Author’s response to reviews

Title: Risk factors for homicide victimization in post-genocide Rwanda: a population-based case-control study.

Authors:

Wilson Rubanzana (wrubanzana@nursph.org)
Michael D Freeman (forensictrauma@gmail.com)
Joseph Ntaganira (jntaganira@nursph.org)
Bethany L Hedt-Gauthier (bethhedt@gmail.com)

Version: 4  Date: 16 June 2015

Author's response to reviews: see over
Our sincere thanks to the editor and reviewers for recognizing the value of our paper, and we have found valuable all edits suggested by the reviewer and we have indicated all edits with red font in the manuscript and gave specific responses to reviewers below.

1. Definitions: what is a male intimate partner – are these all women or were some men? If so how many?
   * The definition of intimate partner has been added in the manuscript “43 (27.6%) were intimate partners that included former or current spouses, boyfriends or girlfriends of the victim. Of these intimate partner homicide victims, 31 (72.1%) were women and 12 (27.9%) were men”

2. Definitions: how was physical/sexual violence defined and ascertained? Was this only from an intimate partner (of the opposite sex)?
   * The definition of physical, psychological/sexual violence was added in the methods section “previous gender based violence that included physical assault, psychological harassment and/or sexual assault”. Like any other characteristic of the study, this variable was ascertained by trained interviewers through a pretested standardized questionnaire. The question was tightly structured and closed-ended, with possible responses (yes, no, don’t know). Data was collected on all study subjects, regardless of their gender status.

3. I am very worried about the variable related to belonging to a religion. The effect size here is so large that I would consider it likely to be a result of controls selection bias, in which case it should not be in the models. I am concerned that it would be much easier to identify people in a neighbouring village for control selection who were better socially networked in the village and being a member of a religion would be an indicator of better social networking. I am also very worried when I see anything written about Africa which has people listed as ‘not’ members of a religion as religiosity is almost universal on the Continent when adhering to traditional religious beliefs is included in the definition of religions. It is not correct to exclude these when considering religion. I wonder if this variable has been correctly measured and whether it should actually be used.
   * The authors understand your concerns on the variable of belonging to a religion, and would like to provide further explanation on the subject:
   - Belonging to a religion variable is now more clearly defined. “… belonging to a religion defined as being a member of a Christian church or being a member of Muslim congregation”
   - We agree with the reviewer’s comment that belonging to a religion may represent more than just religion, it may represent the network in the community. However, we think this concern is addressed, and the interpretation of religion appropriately tempered with the following statement: “Religion may be a proxy for other community and social factors, and the role of religion in protection against homicide must be further studied.”
   - The selection of controls was not random; we individually matched a case of homicide to 3 controls, on age 5 years interval, sex and neighborhood. Therefore, belonging to a religion was not a matching criteria. Further, as indicated in the limitations of the study, effort was made to minimize selection bias. “The
selection of controls was not randomly performed, but rather we relied on matching criteria and the assistance of the village administrators. In order to minimize the effect of non-randomization of living controls, interviewers sensitized the village administrators to reduce the preferential selection of certain individuals.”

-In a study conducted in neighboring Tanzania on a similar subject, entitled Sociocultural factors that reduce risk of homicide in Dar es Salaam: a case control study, belonging to a religion was identified as a protective factor against homicide victimhood. This is also highlighted in the discussion: “Further, as found in a homicide study in Dar es Salam, Tanzania,[8] we found that belonging to any religion reduced the odds of being killed among both men and women in Rwanda.”

In summary, while we understand the reviewer’s concerns, we feel that we have adequately addressed her specific issues and tempered interpretations accordingly. Religion is an important individual factor in the Rwandan context and we feel it should be included.

4. The removal of the religion variable would solve the problem of the variable being presented for men and women separately in table 3 – how has this analysis been done? It says it is stratified by gender but actually the findings are not presented as a stratified analysis. If it has been done so please present both models

* We did not build two separate multivariate models, one for men and one for women because the study was not designed or powered to do so. However, based on the reviewer’s comments, we feel it is important to clarify what we have done in the analysis.

