Author's response to reviews

Title: Changes in Sexual Behaviour among Young People associated with HIV Prevalence Decline in Zambia

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Author's response to reviews: see over
Response to reviewer 1

Reviewer: Geoff Garnett

General
This is a careful, well written analysis of changes in sexual behaviour in young, urban adults in Zambia, with a comparable rural population. The function of the manuscript is to look at behavioural risk factors related to an observed decline in HIV prevalence. The identification of 'confounding' variables that seem to explain the relationship between HIV risk and education is an interesting exercise relating distal to proximate determinants and I thought more could have been made of this in the discussion.

Response: Instead of examining the association between education and HIV risk we now examine the relationship between HIV and time. We have put in a paragraph in the discussion about this model and distal and proximate determinants.

Minor Essential Revisions
page 5 line minus 2 insert 'variable' after 'categorical'
Response: The 15-49 age group is no longer discussed in the paper so the part about adjusting for age as a categorical variable has been removed.

results line 5 start the paragraph with 'The proportion of...'
Response: Done

Page 8 line minus 8 it should be either 'used a condom' or 'used condoms' & Page 8 line minus 2; page 12 line 7 should be 'condom use'
Response: Done in these cases and in the rest of the article.

Page 12 line 12 'reporting bias'; and 'The finding that controlling for less...'
Response: This has been rephrased to “We found that controlling for less...”.

Discretionary Revisions (which the author can choose to ignore)
I think more could be made of the proximal determinants for HIV risk associated with education.
Response: See above
Response to reviewer 2
Reviewer: Anne Buve

General
As far as I understood the main message of the paper is that the decline in HIV prevalence in selected populations in Zambia between 1995 and 2003, can be explained by changes in sexual behaviour. A secondary objective of the paper seems to be to explore the mechanisms underlying the association between HIV infection and education, but only for the year 2003.

Response: The primary objective is to study trends in sexual behaviour among young people and assess whether the decline in HIV prevalence in the selected populations between 1995 and 2003 can be explained by changes in sexual behaviour. A secondary objective is to explore the association between sexual behaviour changes and education from 1995 to 2003.

Answering the first research question (is decline in HIV prevalence due to behaviour change (?)) is enough for a full paper. I suggest that the authors use the same statistical approach they used for unravelling the association between HIV and education, to explore the decline in HIV prevalence between 1995 and 2003. This would mean using year of the survey (1995, 1999 and 2003) as an explanatory variable for HIV and introducing various behavioural parameters into the model and explore how the association between HIV and year of survey changes. If the decline in HIV prevalence is indeed due to changes in sexual behaviour then the association between HIV and year of survey should weaken or disappear after introducing behavioural variables in the model.

Response: This has now been done. See table 4 and one paragraph in Results and one in Discussion.

Major Compulsory Revisions
1. The introduction has to be revised (see also my comments above) and the objectives of the paper should be more clearly stated. If the main objective is to explore the mechanisms underlying the declining trend in HIV in Zambia then the introduction should not start with a short review of the literature on the association between HIV and education. The
introduction should then concentrate on possible reasons for a decline in HIV prevalence and how other authors have addressed this issue. I miss for instance reference to the publications on Uganda and more recently Zimbabwe (S. Gregson et al. in Science).

Response: The introduction has now been revised. We believe that the association between HIV and education is essential to understand the mechanisms underlying the declining trend and would like to mention this but we have changed the order of the paragraphs. When the strongest declines are found among respondents with higher education, this strengthens the hypothesis that the prevalence decline is due to behaviour changes. We have now also mentioned previous research on behaviour changes and HIV declines, including the mentioned publications.

2. Methods p. 6, second paragraph. I suspect age at first sexual intercourse is not normally distributed. Moreover, one can only calculate a mean on the people who have already had sex. A more appropriate statistical approach would be to calculate medians with survival analysis.

Response: Thank you for this appropriate comment. Median age at sexual debut has now been calculated with survival analysis.

3. Methods p. 6, second paragraph. I miss data on marital status. Were respondents not asked about their marital status? Being married has been found to be a risk factor for HIV in several studies and age at first marriage is associated with educational attainment, especially in women.

Response: Being married is a risk factor for HIV also in this study. Our aim is however not to do a risk factor analysis, and we only mention HIV risk in relation to some of the behaviour variables to shed light on how changing behaviours affect HIV risk. But we have now stratified some of the analyses by single/married in response to this concern. See tables 3 and S5.

