Author's response to reviews

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

Authors:

Jennifer A Sherwood (jenifersherwood@gmail.com)
Ashley Grosso (agrosso@jhsphs.edu)
Michele R Decker (mdecker@jhsphs.edu)
Sarah Peitzmeier (speitzme@jhsphs.edu)
Erin Papworth (epapwort@jhuccp.org)
Daouda Diouf (dioufda@endatiersmonde.org)
Fatou M Drame (fatou-maria.drame@ugb.edu.sn)
Nuha Ceesay (ceesayn@unaids.org)
Stefan Baral (sbaral@jhsphs.edu)

Version: 3
Date: 21 January 2015

Author's response to reviews: see over
Response to Reviewers: Version 2

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

Referee 1: Elizabeth Comrie-Thomson

- **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.

**Thank you for requesting this clarification.** The current study was not designed for clustered data collection, and so did not collect data on which individuals were recruited from what venue or by whom. Without these data, it is not possible to conduct a clustered analysis. It has been noted in our limitations section that due to the lack of data on where and by whom each participant was recruited, it is not possible to conduct a clustered analysis, which may have influenced the results of the study. Although the current data collection methods are non-random, and may not be fully generalizable, initial participants were selected based on diverse characteristics in an effort to reach a range of FSW.

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.

**Thank you for prompting this review.** After seeking statistical clarification, we have reported the median (IQR) and have used the Wilcoxon rank-sum test to evaluate the differences between groups for the requested variables, so as to not violate the assumption of normality associated with the t-test. All of the differences between the groups remain non-statistically significant. For the variable, age, the sample also failed the Shapiro-Wilk test for normality and so we have reported the median (30) as opposed to the mean (31.2), and used the Wilcoxon rank-sum test. Age remains non-statistically significant by group.

- **Minor Essential Revisions:**

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

**Thank you for bringing this to our attention; the acronym has been spelled out.

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.
**Thank you for pointing out this necessary edit. The manuscript now reads “Lifetime client-perpetrated forced sex.”**

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 same way as 'depression' 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.

**Thank you for requesting this important clarification. The study by Coleman et al (ref 61) used a modified Edinburgh Depression Scale which “is not intended to diagnosis clinical depression but to select women among whom the substantial probability of depression necessitates further clinical evaluation.” We understand that our methods do not directly compare to those used in the Coleman et al study, and this has been clarified in the manuscript (Discussion para. 3).

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).

**An additional close read has been conducted. Edits have been made to the above error as well as other small corrections.

- **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).

**Thank you for this suggestion. The manuscript now reads “members of community-based organizations”

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.

**Thank you for prompting this clarification; the suggested change has been made.

**Referee 1: Jialiang Li**


**Thank you for prompting this discussion. The question pertaining to depressive symptoms included in our questionnaire was not a validated scale for depression and was not intended to provide any clinical diagnosis. The intention was to complete a screen for depressive symptoms that may signal the need for further evaluation. All participants who screened positive during the questionnaire were given referrals to in-country services and consultations with a psychologist. The extent to which participants were
provided care in-line with current treatment procedures is unknown. The infrastructure surrounding mental health services in The Gambia is often insufficient, where medication may not be a viable option. Data sets, such as these, are valuable in beginning to show the need for mental health services for FSW and survivors of violence in The Gambia. In response to your comment we have added explicit clarification that our measure of depressive symptoms is not intended to measure the prevalence of clinical depression (Study Measures para. 2 and Limitations para. 2), and have emphasized that participants who screen positive for depressive symptoms, as well as for other negative health outcomes including symptoms of STIs, were given appropriate referrals (Methods para. 2). We have also added a discussion of the available research in The Gambia suggesting that depression medication at the population level should be considered a long-term strategy, while therapy and group support interventions for depression may be more feasible intermediate strategies, as seen in other low-resource settings (Discussion para. 5).