Reviewer's report

Title: Sexual Violence against Female Sex Workers in The Gambia: A cross-sectional examination of the associations between victimization and reproductive, sexual and mental health

Version: 2 Date: 24 July 2014

Reviewer: Elizabeth Comrie-Thomson

Reviewer's report:

Thank you for your clear and thoughtful responses to my previous comments, which I think have been well integrated into the revised manuscript.

Please note that while my comments have been addressed, I also indicated that in my opinion the manuscript should undergo statistical review, and I am uncertain whether or not a statistical review has been undertaken. I am therefore including two queries relating to the statistical analysis as Major Compulsory Revisions below. Please note that I am not a statistician, and think that the manuscript should undergo statistical review irrespective of the authors' responses to my comments.

Major Compulsory Revisions:

1. It is currently unclear whether a clustered analysis was conducted. Given that chain-referral and venue-based sampling were used, my understanding is that a clustered analysis would be appropriate. I suggest that the authors clarify whether this was done, and, if it was not, seek statistical review to confirm whether or not a cluster analysis is needed. Again, please note that I am not a statistician and do not feel qualified to fully assess this or provide recommendations.

2. Some of the variables in Table 1 (number of children, age at entry into sex work, and years in sex work) are unlikely to be symmetrically distributed. As I understand it, this means that these variables should probably be reported using median (IQR) rather than mean (IQR), and should be tested with a nonparametric test (e.g. Wilcoxon rank sum test). Once again, apologies for my lack of clarity on this as I am not a statistician.

Minor Essential Revisions:

3. It would be useful to spell out 'ART' in 'government ART clinics' in full (Methods, para. 2).

4. Since the primary exposure is 'forced sex by a client', I assume that the prevalence estimate reported at the beginning of the results section (Results, para. 1) is of forced sex rather than the broader outcome of sexual violence. I would suggest clarifying this.
5. I note that the authors have included a mention of depression among WRAs in the general population (Discussion, para. 3). However, please clarify if 'depressive symptoms' was measured in the current study in the same way as 'depression' was measured in the study in the general population by Coleman et al (reference 61). If different measures were used, then please discuss the degree to which these measures are comparable – or, if they are not comparable, then do not directly compare them. The prevalence of depressive symptoms below the threshold of clinical relevance will always be higher than the prevalence of clinically significant depression, and the two measures are not directly comparable.

6. Please conduct a close proofreading for minor typographical and grammatical errors. For example, I assume that 'those to tested HIV positive' should read 'those who tested HIV positive' (Methods, para. 2).

Discretionary Revisions:

7. Suggest changing 'clients of community-based organizations' to 'members of CBOs' to avoid confusion with clients of sex workers (Methods, para. 1).

8. You may want to replace the section heading 'Outcomes' with the heading 'Study Measures', in order to avoid confusion about whether results are being reported in this section.

Level of interest: An article of importance in its field

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.