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

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Author's response to reviews: see over
Dear Jane M. Dalumpines,

Please find enclosed our revised manuscript, which addresses the reviewer’s concerns and suggestions. What follows is a point-by-point response to the comments provided as part of the review process. Each group of responses has been numbered to correspond with those on the comments. Moreover, in the revised manuscript we have highlighted in red the areas that have been modified compared to the original submission. Finally, we appreciate that some of the additional analytical detail we have provided may be excessive for some of your readership and you may feel that some of this could be formatted as a footnote or put in the appendix.

Editor's Comments:

"Unfortunately, the concerns of the reviewer have not sufficiently been addressed in this revision and additional major revisions are required."

Reply: Thanks for your kind comments. We appreciate the reviewer’s concerns and we have made substantial changes from the previous version of our paper.

Additional Editorial Requests:

1.) Copyediting

After reading through your manuscript, we feel that the quality of written English needs to be improved before the manuscript can be considered further.

We advise you to seek the assistance of a fluent English speaking colleague, or to have a professional editing service correct your language. Please ensure that particular attention is paid to the abstract.

For authors who wish to have the language in their manuscript edited by a native-English speaker with scientific expertise, BioMed Central recommends Edanz (http://www.edanzediting.com/bmc1). BioMed Central has negotiated a 10% discount to the fee charged to BioMed Central authors by Edanz. Use of an editing service is neither a requirement nor a guarantee of acceptance for publication. For more information, see our FAQ on language editing services at http://www.biomedcentral.com/authors/authorfaq.

Reply: Thanks for your kind comments. The manuscript has also been edited by a professional copy editor.
2.) Kindly reduce your article to no more than 3500 words.  
Reply: The word count on the manuscript has been reduced to 3500 words as suggested.
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We would be grateful if you could address the comments in a revised manuscript and provide a cover letter giving a point-by-point response to the concerns.

Reply: A cover letter is attached.

Please also ensure that your revised manuscript conforms to the journal style (http://www.biomedcentral.com/info/ifora/medicine_journals). It is important that your files are correctly formatted.

Reply: We have conformed to the journal style as suggested.

We look forward to receiving your revised manuscript by 5 January 2014. If you imagine that it will take longer to prepare please give us some estimate of when we can expect it.

You should upload your cover letter and revised manuscript through http://www.biomedcentral.com/manuscript/login/man.asp?txt_nav=man&txt_man_id=138903557011517. You will find more detailed instructions at the base of this email.

Please don't hesitate to contact me if you have any problems or questions regarding your manuscript.

Reviewer's report:

Reviewer #1: Matthew Coldiron

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.

Reply: We would like to thank the reviewer for his remarks and comments on our manuscript. Following the reviewer's and editor's suggestion, we have now shortened the manuscript dramatically to 3500 words by removing the section on background of the study area and by shortening the conclusion. Methods section has been also reduced and we have pointed out references where further readings can be found. An appendix has been added to describe in more details the methodology used. It is up to the editor to decide whether details of the statistical methodologies are excessive for the readership of this journal or they can be put in the appendix.

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.
Reply: A reference has been added as suggested including the check of the validity of
statements.

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.
Reply: The sentence has been revised to read “...decades of protracted conflict, poor
governance and economic mismanagement [8-12].”

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.
Reply: We have deleted the redundant text and mentioned it only once in the introduction.
Table 2 remains for ease of flow of tables in the text.
- 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.
Reply: in the sentence that begins with “Little progress is made in the implementation...” we
were referring to progress in terms of development indicators and reduction in U5MRs and
not making a political statement. The sentence has been revised as shown below and
references have been given.

“Little progress is made in the implementation of the Government’s Priority Action Plan on the
National Acceleration framework to reduce infant and maternal mortality in the DRC as
indicated by higher U5MRs and the last ranking in terms of all human development indicators
[17-23, 25].”

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.
Reply: The section “Background on health conditions in the DRC” has been removed as
suggested.

The final paragraph of the introduction makes many unreferenced statements, which often
repeat facts/data/assertions made previously.
Reply: We have revised the paragraph and inserted the appropriate references.

Methods:
- The description of the DHS data collection methods is disjointed and contains subjective
commentaries on the quality of the data.
Reply: The description of the DHS data collection methods we provided in the methods
section is taken from the 2007 DRC DHS final report and we have made a reference to it in
the section. We are not sure exactly sure to what the reviewer is referring to in his comment
on “subjective commentaries on the quality of the data.” The DHS data quality is widely acknowledged in the literature and discussed in every DHS report.

- 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.

Reply: We have simplified the technical details of the statistical methods in the manuscript for the average reader and provided a detailed description of methods in the appendix for advanced readers.

Results:
Table 1 is not necessary.

Reply: We disagree with the reviewer. We believe that Table 1 is needed to put the DRC U5MRs into context and to highlight our observation in the manuscript that progress has not been made in reducing U5MRs compared to other countries in the region.

Table 2 would be better described in the introduction.

Reply: we have amended the text and described Table 2 in the introduction as suggested.

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

Reply: The reviewer is right, U5MR presented in Table 3 differ from those published by DHS report. The slight difference in the rates is simply because we used un-weighted data in our analysis in line with the statistical techniques used and the DHS used the weighted data. We have now made it clear in the heading of Table 3. We have amended the text and described Table 2.

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.

Reply: We appreciate the reviewer’s comment. However, of note is the fact that the second reviewer of this paper Prof Samuel Manda is an expert in this area and has reviewed the statistical techniques we used. Please refer also to our reply above on why our rates differ from the DHS report. Moreover, the DHS rates are from a bivariate association and hence the need to use weighted data. The Bayesian hierarchical modelling is a robust technique that does not require weighted data.

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.

Reply: North Kivu is associated with higher childhood survival from our results in Table 3 (bivariate analysis). For this reason, North Kivu was considered as a reference province for the multivariate analysis. All results stated in the text have been cross reference to whether they pertain to the bivariate analysis, unadjusted and multivariate Bayesian analysis. However, the reviewer is right by saying that the confidence intervals for the AOR cross 1 in the full multivariate Bayesian analysis. However, the results of the full multivariate Bayesian analysis do not invalidate results from the bivariate and unadjusted analysis. As long as the appropriate reference is made, we believe that the confusion is removed for the readers.
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).

Reply: We thank the reviewer for this important observation. The discussion and conclusion have been amended accordingly to reflect the reviewer's suggestion.

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.

Reply: We have removed the mention to the 2001 MICS survey as suggested.

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.

Reply: Fortunately, the second reviewer of this paper, Prof Samuel Manda, is an expert in this area of statistics and we have responded satisfactory to his comments/suggestions.

Level of interest: An article of limited interest
Quality of written English: Needs some language corrections before being published
Reply: The manuscript has been edited by a professional copy editor.

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.

We would like to thank the reviewer for further thoughtful comments and suggestions. We believe that as a result of the review process our paper has greatly improved and hope that it is now acceptable for publication in BMC Public Health.

Yours sincerely,

Ngianga-Bakwin Kandala