

Time trends and risk factors for sexual violence among school-going youth: national cross-sectional studies in southern Africa in 2003 and 2007.

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Reviewer: 1 (Michele Decker)

Comment 1. Introduction.

The introduction is surprisingly brief. The authors provide no justification for the risk factors selected, and provide no conceptualization for the risks of sexual violence.

Response 1

We have expanded the Introduction

Comment 2. Introduction.

Their statement that “different measurement methods produce different estimates of occurrence” is correct, but they provide no justification as to the value of comparing and contrasting estimates across time or national contexts.

Response 2

This is covered in the expanded Introduction. There is very little quantitative evidence about the rates of sexual violence against children in southern Africa.

Comment 3. Sample.

Can the authors describe the portion of youth who attend school with any regularity in these settings? Gender differences therein?

Response 3

We do not have details about enrolment and attendance other than on the day of the survey. We know an important limitation of the study is that it could not contact young women who were unable to attend school due to pregnancy, a possible consequence of sexual abuse, or who left school because of the experience of abuse or fear of further abuse. The percentage of youth interviewed who were female illustrates this: 60%, 59%, 57%, 55%, 53% and 49% with increasing age from 11 through to 16 years.

Comment 4. Measures.

Please provide some description of the community-level variables derived from the household survey. What was the sample size, what were the exact items, and what can be said about their psychometric properties?

Response 4

We have already provided references that give details about the methods of the household survey. We added a list of items derived from the household survey; the frequency of responses to these items from adults in the household survey are in Table 3.

Comment 5. Analysis.

The authors describe that analyses were clustered –was this at the school level? At what level?

Response 5

The analysis was clustered at school level. We have made this clearer in the revised paper.

Comment 6. Analysis.

The utility of the school-level perpetration variable is questionable. This is not giving us a tremendous amount of useful information. Why would it be important to know that school-level sexual violence perpetration is associated with sexual violence victimization? The authors provide no interpretation of this finding beyond the “social nature” of the phenomenon of sexual violence, it seems most likely that the perpetrators and victims attend school together. Is this useful? It seems that this is swamping the other factors in the analysis and has very little conceptual basis for inclusion.

Response 6

We have clarified why we considered it important to include school-level variables, some of which turned out to be strongly associated with the experience of forced sex among students. This clustering indicates that some schools might foster a culture of sexual violence, If we can understand more about how this effect operates, it opens an avenue for prevention interventions at school level.

Comment 7. Table 2/Results/Analysis.

There is considerable confusion as to what is being presented in Table 2. The tables do not really reflect the analyses described. In analysis section, authors state that they report Odds Ratios adjusted for covariates, but there is no indication of this in Table 2.

- a. It is unclear whether the ORS in this table are adjusted, and if so, for what. Please provide further details in notation. Later the text, Page 5, suggests that Table 2 is indeed unadjusted. Goes on to describe adjusted results.
- b. Please provide a table to summarize the results of adjusted analyses.

Response 7

We have revised the text and Table 2 notations to make it clearer that the findings in Table 2 are adjusted only for clustering and country and not for other covariates. We have added a new table of the GLMM multivariate analysis, provided in the text in the previous version. The new table shows separate analyses for male and female students.

Comment 8. Analysis.

Given the robust sample, the analyses are surprisingly unsophisticated. Given the wide literature on violence and risk factors for women worldwide, gender-stratified analyses for the risk factor component would be more appropriate. This is particularly the case given concern that the assessment may intermingle childhood sexual abuse which impacts both boys and girls, with sexual violence during adolescence and early adulthood in which women are differentially impacted.

Response 8

We have provided separate analyses for males and females throughout.

Comment 9. Discussion.

The discussion is quite weak and makes little use of the findings presented in the paper. In particular, what can be said about the perpetration results? What can be said about social norms given that perpetration was associated with low levels of gender equality?

Response 9

We have expanded the Discussion section, including further discussion of the findings about perpetration of forced sex.

Comment 10. Discussion.

The literature on school-based sexual violence suggests that women are differentially affected, with many leaving school following assault or for fear of assault. This research base would suggest

significant biases using the school-based sample to assess lifetime history of sexual violence, particularly for girls, threatening the validity of findings. This should be discussed. The possibility that it explains the lower rate of violence against girls should be addressed.

Response 10

We have expanded our comments in the Discussion about the female rate of forced sex being underestimated because our sample excludes girls forced to leave school as a consequence – including pregnancy – of sexual violence.

Comment 11.

Tables - please provide a table to summarize the results concerning perpetration of forced sex. I assume this is a gender-stratified analysis given the significant gender difference in perpetration?

