

Author's response to reviews

Title: The effect of participant non-response on the HIV prevalence estimates in a population based survey in two informal settlements in Nairobi city

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Author's response to reviews: see over

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Editorial Team

Population Health Metrics Journal

Re: MS: 2133964686287578- The effect of participant non-response on the HIV prevalence estimates in a population based survey in two informal settlements in Nairobi city

Dear Editor,

In response to your email of November 5, 2009, we are pleased to submit a revised version of the above mentioned manuscript with point by point responses to the comments raised by the reviewers. The comments have been very useful.

Reviewer: 1

Reviewer's comment:

***Background**

There appears to be incorrect references, for example the first sentence in the background “Selective participation in a study can significantly skew estimates of the outcome of interest in a study population”. References 1 and 3 do not say this. In fact the conclusions are in the specific studies they looked at (DHS) there was little effect on the estimates after adjusting for non-response. Reference 2 only speculates about the possibility of a effect. Authors need to look through and check that the referencing is correct.

Response to reviewer's comment

It is true the conclusions in those papers indicate that the overall effect is small. However taking a closer the data tables you realise that the observed prevalences and the predicted prevalence for some countries significantly differed (Mishra et al, 2006, table 3). Indeed where non-response was high like the case of Lesotho, the effect was greater (Marston et al 2008, table 3). The authors argue that were it not for high rates of participations, the non-response could have affected the estimates more, meaning that the potential for substantial bias can be real particularly when the non-response rates are high. For those reasons we have maintained those references and added two references by Reniers et al 2009. The wording of the preceding sentence has also been slightly modified.

Reviewer's comment:

Final paragraph [0] is written in the middle of penultimate sentence.

Response to reviewer's comment

The authors searched the manuscript but could not find the [0] alluded to above.

Reviewer's comment:

***Methodology**

Could the Authors add a description of how they go about the HIV testing. For example how many times they visit the household, whether they ask the occupants where the person is etc

Response to reviewer's comment

The methods section has been re-written to give more details on how participants were traced and interviewed.

Reviewer's comment:

Reference 19: Where is this? The reviewer tried to find it but can only find references to it rather than the actual location of the publication. The APHRC website does not seem to have a link to it either.

Response to reviewer's comment

True this report is not on the site and has not been published yet. Colleagues have prepared a peer reviewed publication from the report but not yet available for reference. In the meantime the report is the best reference. However if the publication comes through sooner than later, the reference will change to the peer reviewed one. Nonetheless the methods section has been expanded to provide more details.

Reviewer's comment:

Stata should be written as Stata not "STATA"

Response to reviewer's comment

Point well taken and correction made.

Reviewer's comment:

The authors should mention whether the run of imputations in ice in Stata was enough for the estimates to stabilise rather than running an arbitrary number according to the recommendation of the writers of the software (this might be a good recommendation but does not necessarily apply to all data).

Response to reviewer's comment

The authors initially followed the recommendation of the authors of the ice program (5 imputations), but just to ensure that estimates were more stable we doubled the imputations to 10.

Reviewer's comment:

I am not sure the risk ratio method (a) is really necessary here since the actual multiple imputation method is being used. If the authors want to keep it, it should be discussed, perhaps linking it in to the idea that the non responders have a characteristic that has not been measured therefore the multiple imputation method would not be correct.

Response to reviewer's comment

We took the reviewer's advice and deleted the referred to above.

Reviewer's comment:

*Results

End of first paragraph last sentence should read (addition in bold) "There were no significant differences between those who accepted to test and those...."

Response to reviewer's comment

Thank you. Correction made in the text

Reviewer's comment:

Table 2: Why is the mobility variable not listed here?

Response to reviewer's comment

The mobility variable has been added to table 2.

Reviewer's comment:

Table 6: This is not clear the title “prevalence of HIV: Observed and adjusted” needs lengthening to explain what is in the table. Also the left hand column needs to make clearer what each row represents. I am assuming that the first row “a” is the actual unadjusted prevalence. Rows b-d are imputed.

1. Is row “e” groups b-d together?
2. Is row “f” the actual results from “a” plus the imputed results from “e” added together

Response to reviewer's comment

The title has been expanded and now reads: “Table 6: Observed, imputed and adjusted prevalence of HIV”

Reviewer's interpretation was spot on. We have however added more detail in the column on the left of table 6 for clarity as suggested by the reviewer.

Reviewer's comment:

*Discussion

Paragraph 5: When talking about the “gender angle”. Why would the over and underestimating be due to the varying prevalence levels by gender? The prevalence is of the same order of magnitude even if it is different by gender.

