Author's response to reviews

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

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Author's response to reviews: see over
Thank you to the editor and the reviewers for their thoughtful comments. We have found this assessment to have a positive impact on our paper and we appreciate the opportunity given to us to revise the paper. We believe that this new version is much improved. We have indicated all changes with red font in the manuscript and provide specific responses to reviewers below.

**Reviewer 1: Martin Andresen**

This is a very interesting paper that investigates an important topic. What I find particularly interesting is that the authors find that the risk factors associated with homicide in Rwanda are not too dissimilar to those found in more conventional homicide research conducted on populations in the developed world. I, therefore, think it would be instructive for the authors to include some more reference to this work. This in no way reduces the importance of what has been undertaken here. Rather, highlighting the similarities and differences is instructive to a better understanding of this phenomenon and, more importantly, for its prevention.

* More references were added in order to highlight the similarities and differences between our findings and other studies on homicide in South Africa and elsewhere. Additional references include: Lim et al, 2013, Cocks et al, 2013, Matzopulos et al, 2014.

With regard to the statistical methodology, I too use a general to specific method of final model selection, but with one added step that these authors may find useful. In addition to the individual tests for significance when moving towards the final model I also undertake joint significance tests of removed variables to ensure that I am not removing variables that actually matter but are just collinear with other (combinations of) variables.

* Thank you for catching this. We have added the following in the statistical method section: “Prior to model development, we tested for collinearity between the variables of interest using Pearson correlation (r<0.5).” This was done previously, but not detailed in the methods. “We did not find any variables collinear based on this test.”

Lastly, though odds-ratios are instructive I find them limiting in and of themselves. I strongly urge the authors to calculate marginal effects as well that considers the baseline probability of the event in question. There are a number of ways this can be done.

* This is an interesting suggestion from the reviewer and one that led us to do some investigation into the best means to report results. We used odds ratios as our effect measure because: 1) The study was an individually matched case-control study, and odds ratios are the most commonly reported for these designs and 2) We are the most comfortable interpreting these effect measures and believe that the effect of different risk factors is understood through this measure. Because of the case-control sampling, we cannot get a baseline estimate of the risk for homicide victimization without some assumptions, and similarly for the marginal effects.
We have found some recent methods articles on potential solutions for this. Specifically, from the 2014 PhD thesis of Emma Persson (Umea University), we found the following: “The third paper investigates the particular situation where case-control sampled data is reused to estimate the effect of the case-defining event on an outcome of interest. The consequence of ignoring the design when estimating the average causal effect is discussed and a design-weighted matching estimator is proposed. The performance of the estimator is evaluated with simulation experiments, when matching on the covariates directly and when matching on the propensity score.” You will see that these methods are quite new, and the presentation of the methods very theoretical.

Therefore, we have opted to stick with the odds ratio because of our comfort with the method and interpretation of the results and our inability to implement the new methods without substantial statistical support. However, this may reflect our personal limitations, and we are open to the reviewer’s specific suggestions of how to proceed (ideally with reference to analysis packages to support the reporting of these results).

Reviewer 2: Rachel Jewkes

1. is there information on the number of excluded cases by reason for exclusion?.

*We don’t have a number of cases of actual homicide that were excluded from our study by our exclusion criteria. The study PI (WR) is the head of the forensic medical service that has access on daily alleged homicide reports. He was personally in charge of the identification, confirmation and selection of the cases with a view of ensuring that actual cases of homicide were indeed enrolled in the study. Because Rwanda does not have a national injury mortality surveillance system based on reliable forensic medical expertise, we were unable to compare our sample to the “true homicide cases” during that time period to see who was excluded.

2. what was the non-response rate among controls and case relatives?

* We have added the following to the text: “All cases and controls relatives who were asked to participate in our study provided information”.

3. The introduction provides some annual homicide rates for Rwanda, if I am right in assuming that the investigators have a census of homicides (if unidentified victims and those with untraceable family members are added), could they present rates for total, male and female homicide based on this for interest and to show some comparability with previously published rates

*It is true in the introduction of our article, we provide some annual rates and sources of information, but we did not have a census of homicides. We selected only cases of homicides that met our selection criteria. The main reason for providing annual rates of homicides was rather to highlight the inconsistencies in homicide data collection in Rwanda, like in any other developing country, where
no standardized reliable homicide surveillance system exists. Indeed, one can note that annual rates calculated based on annual police reports are much less than rates reported by international agencies such as WHO, Interpol, UNODC, etc.

4. The analytic approach is a gender blind analysis. This is not appropriate for a highly gendered event such as homicide. It is also not in accordance with good practice in acknowledging gender differences in health problems. Homicide is a particularly highly gendered occurrence as half of female homicides in more countries are perpetrated by male intimate partners and it is very unusual for men to be killed by intimate partners (and data on this from developing countries is very scanty). I recognise that the sample size is small, but the study would be far more interesting if the analysis was done by gender. This would allow the very different circumstances of male and female death to be made visible. This is my strongest recommendation and I would like to see it run through all of the analyses. It would enable a direct male/female comparison in homicide victim characteristics in table 1. It would enable readers to see what is significant for which gender in tables 2 and 3. I suggest not presenting the pooled analysis at all as it is not meaningful in this context. In revising table 2 please pay attention to the column headings.

