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

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Response to Reviewers
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

The article is clearly written and contains well-developed arguments supported effectively by the evidence. The standout issue requiring further consideration is the fact that forced sex by a non-client was not measured in data collection.

- Major Compulsory Revisions:

1. It is necessary to explain the use of forced sex by a client, rather than simply forced sex, as the primary study outcome. This is of particular concern since it is conceivable that FSW are also more vulnerable to experiencing forced sex from non-clients (e.g. by partners, police officers, and/or strangers) and if this were the case then it would be expected to confound findings. Was information on forced sex by a non-client collected? If so, how was this factored into analysis? If not, then is there information available on the prevalence of forced sex by a non-client (or forced sex generally) in this population, or a similar population? This information should be included and integrated into the discussion of findings. I note that the lack of data collection on forced sex by a non-client is mentioned (in Limitations, para. 2 and Conclusions, para. 1), but the implications of this omission need to be comprehensively discussed. I would suggest including this discussion in the sections Outcomes, Statistical Analysis, and Results, as well as Limitations.
**Thank you for raising this concern. We have clarified and emphasized throughout our paper that our sexual violence assessment is specific to client-perpetrated forced sex. We regrettably do not have comparable data on violence victimization by other perpetrators. While participants were also asked to self-report whether or not they had ever been raped, we have deep reservations in the validity of that measure given that the standard in sexual violence research is that of a behavioral assessment and self-reported experiences of “rape” can lead to significant underreporting. Analytically, those who reported forced sex by a client were as likely to report rape by any perpetrator as those who did not report forced sex by a client (p=.99). Therefore rape by any perpetrator does not confound results in this analysis. However, we do appreciate the need for further explanation on this topic. A discussion of this reasoning has been added to para.1 of the Outcomes section, explained in the Statistical analysis section and in the Variable and collection methods sub-section of the Limitations section. This expanded explanation includes data from previous studies (in Introduction para. 2) showing that clients are predominately the perpetrators of violence against sex workers where multiple perpetrators have been compared.

2. The authors mention sexual violence, and physical and sexual violence, when introducing the research problem, but discuss only forced sex as a primary study outcome (e.g. Introduction para. 1, Conclusion para. 1). If the article begins by referring to physical and sexual violence then it would be useful to add an explanation and justification for how forced sex fits within the broader topic of physical and sexual violence victimization, and to justify the use of forced sex rather than the broader spectrum of sexual violence (or physical and sexual violence).

**Thank you for this important comment. The language in this article has been revised to focus only on the health associations of sexual violence. The problem statement now refers specifically to sexual violence and the associations with sexual, reproductive and mental health outcomes. The use of forced sex as the primary exposure, rather than a wider range of sexual or physical violence, has been justified in para. 1 of the Outcomes section. Forced sex by a client was chosen for the primary exposure due to the more direct link with sexual and reproductive health outcomes, which was the primary focus of this paper.

3. I think that it is important to be clear that induced abortion is not, in itself, a poor reproductive health outcome (see, for example, Introduction para. 2). The evidence included in the article from the literature and the present study supports the notion that induced abortion can be a marker for poor reproductive health.

**Thank you for the excellent feedback. We agree with the need to clarify that induced abortion in itself is not a negative health outcome, although it is a valuable marker of unintended pregnancy. The paper now rejects that induced abortion is a negative health consequence and clarifies that induced abortion, in unsafe environments, can lead to serious negative health outcomes, more commonly in settings where abortion is restricted (Introduction para. 3).

4. The title refers to mental health outcomes associated with victimization and I
would suggest making these more prominent in the abstract (perhaps in para. 4, the first sentence could refer to ‘poor sexual, reproductive, and mental health outcomes’).

**Thank you for this suggestion. We have highlighted the associations and implications of mental health in the abstract, as proposed, as well as in the conclusion (Abstract para. 4; Conclusion para. 1).**

5. Given that both a non-probability method (i.e. chain-referral) and a quasi-probability method (i.e. randomized time-based sampling from pre-identified venues) were used in sampling, it would be helpful to include an overarching statement of the likely representativeness of the sample (e.g. in Methods, para. 1). It would also be useful to report the number and proportion of FSW recruited with chain-referral as opposed to randomized time-based sampling from sex work venues.