Response 11

We have provided an additional table showing the analysis of factors associated with perpetration of forced sex, separately for males and females.

Comment 12. Overall frame.

The use of a lifetime measure of sexual violence with an adolescent population presents challenges in that the assessment likely captures both experiences of childhood violence as well as violence during adolescence and early dating experiences. This is a significant limitation, particularly given the literature demonstrating risks for sexual violence among young adult women around the world. The authors assumption the violence captured here occurred during “childhood” is questionable.

Response 12

In most of the countries surveyed, the age of consent for sexual intercourse is 16 years (for Tanzania it is 18 years). For the 80% of participants under the age of 16 years, by definition, the sexual abuse occurred during childhood. Of the 1118 16-year-old females who reported forced or coerced sex, 498 said this first occurred when they were aged 16 or did not give an age when it occurred, similarly among the 1093 16-year-old males reporting forced or coerced sex, 377 said it first occurred when they were aged 16 or did not specify the age of first occurrence. A sensitivity analysis excluded these 875 youth; we could detect no shift in the pattern of risk factors. We clarify this in an addition to the text.

Reviewer: 2 (Matt Breiding)

Comment 1

Let me start with the title as there are two problems with the title that are reflective of issues in the manuscript. First, the authors do not utilize statistical tests that could test trends, but rather examine statistical differences between prevalence estimates at two time points. There is nothing wrong with this approach but labeling it as an analysis of trend is incorrect. It is also incorrect because the authors would need to collect data at a minimum of three time points to make a trend analysis even possible.

Response 1

We have removed references to time trends in the title and elsewhere in the paper.

Comment 2

My second issue with the paper that is reflected in the title is that the authors did not measure sexual violence, but rather forced sex. Sexual violence as a construct is much more broad and can include attempted rape, drug/alcohol-facilitated rape, coerced sex, fondling of a victim, forced touching of a

perpetrator, and even forms of non-touch acts (e.g. sexual harassment, flashing, forced watching of sexual acts or sexual material). Again, there is nothing wrong with what the authors measured, as forced sex is the most serious form of sexual violence, but labeling it as sexual violence is misleading.

Response 2

We have changed the title to make it clear that the paper deals with one form of sexual violence: forced and coerced sex. And we refer to forced and coerced sex throughout the paper.

Comment 3

The authors describe the sample as nationally representative but leave out quite a bit of information that would allow for an understanding as to whether it truly is nationally representative. How were EAs sampled? How were schools within EAs sampled? How many EAs were sampled in each country and how was this determined? They note that 7 classes of 35 children per EA were sampled but how was this number obtained? The authors provide a bit more information on how classes were sampled within schools but this information raises more questions than it answers. They indicate that at least one class was randomly selected per grade. Why would more than one class be selected per grade and how was this undertaken?

Response 3

We have provided some more detail about the sample and sampling in the Methods section.

Comment 4

I think the author's need to explain more about their weighting strategy other than to state that they "weighted regional frequency estimates in proportion to the population of the countries." I'm unfamiliar with this weighting method and am unsure of the justification for such a method.

Response 4

We have given more detail about weighting in the Methods section.

Comment 5

My most pressing concerns about this study are related to whether there were procedures and protections in place to reduce the likelihood of harm to children. It is admirable that they authors sought and received IRB approval in each country and from an "accredited international ethical review board" (side note: why can't they be specific in naming this review board?), it is unclear whether any of these review boards are aware of the difficult ethical issues related to the study of sexual violence, particularly among children. WHO guidelines regarding the study of violence against women (<http://www.who.int/gender/violence/womenfirtseng.pdf>) lay out a number of these issues which are even more critical with regard to children given their added vulnerability. The authors do not provide much reassurance as to how children were protected from potential reprisal if it became known that the study contained questions about forced sex. After all, by asking children to report that they experienced forced sex is, in most countries, to ask them to report a crime. By merely putting them in a position to make that judgment, is to place them in a difficult position as they have no way of knowing to what degree the survey truly is anonymous. What steps were taken to reduce the possibility of distress? The WHO guidelines recommend that only the respondent know the content of the survey. How can this even be accomplished in a school setting? By administering the study in schools, where there is concern in many African countries about teachers' perpetration of sexual violence against children, how could this be accomplished? What were teachers/administrators told about the content of the survey? Parents are another group of potential perpetrators, so what were they told regarding the content of the survey? Finally, what was the protocol in place should a student become upset about the question(s)? I have serious concerns about whether these issues can even be addressed in the school setting. Perhaps they were but it is

critical that the authors explain how they minimized the harm to study participants.