4. Methods p. 6, third and fourth paragraph. Here the focus shifts from explaining the declining trend of HIV, to explaining the association between HIV and education, but only for the year 2003. If the authors follow my suggestion to explore the association between HIV, year of survey and sexual behaviour, then (in principle) they could explore whether educational attainment is an interaction term.
Response: We don’t believe that education is an interaction term when time is the exposure variable and we adjust for behaviour as suggested. Contrasting trends between different educational levels of young people are most likely due to differences in behaviour. Sexually active people in Zambia of all educational groups seem to belong to the same sexual networks and are all exposed to a high HIV prevalence if they take risks. Adjusting for both education and behaviour would mean to adjust for the same twice and might mask important associations between behaviour and HIV infection.

We have, however, done the same modelling among respondents with higher education only as this group had a steep decline in HIV prevalence (Table S15). The HIV prevalence among those with less education was more stable and we could not proceed with the model.

5. Results p. 8. I would like to see a table that shows by year, the HIV prevalence, the % of young people ever having had sex, the % of young people with 2 or more partners in the past year etc. It gives the reader a quick insight in the data and makes it easier to follow the results section.

Response: Done (see Table 1)

6. Results, p. 8, third paragraph. There seems to be something really wrong with the data on condom use at last sex and condom use with the last casual partner in 1999. This issue is very briefly discussed in the discussion section, but I am afraid I find the possible explanations rather weak. Have any consistency checks been done on the variable condom use? The data give me the impression that the questions of condom use with last casual partner and at last sex were not well understood by the respondents, but why was that then a problem in 1999 only? Has the questionnaire been changed?

Response: Unfortunately we do not understand this ourselves. That is why the explanations provided may not be the most convincing. The questions were the same in all three surveys. All the interviewers were, however, not the same in the three surveys, but we still do not think that it is likely that the respondents had a particular problem understanding the questions in 1999. Data cleaning may, however, not have been as thorough in 1999 as in the two other surveys. That is why we mention data entry errors as a potential cause. We have now done some extra consistency checks. When tabulating ‘condom use at last sexual intercourse’ against ‘ever condom use’ we found that some respondents said ‘yes’ to the first question and ‘no’ to the second. We have therefore recoded these responses to ‘yes’ for
‘ever condom use’. You can see that the numbers for ‘ever condom use’ have changed slightly. It is however not possible to do the reverse, so the inconsistent pattern for condom use ‘at last sexual intercourse’ and ‘with the last casual partner’ remains. But as we write in the discussion we think that the problem is primarily related to these few questions. The reporting of other behaviours showed more consistency, and the parallel HIV prevalence decline, especially among higher-education groups, convince us that the trends observed in multiple partnerships and condom use from 1995 to 2003 are real.

7. Results, p. 9, second paragraph. Table 3 gives the data on ever given birth for the HIV negative women only. Please give the table for all women. And again, are there any data on marital status. I suspect most young women who have given birth are married or were married. The factor one really wants to look at is marital status.

Response: The table gave information on HIV negative women as changes in this group clearly show that reduced fertility is due to changes in behaviour rather than physiological changes. We have, however, now replaced it with a table with all women stratified by marital status. As you can see from the table, ever given birth remains strongly associated with education and declines most strongly among single women with higher education but also among urban married women with higher education. The former table 3 is now Table S4.

8. Discussion, p. 13, third paragraph. My gut feeling is that the doubling of HIV risk associated with child bearing is actually due to the women getting married. Can you please check this? Also delay in child bearing may be due to delay in marriage associated with higher education.

Response: Table S5 now shows that previous childbearing was associated with approximately double risk of HIV for single young women, and a non-significantly higher or equal risk for married young women.

Minor Essential Revisions
1. Methods and results. There is very little that is presented on the age group 15-49, so I would suggest to leave out any reference to that age group and concentrate on the young people 15-24 years old.

Response: Done.
2. Results, p. 10, end of second paragraph. Is there a difference in HIV prevalence among "virgins" between the different years?

Response: When pooling data from urban and rural men and women it was found that the risk of HIV infection for "virgins" was lower in 2003 than in 1999 (Table S14) (AOR 0.35 [0.16-0.76]). See supplementary table 14.
Response to reviewer 3
Reviewer: Basia Zaba

Discretionary
Drop all the stuff on first birth, it makes the article unfocussed and can only be dealt with in a very superficial way if it is limited to one figure and one paragraph of text.

Response: We think that looking at ever given birth is important in light of all the potential biases in studies of sexual behaviour. Ever given birth is more objective, less prone to reporting bias as childbearing is associated with high respect in this society and is difficult to keep secret. Changes in the proportion ever given birth shows that there have been some real changes in the sexual activity and/or contraceptive use. Furthermore, further analyses have been performed and are being included, see comments to reviewer 2.