**Thank you for this important comment. We realized that we mistakenly referred to randomized time-location sampling in the methods section in an earlier draft and apologize for this oversight. We have now clarified that by venue-based sampling, we meant that we recruited convenience samples from venues where sex work was sold. We have updated the statement in the methods section as “a combination of peer-based chain-referral and convenience sampling at sex work venues” for clarity and removed the mistaken reference to randomized time-based sampling (Methods para. 1).**

6. It would be good to report refusals or exclusions by reason (e.g. in Methods, para. 1 or para. 2 – suggest also including a flow diagram). In particular, for someone not familiar with this setting it would be helpful to know the proportion and number of FSW excluded due to being aged less than 16 years, as well as the proportion and number excluded due to language requirements (i.e. are there ethnic minority groups who are likely to be under-represented in the sample).

**Thank you for this comment. Because this was not a probability sample and the majority of participants were recruited via peer-based chain referral, the refusal rate was unfortunately not possible to calculate, and reasons for refusal/exclusion were likewise not possible to tabulate.**

• Minor essential revisions

7. The terms violence, physical and sexual violence, sexual violence, forced sex, rape, and assault are all used in the article. I would suggest tightening the use of terminology, perhaps to using only sexual violence and forced sex. Moreover, where physical violence is mentioned, there is not a clear rationale for this as the majority of the paper addresses sexual violence (in the form of forced sex) only; I would suggest either adding a rationale, for example by linking this more explicitly with the data collection and results on beatings and torture, or removing the references to physical violence.

**Thank you for this feedback. The range of terminology in the paper has been edited to include only references to sexual violence and forced sex. The references to physical violence have been removed so that the paper more...**
clearly focuses on the associations of sexual violence in the form of forced sex.

8. I think it is important to define the term ‘street females’ (Introduction, para. 2) and clearly explain the relevance of this population to FSW.

**Thank you for this comment. Although the women participating in these studies of “street females” have similar reports of violence as do FSW, we have decided that based on the percentage of “street females” that report selling sex we are going to exclude references to studies of “street females” in the Introduction.

9. There appears to be an error in the figure used for either the study population, or the number of study participants reporting forced sex, in the study of street females in Ethiopia (Introduction, para. 2). Please double-check the figures from this study.

** Thank you for bringing this reporting error to our attention. We have checked the original figure, however this section has been removed based on relevance to the FSW Gambia population.

10. When reporting findings from the study of FSW in Nepal, it would be useful to clarify how violence was defined (e.g. physical and/or sexual) and in particular to clarify any results from this study relating to forced sex victimization as a discrete outcome; if forced sex was not measured as a stand-alone outcome in this study, I would suggest noting this when reporting the findings (Introduction, para. 3).

**Thank you for prompting this clarification. The manuscript now reports results specific to forced sex and depression rather than violence more generally (Introduction para. 4).

11. Please clarify whether data collection was conducted by male or female interviewer(s), or both (Participants/Collection Methods, para. 2).

**Thank you for this comment. Data was collect by only female interviewers for FSW in this study. This information has been added to Methods para. 2.

12. Please clarify whether the interview protocol included appropriate referral mechanisms for study participants reporting violence.

**Thank you for this important comment. For this study, participants were referred for comprehensive services through global fund recipients. Gender based violence was emerging as a programmatic domain for Action AID in-country, and they had counseling services available. Although these services were limited, we did refer to the gold standard in The Gambia. This information has been added the Ethics section of Methods para. 3.

13. Please explain which features of the interview protocol were designed to increase the reliability of findings, particularly relating to the reporting of physical and/or sexual violence victimization. I note that this issue is mentioned in Limitations, para. 2, but I suggest that it is useful to mention what was done to address the issue (e.g. in Participants/Collection Methods, para. 2).

