Reviewer's report

**Title:** Work-related violence and inconsistent condom use with non-paying partners among female sex workers in Adama City, Ethiopia

**Version:** 1  **Date:** 6 April 2013

**Reviewer:** Souradet Shaw

**Reviewer's report:**

1. **Major Compulsory Revisions**

Thank you for the opportunity to review this paper. This is a well-written and well-researched paper, presenting novel information on an important topic in an under-researched vulnerable population. However, I have two main issues with the paper, which I feel the authors need to address in order for the paper to be acceptable for publication.

- First and foremost, the model-building process for the authors’ final model needs to be more clearly articulated. It is essential for the authors to not only describe the choices behind initial variable selection, but how they arrived at their final model, especially in light of their findings re: alcohol abuse not being associated with inconsistent condom use once violence was factored in. As well, given that a structured questionnaire was used (and not clinical data, for example), Table 1 contains relatively few variables. Some more information on the questionnaire would be useful – what types of questions were asked, and why were important questions, such as frequency of sex work and type of client not included in the analysis? The authors do include total numbers of sexual partners of any type was included, but it is curious that no questions on frequency of sex work vs. sex with non-paying partners were included.

- Second, and this is somewhat related to the first point, the authors start off their Introduction by discussing the syndemic of alcohol and violence and how through their synergy, HIV risk is produced. They talk at some length about the interplay between these factors, and state as one of their objectives, their desire to investigate the effects of both alcohol and violence on inconsistent condom use. However, I am not completely convinced that the model and the results from that model adequately addresses this “synergy”. Certainly, the authors’ final model does point to an association between (unspecified) work-related violence and inconsistent condom use; however, very little is learned about the impact of alcohol on this relationship, as their methods do not seem to suggest that this was actually a planned analysis.

Unless I am misunderstanding their results, all their final model seems to suggest is that work-related violence is associated with inconsistent condom use, and that this relationship remains significant even after adjusting for alcohol abuse, and other variables they have included in their model. In fact, from what can be seen in Table 2, alcohol abuse is not significantly associated with condom use at both
the univariate level and in the final model. Without knowing how the authors built
their models to formally assess the interplay between violence, alcohol and
condom use, I think the statement in their Conclusion “Alcohol abuse was not
associated with condom use after controlling for violence…” may be slightly
premature. If the authors do think it is worthwhile to pursue the “syndemic” angle
between violence and alcohol on HIV risk, I would suggest either stratification by
alcohol abuse at the very least in their analysis (incl. in their regression models),
or using something like mediation models (see the classic work of Baron and
Kenney; for binary outcomes Stata has a user-written .ado program:
http://www.ats.ucla.edu/stat/stata/faq/binary_mediation.htm) to get to a more rich
understanding of this relationship. Although the authors mentioned that the
interaction between alcohol use and violence was not significant, one wonders if
the study was adequately powered to detect such an interaction. More details in
how this interaction was tested, and what the results were, would be essential.

At a minimum I would recommend including a 2 x 2 table which looks at how
violence and alcohol abuse are inter-related, as well as the level of inconsistent
condom use within each of the four cells of the 2 x 2 table. I think using simple
descriptive tools like this would lend more weight to their findings of whether or
not alcohol abuse is important in the context of workplace violence. Additionally,
a clearer sense of how confounding/mediation/moderation was formally
assessed in the model building phase would be integral to the interpretation of
their findings.

Otherwise, as stated, this is just a study on violence and HIV risk. Which is still
worthwhile, given the findings, and the lack of data on this particular population,
but the present findings probably need a different approach in both the
Introduction and the Methods than what is currently included in the authors'
paper.

• Pg. 11, 1st Parag., 3rd sentence: “Table 1 displays the demographic,
socioeconomic and interpersonal characteristics of the study participants,
comparing the alcohol abuse group to the reference group.” – I think this is
improperly referenced, as Table 1 only shows the characteristics of the entire
sample, without stratification by alcohol abuse. This needs to be fixed.

2. Minor Essential Revisions

• Minor point, but I think in the Introduction authors need to add in a sentence or
two about whether the literature they cite, and the relationships between alcohol
and sexual risk-taking pertain to FSWs in the developed world, the developing
world, or both. I think some statement here about heterogeneity in the
environments of FSWs globally would be useful.

• Intro, pg. 5, 1st parag, 3rd sentence – some explanation of what the authors' working
definition of non-paying partner would be essential here. The authors also
neglect to define what definition of “non-paying partner” they used in the
analyses – was it a specific question on their questionnaire? If so, how was it
phrased? Given the fluidity in how clients vs. non-paying partners are defined,
this would be important information to know.

- Would change (on pg. 5), “While no study has investigated…” - see Deering et al. “Violence and HIV risk among FSW…”; STD. 2013. 40:168-74. Here the authors found no significant association between ICU and violence in NPP partnerships, but a positive relationship between ICU and violence in regular and occasional clients. Would be important to mention, and then phrase this sentence to something along the lines of “There is a paucity of data on the links between workplace violence, and HIV risk within non-paying partnerships in the developing world. One recent study found no link between inconsistent condom use and violence in non-paying partnerships, while observing a positive relationship between ICU and violence in regular and occasional sex work relationships [Deering et al.]. However, other literature suggests the importance of lifetime gender-based violence…etc. “

- Pg. 6, 1st sentence - would suggest being careful with interchangeably using the words “intimate” and “non-paying” partner; one does not necessarily mean the other.

- The authors should clarify whether written informed consent was obtained, and if not, what other consent process was used, and what were the procedures used to document this consent.

3. Discretionary Revisions

- For those variables reported as means in the Results sections (such as age, sex work duration and income), it would be informative to have a measure of dispersion – such as interquartile range.

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Acceptable

Statistical review: Yes, but I do not feel adequately qualified to assess the statistics.

Declaration of competing interests:

I declare that I have no competing interests.