Reviewer's report

Title: Childhood mortality in the Democratic Republic of Congo: Cross-sectional evidence of the effect of geographic location and prolong conflict from a national household survey

Version: 2
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Reviewer: Matthew Coldiron

Reviewer's report:

This revised manuscript is an improvement on the previous submission, but many of my prior concerns remain. The manuscript (though shorter) is still too long, and studded with unsubstantiated socio-political commentary, that, however valid, is not appropriate for scientific publication. The authors present conclusions in a definitive manner that I believe is not warranted by the results of their analyses, which is problematic.

The lengths of the respective sections still seem disproportional. Introduction: 5+ pages, Methods: only 2 pages, Results: 3 pages (though with several tables/figures), Discussion 5+ pages. Given the authors' statements about using novel methodology, I would have expected a more lengthy description of their methods, particularly as over half of the methods section describes the DHS data collection, which was not performed by the authors and which is well-described in the 2007 DHS report.

Major compulsory revisions

Introduction:
This section remains long. The authors make several statements which need to be referenced, and others where I question the validity of the references cited for the statements they make.

Paragraph 1:
- "Child mortality rate is considered the best proxy indicator..." should be referenced.

Paragraph 2:
- "One is that the country relies upon a physical and human health indicator..." ends with a likely true but subjective statement regarding poor governance and economic mismanagement which is not supported by the two references cited.
- The following sentence suggests that only the rural areas and certain providence of the DRC lack good health infrastructure. I do not believe this is the case, but regardless, the authors would be advised to provide a reference for their assertion.

Paragraph 3:
- The last two sentences of the paragraph are commentaries on the current political situation in DRC. The final sentence suggests that the militias are the only cause of the collapse of the health system, which is an oversimplification.

Paragraph 5:
- The presentation of U5MR as estimated by various surveys is appropriate in the introduction. On the other hand, this data is presented again in Table 2, which is referenced in the text in the results section. Once is sufficient.
- The sentence which begins "Little progress is made in the implementation..." has no reference and is an overt political commentary which has no place in the scientific literature.

The first two paragraphs of the section "Background on health conditions in the DRC" actually discuss economic development indicators. The entire section could be shortened into one paragraph and integrated into the prior section.

The final paragraph of the introduction makes many unreferenced statements, which often repeat facts/data/assertions made previously.

Methods:
- The description of the DHS data collection methods is disjointed and contains subjective commentaries on the quality of the data.
- The description of the statistical analysis is quite technical. I am not qualified to comment on the precise methods, but I do believe that the description of the methods could be expanded, possibly with easier-to-interpret descriptions of the methods used, as the average reader of BMC Public Health is not likely to have the ability to critically interpret the methods described.

Results:

Table 1 is not necessary.
Table 2 would be better described in the introduction.
Table 3: I note that the estimates of U5MR differ from those published by DHS. The methods do not clearly state how mortality is estimated. (By my reading, the methods described apply to the different regression models presented in Table 4.) I am not an expert in this field, so do not wish to comment on the validity of the methods used or results presented, but I do believe that a more comprehensive explanation of the methods used to estimated mortality, as well as a description of why these methods would produce different estimations than the DHS data, would be in order and helpful to the reader.

Table 4/Figure 2: I do not understand the assertion that living in North Kivu is associated with higher childhood survival given that the confidence intervals for the AOR cross 1. Furthermore, the statements made on page 11 regarding excess mortality in Maniema, Kasai Orientale, and Katanga are confusing - in the adjusted model, these regions do not have higher mortality.

Discussion and conclusion
The authors draw conclusions that I believe are not supported by the data they present, particularly regarding the differences in mortality by provinces. Their hypothesis that U5MR is lower in North Kivu because of the presence of humanitarian actors is possible, but I think that a more likely explanation would be that the DHS survey was not able to access the entire province (as alluded to in the methods section). The discussion of the limitations of the data is bizarre, the second paragraph actually describes problems with the 2001 MICS survey, which was not used in the present analysis.

In the end, I am not able to comment on the validity of the methods used, and if this article is further considered, a statistician should assess this. However, I believe that, based on the results presented, the authors overstate their conclusions.

**Level of interest:** An article of limited interest

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

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.