**Thank you for this feedback. Several steps were taken to increase the reliability of findings, particularly related to sensitive questions such as those about violence. As explained in Methods para. 2, trained interviewers conducted the
questionnaires in a private room at the study site, and no identifying information was collected. We believe these steps to assure confidentiality helped to increase the reliability of the information provided by participants. Additionally, participants were told they could refuse to answer any question. Giving participants the opportunity not to respond to these sensitive questions may have reduced the effects of self-report bias. We have now summarized this in Participants/Collection Methods, para. 2.

14. I think it is important to explain how torture (as in, ‘had ever been tortured as a result of selling sex’) was defined, or not defined, during data collection. Again, I note that this is mentioned in Limitations, para. 2, but I think it should be made explicit earlier (e.g. in Outcomes, para. 1).

**Torture was not well-defined in this survey. In addition to the discussion of the limitations of this variable in the limitations section this had been mentioned earlier in the Outcomes section (para. 2).**

15. The outcomes relating to difficulty of accessing condoms are reported differently in three different sections of the article (Abstract, para. 3; Outcomes, para. 2; Results, para. 3). I think it is important to use one categorization (e.g. ‘no access’, ‘difficult access’, ‘somewhat difficult access’) consistently.

**Thank you for pointing out this inconsistency. The manuscript has been revised to report ‘no’, ‘very difficult access’ or ‘somewhat difficult access’ in all sections.**

16. Please clarify that previous research on condom non-use during forced sex (Discussion, para. 2; references 13 and 35) is transferable to the study population of this study.

**The study populations in these past studies had similar rates of violent exposure and basic demographics to the population in our study. This justification has been added to the manuscript (Discussion para. 2).**

17. It is important to make explicit the argument that poor mental health outcomes among FSW are a problem in themselves, rather than leaving open the implication that poor mental health outcomes are problematic largely because of their association with future poor sexual health outcomes (Discussion, para.3).

***Thank you for this feedback. We agree with the importance of addressing poor mental health for FSW in its own right in addition to the linkages with forced sex and poor sexual health. This has been added to the Discussion para. 3.***

18. The limitations arising from chain-referral sampling are discussed (in Limitations, para. 1) without reference to the randomized time-based sampling that was conducted. I suggest that it would be useful to mention both methods at the same time, and discuss their combined implications for the likely representativeness of the sample.

**Thank you for this comment. As discussed earlier, the reference to randomized time-based sampling was an error and has been removed. In addition to the Limitations section, a discussion of the likelihood of the representativeness of the sample has been added to the Methods section para. 1.**

19. Given the findings of the study as well as the title of the article, references to
sexual and reproductive health should also include mental health as appropriate (e.g. in Conclusion para. 2). For example, if the authors think this is reasonable, recommendations for the provision of specific services (EC and PEP) could also refer to mental health crisis services as appropriate (see Abstract para. 4; Conclusion para. 1).

**Thank you for this feedback. We have emphasized the integration of mental health services for survivors as one of the primary implications of our findings (Conclusion para. 1, 2).**

Discretionary Revisions:

21. I think it would be helpful for the reader to know whether the interviews took place at venues from which study participants were recruited, or elsewhere.

**Thank you for this comment. The interviews were conducted in a private setting at a dedicated site. We added this information to Methods para. 2.**

22. It is potentially unclear on first reading that the level of educational attainment reported for study participants (Results, para. 1) is the highest level attained, i.e. that the categories are mutually exclusive. It could be useful to clarify this.

**Thank you for prompting this need for clarification. The variable for education has been specified as the “highest level of education achieved” in para. 2 of the Outcomes section.**

23. It would be useful to clarify the statement that FSW who have experienced forced sex are 2 to 5 times more likely to report depression compared with FSW who have not experienced forced sex (Discussion, para. 3). As it currently reads, the comparator group it is potentially unclear.

**Thank you for this comment. The comparison group has now been specified.**

24. The authors may wish to consider including some more specific recommendations for services to address the mental health needs of survivors of violence at community level (Discussion, para. 3). Additionally, the statement that the recommendation for community-based interventions to address mental health would also be beneficial for the general population would be strengthened with reference to the prevalence of depression and other common mental disorders in the general population of The Gambia (if this is available).