Minor Essential Revisions
Rewrite abstract sentence: “More rural than urban and less versus higher educated young participants reported sexual experience in 2003.”

Response: The sentence has been rewritten as: “Participants from the rural area and those with less education reported more sexual experience than urban and higher-education participants in 2003.”

Laboratory analysis
Give indications of sensitivity and specificity of saliva tests – other researchers have reported that these are unreliable.

Response: More information, including specificity and sensitivity, has now been included in the paragraph. There is a substantive literature on saliva- based HIV testing showing that sensitivity/specificity is comparable to blood-based tests.

Data entry
Give details of methods used – e.g. double data entry, entry with verification, optical scanning, etc. – especially if later speculating that data entry may have been bad enough to distort trends!

Response: We have added at the end of the 1st paragraph of Methods that the data was double entered.
Data analysis

“Age adjustment was performed with a continuous age variable for the 15-24 age group, and a categorical for the 15-49 group.” Explain the categorical variable – e.g. five-year age groups?

*Response: The 15-49 age group is no longer discussed in the paper so the part about adjusting for age as a categorical variable has been removed.*

Figure 1

Label x-axis. Why does this figure show only males?

*Response: The x-axis is now labeled. We have also included women in the figure. We did not include women in the original version as this increase is not significant.*

ORs and AORs

Please give both in the tables where these are quoted. In some cases (e.g. years of education) the adjustment clearly makes a huge difference (since those with 10+ years education won’t include any 15 and 16 year olds) and it is useful to show this for people’s understanding of certain effects.

*Response: Done*

Major Compulsory Revisions

Differentiate between prevalence and incidence e.g. in the introduction it says “The best method to obtain indications of the effects of sexual behaviour change on HIV prevalence and incidence rate is to study how prevalence and sexual behaviour change in relation to each other in the same population [16].”

The only way to study incidence directly (i.e. without modelling) is to look at individual seroconversion relative to person-years exposure of HIV negatives. The methods section does not say if this was done or whether it was possible – do the study participants have individual identification numbers that allow them to be followed (anonymously) from one survey to the next? If not, then delete all references to incidence measurement. In fact right at the end of the discussion it is finally admitted that the study design is a set of cross-sectional (i.e. unlinked) studies – this needs to be stressed right at the beginning in methods section.
Response: The title is clearly stating that this is about prevalence decline. However, the reviewer is right in pointing this out and we realize that we had not discovered that we were erroneously mixing the terms prevalence and incidence. We have now corrected this by reformulating the sentence. We mention that prevalence changes can be used as a proxy of incidence among young people in the introduction. We have also mentioned under Introduction and Methods that we are reporting from cross sectional surveys which constitute an open cohort survey. Long periods between surveys and high mobility caused a problem studying incidence: small numbers and a highly selected group – so longitudinal data, eg. incidence, is not presented.

Selection and participation biases
Mention what characteristics were associated with interview refusal, with absenteeism, and with refusal to provide saliva samples. Did these change from one survey to the next? How did age structure of sample change between surveys? Were there other structural changes?
Response: More information on this has now been added. In the whole sample the most common reason for not being interviewed was absence because of school, admittance to hospital or temporary travel. 16% were absent in 1995, 28% in 1999 and 20% in 2003. In all three surveys eligible men were approximately twice as likely to be absent as women. In all the three surveys only 1-2% refused to be interviewed, and less than 10% refused to give saliva for testing. The refusal rates were similar for men and women. There were no big changes in the age distribution of those successfully interviewed throughout the period, but we report an increase in median age of rural men from 26 in 1995 to 28 in 2003. This information has now been included in the first paragraph of Results.

Pay more attention to reporting and sampling biases
page 10, results section says: “Interestingly, a comparison of the proportions that in 1999 and 2003 reported early debut, shows clear changes in reporting for men and young women. In 1999, 22% of female and 34% of male 15-19 years olds reported sex before age 15, but 4 years later, in 2003, 11% of female and 19% of male 19-23 years olds reported the same (uncorrected chi square: p<0.0001).” The figure shown in supplementary document 17 shows that this reporting error affects all age groups among men, but only the youngest age group of women – why not comment on this? This is a massive change, and potentially
undermines the interpretation of trends, as it either indicates that respondents were not giving true answers in one or other survey, or that the sample had changed so that the interviewed subjects were not the same people (e.g. because of migration). See additional ref on detecting the effects of this kind of reporting error. What about reporting changes in this variable between 1995 and 1999?