Response 5

These are important ethical issues. We spent a lot of time and effort to address the ethical issues that arise when asking children about their experience of forced sex in a school setting, in design, training, and implementation of the study. We took into account the WHO guidelines on the study of violence against women, as well as other relevant ethical guidelines. In the expanded Methods section we have included more information on the measures we had in place to address ethical concerns. The question of consent from parents was one issue. In one country, the ethical review board initially wanted us to send the questionnaire to parents to seek their explicit written consent for their children to participate in the study (an opt-in process). After we pointed out the evidence that much sexual abuse of children arises in the family, and that this might mean sexually abused children did not participate in the study or risked retribution for their participation, the committee agreed to parents being given general information about the study (without details of the contents) and dropped the requirement for specific written consent from parents, instead having an opt-out arrangement, whereby parents who did not want their children to participate should instruct them not to complete the questionnaire. The consent from school heads was obtained, in nearly all cases, without showing them the details of the questionnaires. In case the questionnaire did need to be shared, this was restricted to the school head only. No teachers were present in the classroom when the facilitators read out the questions. The facilitators explained to the students at the beginning of the session about how to keep their answers private and explained that their answers would be anonymous; they should not write their name anywhere. We provided specific training for facilitators in how to handle students who became upset as a result of the questions or who wished to talk further about personal experiences after the questionnaire session.

Comment 6

I don't recall seeing the overall prevalence estimates for victimization broken down by sex, only a combined estimate. These numbers are almost always stratified by sex (as was done for perpetration). It's also unclear why the authors combined the risk factor analyses for men and women. These are also never combined as females and males often have different risk factors. Why was this done? Do they have evidence to suggest that the risk factors were the same when analyzed separately?

Response 6

We have provided separate figures for males and females throughout the revised paper, as well as separate risk analyses for males and females.

Comment 7

Something should be mentioned, given that the primary focus of this study was not forced sex, that the risk factors examined were likely not selected a priori based on a hypothesized relationship to forced sex, and that other unmeasured risk factors are most likely at play.

Response 7

We did indeed include in the instrument questions about factors we believed were likely to be risk factors for forced sex, but we have added a comment in the Discussion that there are likely to be risk factors we did not measure.

Comment 8

The authors conclude that their estimates are likely underestimates, which is probably true, but also state that it is probably more of an underestimate for females than males. What is the support for this?

Response 8

We have added more detail in the Discussion about why the rates of forced sex are likely to be more underestimated among females than males.

Comment 9

Finally, there is a lot of information presented in the article but very little to tie it together in the discussion to prior work, future studies, or prevention/intervention. Further, a discussion of how these findings relate to the HIV epidemic in Africa is critical yet the authors don't mention HIV until the final words of the manuscript.

Response 9

We have expanded the Discussion, in including adding more about the relevance of the findings to the HIV epidemic in southern Africa.

Reviewer: 3 (Karen Devries)

Comment 1

The paper is interesting and well written, and provides unique data on the prevalence of forced sex among young people attending school in a variety of settings. This is a very important area of research—experience of sexual violence has numerous adverse health consequences and is itself a human rights violation. There are few comparable data available on the topic, especially from lower income settings—measures of sexual violence in this age group are not included in other international surveys such as DHS. However, I think there are several areas where further explanation/discussion are required before this could be considered for publication in a high impact journal of such as BMJ.

Response 1

We have addressed the specific points raised, in particular providing additional information as requested.

Comment 1. Data quality.

I would like further information about the survey data to be assured of quality—levels of literacy in many of these settings, especially in younger children, are likely to be very low. The methods section states that a facilitator read out the questions and children filled in the corresponding bubble on the answer sheet. I worry about one facilitator being able to ensure that what can be very large classes of children are following along and completing the score sheet correctly, especially if they will have difficulty in reading and following along themselves. This method would require all children to be listening and following along at the same pace—data quality would probably go down considerably towards the end of the survey. How long did it take to complete? Did patterns of response any indicate difficulties? It would be helpful for readers to get more information about data collection to ensure that quality is adequate.

Response 1

We have provided more details in the Methods. Levels of literacy were indeed low in some places. This was one reason why we strongly encouraged the students to wait to *hear* the question and the response options before selecting their response. The language used was that chosen by the class; some preferred their local language, some preferred English, some requested a combination. We trained the facilitators in how to give explanations of questions that might arise, and the facilitators encouraged the students to ask if they were not clear about the meaning of any question. At least two researchers were present in every class, and for larger classes three or four researchers, in order

to ensure privacy and that the students were coping with the questions. The session, including the initial explanations and instructions about privacy (see below), the reading out of the questions, and the collection of the completed answer sheets, took less than one hour. In some cases, the facilitators spent longer in order to discuss either with the whole class, or with individual students, additional questions the students wanted to ask.