**Thank you for this recommendation. We find this information important to include in the manuscript. The discussion section now includes data on estimates of depression levels in The Gambia among women of reproductive age, as well as some specific recommendations for addressing the mental health of survivors at the community level.**

25. The discussion of the observed association between forced sex and access to condoms and receiving STI test in the previous 12 months would (Discussion, para. 4), in my opinion, be strengthened by an explicit discussion of the inability to determine the direction of these associations with the present study design. I note that this is included in the Limitations section, but given that Discussion para. 4 speaks directly to prevention I think it would be useful to mention the issues of direction and causality at the point of interpreting these findings.
Thank you for this suggestion. The Discussion section para. 4 has been edited to include a comment on our inability to determine the direction of the association given the present study design.

26. I would suggest referring to either chain-referral or snowballing sampling consistently. Currently the term snowball sampling is used for the first time in Limitations, para. 1.

**Thank you for this comment. We have made the language consistent, using the term chain-referral as opposed to snowball sampling.

27. I think it would be useful to explain why Respondent Driven Sampling was not used for this study, given that it is mentioned as a desirable sampling method for future use (Limitations, para. 1).

**Thank you for this feedback. Respondent Driven Sampling was not used for this study due to time and budget restrictions. We now note this in the Limitations section.

28. It would be helpful to include some additional information about the ‘strategic plan’ (Conclusion, para. 2) – is it a national-level government plan? Does it have a title, or if not then what specific area(s) does it address?

**Thank you for this comment. Information regarding The Gambia’s National Reproductive Health Policy has been added to the Conclusion para. 2 describing current objectives as they relate to violence and gaps.

- Minor issues not for publication Spelling, typographical errors, grammatical errors, stylistic suggestions etc.

29. I would strongly suggest the use of ‘whom’ (or ‘who’) rather than ‘which’ when referring to study participants (e.g. Results, para. 1; Introduction, para. 2).

**Thank you for this comment. The language used when referring to study participants has been edited.

30. Please consider using whole numbers in statistics reported in Introduction, para. 2 to match the rest of that section of the report.

**Thank you for pointing out this inconsistently. The Introduction now reports whole numbers except for when reporting HIV prevalence where we find the nuances in the varying country data to be valuable.

31. One sentence in Statistical Analysis, para. 1 (beginning ‘Age of entry was controlled for…’) is a little bit unclear. Suggest revising sentence to read something like ‘…higher levels of reported abuse and drug and alcohol use, as well as abuse in childhood and depression as an adult’.

**Thank you for this suggestion. We have re-written this sentence.

Referee 2: Luh Putu Lila Wulandari

Overall, a clear and specific research question has been defined. Appropriate methodology has been used in this study, with some measures were taken to ensure the validity of the data.

With regards to the standards for reporting, this manuscript has adhered to the
relevant standards for reporting, with a strong and justified arguments in the background and discussion session.

The methodology section is clearly and comprehensively described. The authors has clearly stated the limitations of the study and proposed future studies which might address the limitations of the study.

However, there are some questions that might need to address:

1. Was there any time frame for the primary exposure, i.e. “forced by sex clients” (in line 15)

**Thank you for bringing attention to the need for clarification. The timeframe for the primary exposure was “ever” having been forced to have sex by a client. This has been clarified in line 12 by specifying lifetime prevalence.

2. In statistical analysis section (line 10), the authors mentioned the use of chi square test to evaluate the differences between groups. As in table 1 they mentioned the used of z test, this analysis need to be stated in the statistical analysis section.

**Thank you for pointing out this discrepancy. The use of z-tests has been added to the statistical analysis section of the paper.

3. The authors mentioned that there is a table 3 in line 11 of the result section, but the reviewer did not found the table 3.

**Thank you for pointing out this error; the Results section now refers to the appropriate table where the information from the text can be found.

Editors comment: "please note that the authors should report column percentages in Table 1 for the columns "Exposed to forced sex" and "Unexposed to forced sex"

**Thank you for this feedback. We agree that column percentages will be more useful and have updated Table 1 and the results section accordingly.