Reviewer's report

Title: Maternal Mortality in South Africa in 2001: From Demographic Census to Epidemiological Investigation

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Reviewer: Margaret Hogan

Reviewer's report:

This paper presents a maternal mortality analysis of a 10% sample of the 2001 South African census, presenting both estimates of major maternal mortality indicators as well as an investigation into disparities in these indicators. A major strength of the paper is the use of multiple and diverse data sources to support the findings. This is a valuable paper, but some points ought to be addressed before the paper can be considered for publication.

Major compulsory revisions:

1. In the Methods section, much more elaboration is required in the “analysis of biases” section. A clear, concise identification of the biases which were considered important, and how those biases were investigated and adjusted for, should be added. For example, clearer discussion of how the authors “exhaustively analysed these imputations and concluded...no sign of bias by household characteristics” is needed.

2. Also in the Methods section, how the matching was carried out between the maternal death “cases” and the controls should be more clearly explained. What were the exact matching variables used? How many controls were matched to each case? Would it be possible for two women in the same household to be matched to one another? Without this information it is difficult to determine how appropriate this approach is; it would also be useful to have some discussion of why this approach was chosen and why it is considered advantageous.

3. The use of the income variable as a major socioeconomic factor is worrying as self-reported income is known to be problematic in settings like South Africa. This is also evidenced by the fact that more than a quarter of maternal deaths did not have a reported income. The wealth index is more promising, but appears to be a simple count of the number of assets owned; the use of factor analysis (as in the DHS) or another existing method of combining this information may be more appropriate. It may also be practical, given the small number of maternal deaths in the existing 16 categories of the wealth index, to break this variable into fewer groups (the standard quintile breakdown of a wealth index may be more useful) and treat it is as categorical variable in the multivariate analysis.

4. The description of how birth rates were estimated from the census was unclear to this reader, and the interpretation of results, particularly with regard to the month of October, is difficult to understand. I would recommend revision to clarify what exactly was done.
5. A clearer description of how 95% CIs were calculated is needed.

Minor Essential Revisions

6. The paper is too long and should be cut down before publication. Some suggestions of how this could be accomplished include:
   • Reduce the “definition of maternal” deaths section, perhaps into tabular form. These definitions are known to those in the field and easily available to those unfamiliar. However, it may also be useful in this section to clarify the distinction between maternal causes of death and pregnancy-related causes of death (which is what is typically measured in a census);
   • Refocus the introduction on a discussion of why this study is particularly interesting in South Africa (i.e., what is the local context that makes this interesting and relevant, and perhaps important for other settings). The introduction’s current focus on the global context is similar to other studies with a global analysis (such as Hill’s Lancet paper);
   • It is unnecessary to present the results in both figures and tables (as in Table 2 and Figure 1) – the Tables are more useful as they also present confidence intervals around the estimates;
   • Reorganize the results to first present all results from this study, and then have one section comparing this analysis to other sources. The way it is organized is confusing, especially as further comparisons are presented in the discussion.

7. What does “harmonized” mean with regards to the IPUMS 10% sample?

8. What was the specific question asked in the census used to identify “maternal” deaths?

9. The methods section should specify how p-values are calculated for comparisons between groups in the MM differentials section.

10. The paper would benefit from revision by a native English speaker as in some places the language used is confusing and makes the interpretation difficult. For example, the use of the term “gradient” in the MM differentials section is misleading, as these are not gradients but relative risks.

11. In Table 2, why is there no RR for English language? I would also highly recommend including confidence intervals around the reported relative risks.

Discretionary Revisions

12. It may be useful to consolidate the multiple estimates of maternal mortality from various sources into a single table, which can then be referred to in the text to show that the estimates from this study are consistent with other sources.

13. Why is the MV analysis considered potentially misleading?

Level of interest: An article whose findings are important to those with closely related research interests

Quality of written English: Needs some language corrections before being
published

**Statistical review:** No, the manuscript does not need to be seen by a statistician.