Response: For older men and women the differences in reporting were not significant, see Table S13. The apparent increase in reporting bias in reporting sexual debut forces us to be cautious in interpreting the findings. However, some of the findings indicate less reporting bias with more rural higher educated women admitting multiple partners and the HIV prevalence declining among self-reported virgins. We think that the problem is limited to just a few questions. The reporting of other behaviours showed more consistency, and the parallel HIV prevalence decline, especially among individuals with higher education, convince us that the trends observed in multiple partnerships and condom use from 1995 to 2003 are real.

The questions about ever sex and sexual debut were not included in the interview in 1995.

What about other “lifetime” variables – e.g. ever used condom, ever had casual partner, lifetime number of partners, ever gave birth, age at first birth … do they show logically consistent behaviour (increase, decrease or constant) when compared across members of same birth cohort interviewed in consecutive surveys? What about consistency between age at first sex and age at first birth?

Response: Of the mentioned parameters we only have life time information about ever condom use and ever given birth. Reporting of ever condom use and ever given birth increased consistently as the youngest age cohorts grew older between 1995 and 2003 as would be expected as the cohorts grow older (Figures S2 and S3). We do not have information about age at first birth and could therefore not do this consistency check.

Trends don’t have to be linear to be “true”

page 13 discussion says “Such inconsistencies disturbing linearity in trend may indicate random variation between the surveys, changes in the sample due to migration, or data entry errors.” Is it conceivable that data entry was so bad that it actually distorted trends? What about changes in social desirability bias, enumerator training, questionnaire design, participation rates?
Response: Unfortunately we do not understand this ourselves. That is why the explanations provided may not be the most convincing. The questions were the same in the three surveys. All the interviewers were, however, not the same in the three surveys, but we still do not think that it is likely that the respondents had a particular problem understanding the questions in 1999. Data cleaning may, however, not have been as thorough in 1999 as in the two other surveys. That is why we mention data entry errors as a potential cause. We have now also added that it may be due changes due to higher absence in 1999 and reporting bias. But as we write in the discussion we think that the problem is primarily related to these few questions. The reporting of other behaviours showed more consistency, and the parallel HIV prevalence decline, especially among individuals with higher education, convince us that the trends observed in multiple partnerships and condom use from 1995 to 2003 are real.

Effect of delay in first pregnancy

Bottom of page 13, discussion, says: “Ever given birth was associated with doubling of the likelihood of being HIV infected, which supports the interpretation that delay of first pregnancy may be an effective preventive strategy [28, 30-34].” Authors go on to discuss hypothetical contribution of abstinence, condom use and hormonal contraceptive use. If all these things were measured why can’t they report contribution of each to delay in first pregnancy? What about marriage? What proportion of first births occurred more than 9 months after first marriage? What proportion within 9 months (presumably precipitating marriage), what proportion of births are pre-marital? What is association of HIV infection with these different timings? Please note literature on effect of years of pre-marital sexual exposure and years of exposure to sex after marriage. Response: We have now examined the confounding effect of condom use, contraceptive use and sexual activity the last year on the declining proportion ever given birth, see Table S6. We have also stratified both the analyses of ever given birth by education and HIV risk by ever given birth by marital status. As we do not have the information on age at birth and births in relation to marriage, we cannot do the other mentioned calculations.

Continuing to top of page 14 “There was a bigger reduction in the proportion ever given birth than in the proportion admitting having sex”

Give proportionate declines and say what ages this corresponds to
Response: When we stratified the analyses of ever given birth by marital status we did no longer find a consistent pattern across groups, so we have removed this sentence.

Figure 2: Why is it limited to HIV negative women? What is the relevance of this figure for the text as a whole?
Response: As we found postponement of first birth among HIV negative women too it clearly indicates that decreased fertility among young women is due to behaviour change, rather than a physiological effect of HIV infection.

Condom use
Page 14 says “Its use at last sexual intercourse is often used as an indicator of consistency, but has obvious limitations and may not give a representative picture of how many always use condoms.”
No, use of condom at last sex is often used as an indicator of frequency of use in the population, on the assumption that last sex is typical of all sex. Consistency of use can only be measured if questions are asked about use on more than one occasion. See ref on measurement of condom use.
Response: Thank you for this comment. We have now corrected this.

Supplementary materials
Surely these have to be alluded to in the text somewhere, or how will people know what is available and why? What is the point of all these extra tables, and why can't they all go into one supplementary document?
Response: All the supplementary tables have been collected in one document and references to them have been put in the text.