Surely it would be more to do with differences between men and women and whether they are in the household or not at time of interview.

Response to reviewer's comment

Point noted and the last sentence in the paragraph as been deleted.

Reviewer's comment:

- Discretionary Revisions

These are recommendations for improvement which the author can choose to ignore. For example clarifications, data that would be useful but not essential.

It might be interesting to give a brief description of levels of mobility (i.e. what was the distribution before you grouped them into highly mobile and not highly mobile?)

Response to reviewer's comment

More detail on the description of mobility has been added to the methods section. Mobility was proxied by number of times an individual changed residence within and without the demographic surveillance area per unit time. An individual was considered to be highly mobile if they had at least one or more episodes of change of residence per year or at least one outmigration and return episode to the surveillance area in two years. About 99.98% of the score distribution ranged between 0 and 5 and 82% between 0 and 1. Because of skewness we decided to dichotomise the outcome to “not highly mobile” (score 0-1) and “highly mobile” (score above 1)

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Reviewer 2

Reviewer's comment

Population-based HIV prevalence surveys are an important vehicle for assessment of the size and course of the epidemic in African countries with generalized epidemics. Non-response due to absence or refusal may lead to under and overestimates depending on the relative HIV prevalence among non-responders to responders. This issue that has been addressed in the context of DHS surveys, and it was concluded that the potential effect of non-response was relatively small in most circumstances if non-response rates were below 25% or so.

This paper contributes two new perspectives to this issue. First, it is concerned with urban slum populations, which are often missed or underrepresented in DHS or similar surveys. Non-response is likely to be a larger issue. Second, the HIV survey is nested in a DSS, which implies that good data could be available on mobility from an independent source.

The size of the bias depends on how different the HIV prevalence is between the two groups, which in the analysis is approached by:

- (1) describing how different the absentees are from participants; how different refusals are from participants - both are done extensively in the paper
- (2) describing how different the HIV prevalence is by major background characteristics - the paper should provide more detail here.
- (3) applying (1) and (2) to get the best estimate of the bias for absentees and refusal and both combined.

Response to reviewer's comment

Thank you for the remarks. More detail in the results section describing HIV prevalence by major background characteristics have been provided.

Reviewer's comment

The paper should describe in greater detail the biases by individual variables and whether or not they cancel out. In general, within each sex stratum HIV prevalence variation is greatest by age (typical age patterns by sex, with e.g. five to tenfold differences in HIV prevalence between some age groups), to a lesser extent marital status (widows), in this case ethnic group (given the very uneven distribution of prevalence within Kenya). Other variables like education and wealth especially within the same area are less likely to have an impact.

Response to reviewer's comment

More detailed description has been given based on revised table 2 with HIV prevalence by individual socio-demographic characteristics. Age, gender, residence, ethnicity, education and marital status were associated with HIV status. Differential participation along these variables therefore has the potential to affect estimates but overall effect may cancel out because of different directions of bias.

Reviewer's comment

Table 2 should be expanded - adding a column with the HIV prevalence among the survey participants for each of the categories - this would probably show why the adjustment is small. All other adjustments for variation are unfortunately more or less speculative.

Response to reviewer's comment

Column for HIV prevalence has been added to table 2 as proposed by the reviewer to aid further discussion for potential bias by individual socio-demographic variables.

Reviewer's comment

Mobility index may be important, although it often is not a lead determinants compared to age in many studies. This study should be able to throw more light on the different types of mobility that have resulted in non-response, based on the DSS or other work conducted in this study. If there are no clear HIV-associated determinants of mobility /non-contacting, then indeed the effect on the overall prevalence estimate is likely to be small.

Response to reviewer's comment

Our experience with the DSS is that there are both internal (change of place of abode but within the demographic surveillance area) and external movements (Change place of abode outside of the demographic surveillance area-DSA). Movements outside of the DSA may be to other parts of the city, other urban or rural areas but often the destination is not known. A study in South Africa showed a relationship between rural return migration and HIV/AIDS related mortality, meaning that migrants had a high burden of HIV. This has not been examined in Kenya.

These points have been expanded in the discussion section of the paper

Reviewer's comment

I do not find Table 5 very helpful. The main point of the paper is to come up with the best estimate of RR of HIV infection among those that were absent and among those that refused (present separately) and as well as provide the best overall estimate of the RR among the non-participants.

Response to reviewer's comment

Table 5 and text related to it have been deleted.