

Peer Review Report

Review Report on A cross-sectional analysis of young men's gambling and intimate partner violence perpetration in Mwanza, Tanzania

Original Article, Int J Public Health

Reviewer: Janina Steinert

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EVALUATION

Q 1 Please summarize the main findings of the study.

The study assesses the correlation between men's gambling and IPV perpetration in a sample of 1002 men aged 18–24 from Mwanza, Tanzania. The authors find significant associations between gambling and sexual as well as emotional IPV. They control for potential confounders, including alcohol consumption, depression, and substance use.

Q 2 Please highlight the limitations and strengths.

Limitations:

- The major limitation of the paper is the cross-sectional nature of the analysis. While the authors rigorously control for some potential confounders, other underlying behavioural characteristics such as self-control/impulsiveness were not measured and could be omitted variables.
- The concept of mediation is not presented in a clear manner both conceptually and statistically.

Strengths:

- The paper presents important new evidence from an LMIC context on the question at hand.
- The collected data is high-quality and violence measures are well designed.
- The analyses are rigorously conducted.
- The paper is generally well written.

Q 3 Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

I thank the authors for the interesting and well-written manuscript. I have a number of concerns that would need to be addressed prior to publication.

I have three major concerns:

- 1) Omitted variables: The authors are mentioning factors such as traditional masculinity norms and behavioural characteristics such as impulse control and risk-taking. These are factors that could, as also argued by the authors, be associated with both gambling and IPV. My key concern is that these factors have not been measured/captured in the questionnaire and may therefore be important omitted variables. It is even possible that the examined relationship between gambling and IPV is in fact spurious and driven by unmeasured omitted variables. While this issue cannot be tackled with the data at hand, it should be explicitly discussed in the paper's limitation section.
- 2) Partnered vs. unpartnered men: It is unclear to me whether the authors used the full sample of men or the subsample of partnered men? They cite 1002 men in the abstract while it looks like they only included the subsample of 755 partnered men in the analyses reported in Tables 2–3. I agree with the focus on partnered-

only men but this would need to be made more explicit and the authors should then also adjust the reported sample size in the abstract.

3) One of my key recommendations would be to carefully differentiate between two types of covariates: 1) socioeconomic controls (including age, education, employment, and if available income) & 2) confounders. For all analyses/computation of adjusted ORs, I recommend to always include all socioeconomic control variables, irrespective of whether these were significantly associated with IPV in the bivariate analyses. The approach for confounders can then be kept as is.

4) The authors misrepresent the concept of mediation. The term "mediation" is used in reference to Figure 1 as well as in the contribution section. However, the authors do not conduct a formal mediation analysis and the covariates should therefore not be referred to as mediators. I agree that it might be interesting to test pathways from gambling to IPV via a mediation analysis, however, I do not think that this can be adequately done with the cross-sectional data at hand.

I organise my other comments along the structure of the paper.

Abstract:

- I would suggest to briefly name the "potential confounders" for greater clarity.
- "The analysis shows that young men's practice of gambling is an additional risk factor for IPV perpetration." - I would advise the authors to be very cautious with regards to claiming a causal relationship between gambling and IPV perpetration. The cited sentence should best be rephrased as: "The analysis shows that young men's practice of gambling is correlated with sexual and emotional IPV perpetration."

Contribution to the field:

- "Tanzania has shown that gambling was associated with increased odds of emotional and sexual IPV perpetration, and that the association was partly mediated by alcohol use and depressive symptoms." - This needs to be rephrased since the authors are not formally testing for mediation. They simply control for confounding through alcohol use and depression.

Introduction:

- The introduction would benefit from presenting a detailed definition of gambling and a brief elaboration on gambling practices in Tanzania and of how these might differ from gambling practices in high-income country contexts.
- "Young males were identified as more likely to engage in gambling-related activities, compared to young women (46-48, 50), which is explained by young women generally being more risk averse." - What is the comparator for young women's risk avoidance? Is this compared to other young men or compared to older women? Either way, there needs to be a reference to back up this statement.

Methods:

- Sampling: "Only one young man was randomly selected from each household." - Could the authors elaborate more on how this man was selected? How did the authors proceed if more men in the household were between 18-24 years old? It might be helpful to present a participant flowchart to also have a better understanding of how many approached participants refused and how many approached households did not have a man in the eligible age category.
- It is unclear to me with how many items each of the IPV types were measured. If these are several items per IPV type, how were they aggregated? Was the outcome binary and coded "1" if any of the items were answered affirmatively? This needs to be spelled out.
- Similar to this, it is unclear how the gambling items were aggregated into a final measure.
- For greater transparency, I would recommend that the authors add a table to the supplementary material, which presents the exact phrasing of each question/item that was included in the analysis.
- Was ACASI only used for sensitive violence questions or also for sensitive questions on gambling behaviour, drug use, etc.? If only the former, the authors should mention this as one of the key limitations in their limitation section since these behaviours could then be prone to under-reporting due to social desirability and stigmatisation.

- The authors indicate that they have excluded illegal gambling. First, it might be helpful to outline how illegal gambling is defined and in what ways it differs from legal gambling. Second, it would be valuable to provide a justification of why illegal gambling was excluded.

- Covariates: There is one important covariate missing: A measure of economic welfare such as income or household living standards. Was this captured to some extent in the survey? If it was, I strongly advise to include this measure since spousal conflicts related to gambling (and linked to this: emotional IPV) are likely affected by how many financial resources are generally available in the household/family.

Results:

- I would prefer to see Table 1 in the main text rather than in the supplementary files.

Discussion:

- Please include a thorough discussion of possibly omitted variables and potential avenues for future research with regards to these.

PLEASE COMMENT

Q 4 Is the title appropriate, concise, attractive?

Yes

Q 5 Are the keywords appropriate?

Yes

Q 6 Is the English language of sufficient quality?

Yes

Q 7 Is the quality of the figures and tables satisfactory?

Yes.

Q 8 Does the reference list cover the relevant literature adequately and in an unbiased manner?

Yes

QUALITY ASSESSMENT

Q 9 Originality



Q 10 Rigor



Q 11 Significance to the field



Q 12 Interest to a general audience



Q 13 Quality of the writing



Q 14 Overall scientific quality of the study



REVISION LEVEL

Q 15 Please make a recommendation based on your comments:

Major revisions.