*This comment is very important and though our study was not designed to assess different risks for men and women, a stratified analysis by gender status was performed. You will see the changes in the methods, results and discussion. Notably, we only found one difference – the effect of religion was significant and in the same direction for both men and women, but the effect was stronger for women. However, part of the reason for not finding a difference, particularly for previous gender based violence, may have been a power issue and we highlight this potential limitation as “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.”

5. In labeling the variables in the tables please provide fuller labels. As a reader I would like to be reminded in a glance that it is victim drinking etc (rather than perpetrator).

*Because our study design was a case control (homicide victim/living control), and therefore all variables applied to both homicide victims and living controls, we addressed the labeling of variables in the tables titles as follows: “Table 2: Univariate conditional logistic regression analysis of hypothesized risk factors by sex status of 156 cases of homicide victims and 468 living controls.” “Table 3: Multivariate conditional logistic regression analysis of hypothesized risk factors by sex status of 156 cases of homicide victims and 468 living controls.”

6. The discussion will actually become more interesting with the gendered analysis. It would be good to have the proportion of women killed by partners compared with that found internationally and very interesting to see if there are any men killed by female partners (and a comparison likewise). The role of alcohol and drugs in homicide are more interesting if women are drinking when it is not a social norm for women to...
drink. I am not aware of the situation in Rwanda. I find the non-importance of genocide-related exposures very interesting, perhaps there is a gender difference here too, or may be the question is not about revenge, but trauma exposure of the perpetrator.

*Per the previous comment, we ran a full stratified analysis and none of the suggested factors appeared in our study. However, the number of men and women killed by their partners was provided in the discussion section of the manuscript: “Further, descriptive analysis showed that 85 (54.5%) of cases were killed by an intimate partner or first-degree family member. Of these, 51 were women and 34 men, accounting for 76.1% of all female homicide victims and 38.2% of all male homicide victims, respectively”

7. Gender-based violence is an important risk factor (in women I suspect) and it would be interesting to highlight this as it has evidentiary implications in cases as well as being amenable for prevention

* We fully agree that the gender-based violence could be one of the important risk factors for homicide death in Rwanda, and this subject needs to be further investigated using other study design methods. We discuss this factor in the following paragraph: “Notably, our sex specific findings differed from studies conducted in South Africa and Western countries that extensively investigated intimate partner violence and firearm availability and female homicide [23-32]. Our study did not specifically study these two homicide risk factors, but rather our aim was to evaluate common socio-demographic characteristics of homicide victims and long lasting effects of genocide. Unlike South Africa and Western countries, the availability and use of firearms are legally limited to national security organs and few private security companies in Rwanda. Intimate partner violence was not well documented till 2009 when the Government established Isange one stop center. Our findings did identify previous physical and/or sexual violence as a risk factor for homicide victimization and more interesting, the characteristic did not any significant difference on homicide victimhood based on gender status. This finding is inconsistent with the results of a studies carried out in South Africa and Portugal that found prior domestic violence as a high risk factor for homicide victimization among women [25-32]. 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.”

It is worth noting that in the spirit of this being an important issue needing more research, we recently submitted for publication an article to BMC Women’s Health Journal titled “Factors associated with intimate partner violence against women-A cross sectional study”.

8. I would caution about making much of religion. It’s possible that it’s a real finding but I am concerned that it could be the variable that is most likely to be biased and may indicate control-selection bias. It’s a case control study Achilles heel. A disproportionate number of people who are killed are homeless/mentally ill/drunks/anti-social in some way and these groups are not going to be one’s first
choice for control arm participants. In contrast people who are involved in religion are more upstanding/cleaner living and often are more ‘known’ in a positive sense to authorities. I know efforts were made to mask the study aims, but unless there was random selection among an eligible age group in a village this problem will have been present. It’s impossible to know how important it is but I just suggest that it be acknowledged and the religion finding treated with caution.

* In Rwanda, like in any other Sub Saharan African countries, religion is an important socio-demographic factor that is often evaluated alongside with other basic socio-demographic characteristics. Additionally, we included this characteristic because it has been identified as a protective risk factor for homicide in other African setting (see reference on a study conducted in Dar es Salaam, Tanzania). But to note the reviewer’s comment, the paper has the following sentence: “Religion may be a proxy for other community and social factors, and the role of religion in protection against homicide must be further studied.”

9. Recent South African homicide papers are not discussed, I suspect this indicates that the references are somewhat old and the paper would benefit from updating here. These are quite major revisions and so I will not provide minor feedback until the next round of reviews.

Editor’s comment

Please specifically state in the manuscript whether the ethics committee you received approval from is in Rwanda.

* We have stated in the manuscript that our study was approved by Rwanda National Ethics Committee: “The study protocol was reviewed and approved by the School of Public Health’s institutional review board (IRB), “operating under the Rwanda National Ethics Committee.”

*Thank you, we have reviewed the publications and added the following references: Cocks et al, 2013,Matzopulos et al, 2014. We are open to other specific articles from the reviewer if she feels these are not sufficient.