Comment 2.

I have serious concerns about privacy during data collection, and consequently, disclosure of violence. Did facilitators have any training in how to ensure privacy and confidentiality, and how did they do this in a classroom setting which was likely to be crowded? I would suspect that disclosure would be low with this type of method, which needs some discussion. Were facilitators trained at all in asking about sensitive issues? It is true that anonymous surveys in high income settings have more disclosure, but I'm not sure that the situation described in this study is at all parallel. Likewise, it is also true that general health surveys find lower rates of violence than studies specifically designed to measure violence, which usually have interviewer training and well established procedures around privacy and confidentiality. This should also be mentioned.

Response 2

Privacy was indeed a pivotal concern during training of facilitators and fieldwork, and the classes were crowded. We have added more details in the Methods section. The training of the facilitators included techniques for increasing privacy, which they covered with the students at the beginning of the session. The facilitators advised students to use exercise books to shield their response papers to ensure privacy and co-facilitators circulated to ensure this was working. At the beginning of the session, the facilitators explained that the responses would be anonymous; the students should not write their names anywhere. At the end they showed the students how they sealed the completed responses from the whole class into an envelope. We also provided specific training for the facilitators in how to ask about sensitive issues, and how to deal with students who might be upset by the questions or want to discuss concerns after the session.

Comment 3. The measure of sexual violence.

The authors clearly state the measure, which is around forced sex. It would be helpful to include some discussion of what this measure is not capturing, specifically around coercive sex. In these settings, this may be especially important for girls. There is also the added question of what students reporting forced sex actually experience—there is evidence from other settings that boys reporting forced sex do not find the experience as distressing as girls who report the same. This does not invalidate the measure, but it would be interesting to further discuss this to help interpret the findings.

Response 3

We have stated more clearly that we measured only one form of sexual violence: forced or coerced sex. The specific question was: “Has anyone ever persuaded or forced you to have sex when you did not want to?”. This does include coercion that was not specifically physical force. We did not attempt to investigate in this study what the experience meant to the young people who suffered it, although we agree this is an important issue. The level of reporting is important. While boys who report forced sex might not find it as distressing as girls who report, it might be that boys who find the experience particularly distressing and emasculating are unable to report it. We do not know much about forced sex in male youth in Africa; it has been less studied than forced sex among female youth, sometimes on the assumption that it happens less.

Comment 4.

The difference in prevalence between girls and boys—it is consistent, and needs further discussion. It could be function of disclosure or could be a real difference, but it is at odds with most published

literature. I think it is also essential to present all data and fit all models for young men and women separately. Different epidemiology and to some extent theoretical explanations and risk factors for violence to me preclude presenting any combined estimates for 'children'.

Response 4

We have presented all figures for males and females separately and undertaken separate risk analyses for males and females. In the expanded Discussion we discuss the possible reasons for why we found an equal or higher rate of forced or coerced sex among the male students. It could be differential reporting, although we consider it more probable that under reporting would be more of an issue among males than among females. Another possibility is selection bias in that students who experienced forced sex, especially if it occurred in the school, might have left the school, and that this might have been more of an issue among girls than boys.

Comment 5. Time trends.

I'm not sure you can conclude that there is a time trend in this data. Almost none of the countries show any difference individually, and some are going in different directions—this makes a pooled average of questionable utility. What I think you are showing more convincingly is that there has not been any real decline. It would also be helpful to have information in variation between countries about age-specific prevalence to be clear that these are meaningful averages.

Response 5

We agree that the differences between two time points are not convincing of real decline. We have now stated this more clearly. Age-specific rates in individual countries are not very stable as the numbers for individual ages become relatively small. We dealt with this by age-standardisation based on the age structure of the 2007 Botswana male sample.

Comment 6. Risk factor analysis—exposure to sexual violence and school level measures of exposure to sexual violence.

I find this conceptually difficult, since the participants' experience is represented on both sides of the equation. What you are finding essentially is that there is clustering in sexual violence, which then begs the question of which variables then predict which schools are more violent. To me it would make far more sense to present results in this way. Same with the model where perpetration is the outcome and school level perpetration is the exposure. It is an interesting analysis though, and I think it is very interesting that you have added school and community level variables to the model.

Response 6

We agree the next question is to characterise the high occurrence schools. In this cross-sectional study, we are not drawing conclusions about which came first: individual experiences leading to school characteristics or the other way around. We believe the association between school level variables and individual experience is potentially important as it may open possibilities for intervention.