Results - The authors state that 212 women out of 14288 screened positive for IPV and list a prevalence rate of 0.02%. The percentage from these numbers should be 1.5%.

In the multivariate logistic regression section the authors state that high physical violence and suicidal behaviours are "marginally significant", however, the p-values are greater than 0.05 and therefore findings are not statistically significant. This should be deleted or revised to say "approached statistical significance". This should be corrected throughout the paper.

Discussion - Is it ethical to conduct IPV screening in settings that aren't private?

We do not see anywhere mentioned IPV screening in a non-private setting.