First, the analysis was done stratified in Tables 1 and 2. (Note, the stratification in Table 1 is new, added at the request of the reviewer below.) In Table 2, in addition to bivariate analysis stratified by sex, we used conditional logistic regression with an interaction term to test whether the effect of the predictor was different between men and women. If the interaction term testing whether the effect was different for men and women was not significant, then we modeled the effect as a single effect, collapsed across sex. If the interaction was significant, then we considered that interaction term (essentially modeling the effect for men and women separately) in the multivariate model. Only the following interactions were significant in Table 2, and therefore even considered for the multivariate model: religion and employment status.

For the final multivariate model, we considered any significant predictors and interactions for the final model. We performed backward stepwise elimination to identify risk factors and just the same as any other term in the model, we only kept an interaction term in the model if it was significant (indicating that the effect was different between men and women). During this process employment status interactions were dropped from the model, and belonging to a religion was found to be the only significant interaction on sex status. As such, we report the effect of this variable for men and for women, but not all factors for men and women separately.
We feel the process for this analysis is explained in the methods: “The first step identified potential risk factors using univariable conditional logistic regression models for each predictor, stratified by gender. Further, risk factors were assessed for whether there was a significant interaction between the risk factor and gender. Variables that were significant either for women and/or men at the $\alpha=0.1$ significance level or interactions that were found to be statistically significant at the $\alpha=0.1$ significance level were subsequently considered for the multivariable conditional logistic regression model developed using stepwise backwards elimination, stopping at explanatory variables that showed significance at $\alpha=0.05$.” And again, we have opted to not complete a full stratified analysis because that was not the original design of the study and we do not believe we would have adequate power to do so.

5. Table 1 should be presented by gender and then for both genders together

*This change has been made and discussed in the first paragraph of the results.

6. Table 3 – is this analysis age adjusted? It should be as parental death is age related

* In this study, we matched for age in the selection of controls, and therefore, it was self-adjusted.

7. Pooling intimate partner AND first-degree family relative at times and at other times separating them is confusing. There is considerable interest in intimate partner homicide and so please present the data for these separate from ‘first degree family relative’. The discussion should then compare the male and female figures with global proportions (there was a recent systematic review)

*We have clarified in the text so that intimate partner and first-degree family relative results are separated: “43 (27.6%) were intimate partners that included former or current spouse, boyfriend or girlfriend of the victim. Of these intimate partner homicide victims, 31 (72.1%) were women and 12 (27.9%) were men. Further, there were 42 (26.9%) homicide victims who were killed by family members, other than spouses.” and in the discussion, our findings were compared to a recent systematic review’s results “Further, descriptive analysis showed that 43 (27.6%) and 42 (26.9%) of cases were killed by an intimate partner and a first-degree family member, respectively. Of the intimate partner homicides, 31 were women and 12 men, accounting for 46.3% of all female homicide victims and 13.5% of all male homicide victims. This was considerably higher than the 38.6% of female and 6.3% of male homicide victims who were killed by intimate partner, identified in recent systematic review of intimate partner homicides from 66 countries”. (Reference 12)

8. The alcohol abuse issue has been discussed in South African literature on intimate femicide, I suggest that the authors read the other Mathews and Abrahams papers

*Mathews and Abrahams on alcohol and intimate partner homicide has been discussed: “We were surprised that the effect of alcohol was the same among women and men. We thought that this finding among women could be explained by their consumption of home-brewed alcohol sold illegally, especially in rural
areas. “This result is consistent with findings of a study conducted in South Africa that demonstrated high level of blood alcohol concentration among the majority of women homicide victims at the time of their death in Western Cape.” (Reference 25)

9. The paper twice says that the risk factors for homicide pertain irrespective of gender. In fact the differences in the proportion of men and women with previous physical and sexual violence is very large and I would be surprised if there were no differences in AOR. We are told in the text that a stratified analysis by gender was performed but the cover letter says it wasn’t – what was done – if stratified please present models for each gender – if not stratified then please examine and discuss interaction effects.

* The original design of the study was not to be stratified by gender and we were not powered to look at all of these factors separately by men and women. We have tried to explore potential differences using interaction terms, reporting factors that were significantly different between men and women. However, in this case, religion was the only factor that had a significantly different effect. We believe the following acknowledges this and calls for more research in this area: “However, while interpreting our results, the lack of significant difference of prior gender based violence on homicide victimization by gender status should be considered with caution because our study was not initially designed to specifically investigate intimate partner violence. Further, we recommend future studies in Rwanda and the region to better understand how this and other risk factors for homicide differs between men and women.”