Author’s response to reviews

Title: A population-based study of HIV in south India reveals major differences from sentinel surveillance based estimates

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Author’s response to reviews: see over
Dear BMC Editors,

Thank you for inviting revision of this manuscript. The revised manuscript is submitted. We appreciate the reviewers’ comments. Our response to these comments:

Reviewer: Geoff Garnett

1. We have intentionally written a long paper with full description and supporting data, as the findings were unexpected and we would like full detail available for scrutiny by the readers. BMC journals are online and provide the opportunity to present details that may not be possible in most print journals. The reader can chose to scrutinize the detail or to ignore it. We believe that the option of presenting details in the online BMC journals is an advantage rather than a handicap. Specifically:

The details of HIV testing are often not presented in epidemiology papers. Presentation of this detail is a strength of this paper as it allows the reader to understand the attention that we paid to the laboratory methods. Figure 3 presents results and complements the description of the methods.

Table 1 shows the method used by NACO for the state level HIV estimation. When we presented this paper to different audience over the past few months, initially without this table, it was pointed out that the NACO estimation method should be explained in the initial part of the paper to lay the foundation for subsequent comparison with the population-based method. Table 9 shows comparison of the sentinel surveillance and population-based methods at the district level.

We believe that Table 10 is essential because often over the past few months while presenting these data we have been told that there are other studies from India that show that antenatal sentinel surveillance and population-based HIV prevalence are comparable. This table shows the reasons why the previous studies cannot be used for such an inference, and therefore is important.

Again, based on presentation of this paper to various audience we have been prompted to provide details of the HIV distribution by SLI and age, which we do in Tables 3 and 4. Tables 5 and 6 cannot be combined as Table 5 shows important detail by SLI quartiles, whereas Table 6 shows HIV rates by SLI halves as the number of HIV positive pregnant women were small for quartiles. Attempts at merging these two tables would lead to loss of detail, which we would like to avoid.

Table 2 presents important detail regarding HIV prevalence by gender and study areas, which we believe is more easily understood as a table instead of lengthy descriptive text.

Table 8 shows important information about referral to public hospitals. As this was estimated to be a major cause of distortion of antenatal HIV prevalence in public hospitals, we believe presentation of this table highlights this issue.

We assessed the four and a half pages of discussion and could not find unnecessary repetition. The issues presented are directly pertinent to the data presented and its interpretation.
In brief, regarding the suggestion to shorten this paper, we believe that doing so would lead to loss of important and useful detail that supports the unexpected findings of this study. Additionally, BMC online journals allow full length papers.

2. We have described each step of the sampling on pages 5-6. We have now changed “Systematic random sampling” to “Systematic sampling, with the first number drawn randomly, was done....” on page 6.

3. HIV rate is now replaced with HIV prevalence throughout the paper, as suggested.

4. The “men” issue related to STI clinic HIV data is now highlighted on page 19 (third paragraph): “The STI component was originally included in the NACO method with the assumption that hidden high-risk groups, particularly among men, may not be reflected in the antenatal HIV component.”

5. We tried to bring in comparison with the Africa ANC-SES-HIV issue as suggested, but could not find published data regarding the specific issue mentioned by the reviewer. Also, properly addressing the ANC versus SES versus HIV context in Africa would require addressing a bunch of related issues. This would require extensive further discussion, which does not fit in well with this paper in our view.

6. We have now taken out the reference to the equilibrium possibility on page 21.

7. We have now taken out the reference to unpublished DHS survey results from Africa.

8. This is a complicated paper in which explanations have often required long sentences to connect various issues. Further changes in language could perhaps be explored at the copy-editing stage of the paper as long as the intended meaning is not lost.

Reviewer: J. Ties Boerma

Regarding the comment on the paper being lengthy, we have explained above the reasons why this is necessary.

We have also shortened the abstract as suggested by the editors.

We thank BMC for considering our paper for publication. We would be pleased to respond promptly if any further input were needed from us to arrive at a rapid decision on this paper.

Kind regards,

Lalit Dandona

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