Author’s response to reviews

Title: Socioeconomic and Environmental Determinants of Under-Five Mortality in Gamo Gofa Zone, Southern Ethiopia: A matched case control study

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Author’s response to reviews:

Response to reviewer 1

Overall, I think the authors are misapplying the work by Henry Mosley and Lincoln Chen (extract found here:

http://apps.who.int/iris/bitstream/10665/71801/1/bulletin_2003_81%282%29_140-145.pdf),

and/or misinterpreting concerns raised in Victora et al, found here: https://www.ncbi.nlm.nih.gov/pubmed/9126524). For the objective of this manuscript, and in terms of potential policy intervention, I believe the appropriate and final model would be a single model (similar to the example model 3 in Victora et al) where effects of these individual social, economic, and environmental factors are assessed in the presences of the others, acknowledging that they are not accounting for other potential intermediate factors (which there are several of them). This means that the final effect sizes to be reported in the Results section of this manuscript would be as in Table 3 currently, but also including the effect sizes (as estimated from this final model) of the selected variables from model in Table 2 (i.e. sex of the child, mother's education, wealth index, husband occupation and marital status of the mother). As it is, it is misleading, in this case, to report separate ORs for 'distal' factors as done in Table 2, and then add additional variables to those selected "socioeconomic" related factors from model in Table 2 but then turn to report (as in Table 3) only the ORs of these additional variables in the presence of those brought from Table 2 using p-value of 0.1.

Thank you very much once again for your critical comments which we feel is crucial for improving the manuscript further. We accepted your suggestion and re-executed two models for the distal factors (one without proximate factors and the other with relevant and significant proximate factors both for under-five and infants). We have reflected this revision in the method (line number 247-252). We presented both models so that the readers will have the full picture. Except for slight change in the value of odds ratios, no significant difference was observed in the
two models. However, marital status in the recent form (in two categories) tend to be significantly affecting both under-five and infant mortality in the model with proximate factors. Similarly, the odds of death among both under five children and infants whose father were merchant was significantly higher than those whose fathers were farmers in the model with proximate factors. Accordingly we revised table 2 (previous odds ratio values also slightly changed in this table as marital status in the current model was re-coded as two categories) and the corresponding narration in the abstract (line number 31-34), results (line number 285-311) and discussion (359-368).

I don't quite agree with the authors' reason for not including household's main cooking fuel in the model. Again, given the aim of this paper, I am sure the authors would agree with me that it would be useful for policy makers to know which areas (or variables) they could focus their limited resources in order to achieve maximum gain in reducing child deaths. Thus, knowing which solid fuel type has the highest adverse (or otherwise) impact on child deaths in this setting would equally be useful for policy purposes. So I'd still suggest they include in the model the individual fuel types (i.e. wood, animal dung, and charcoal) as categorical variables with one serving as a reference category.

We agree with you and that is why we tried to collect this information. However, as we reported in our previous response, this variable doesn’t well differentiate the two groups in our study community. Majority of the households in the study area were using wood as source of fuel for cooking, more or less this is true both for cases and controls (see the following table), which lead to a very wide confidence interval for those categories with small sample size and made the models unstable. The results were also statically insignificant when we compare animal dung and charcoal with wood (which may be because of the small sample size of the categories). So we insisted not include this variable in the model.

<table>
<thead>
<tr>
<th>Material for cooking</th>
<th>Alive</th>
<th>Dead</th>
<th>Total</th>
</tr>
</thead>
<tbody>
<tr>
<td>wood</td>
<td>682</td>
<td>351</td>
<td>1033</td>
</tr>
<tr>
<td>animal dung</td>
<td>8</td>
<td>3</td>
<td>11</td>
</tr>
<tr>
<td>charcoal</td>
<td>68</td>
<td>27</td>
<td>95</td>
</tr>
<tr>
<td>electricity</td>
<td>4</td>
<td>0</td>
<td>4</td>
</tr>
<tr>
<td>Total</td>
<td>762</td>
<td>381</td>
<td>1143</td>
</tr>
</tbody>
</table>

It is still not clear what proportion of the distribution in Table 1 accounts for the Under-5s vs. infants (these can't possibly be the same).
Thank you for the reminder, 69.03% of the children were under one year of age. We added this statement in the result part (line number 271).

Since the authors are also concerned with infant mortality, then it'd be appropriate to tweak the main title, and subtitles in the Results to reflect both infant and under-5 mortality.

We appreciate your concern. However, as the main targets of this study were under-five children and the sampling and sample size determination were based on information from under-five children, the analysis for infants is a secondary analysis. So we opt to reflect only under-five in the title.

The sample sizes for Marital Status categories 'Single' and 'Others (separated/divorced or widowed)” are individually too small (hence the wide CI). It would make sense to combine them as they did for Educational status.

We accepted your suggestion and re-categorized marital status in to two (married vs not married) (table 2). Accordingly we have revised the results (line number 294-297) and discussion parts (line number 359-368). However, marital status in the current form (in two categories) for table 3 (model for environmental contamination related factors), “STATA” removed the variable “marital status” from the model (because of collinearity). The model fitness with the previous form (three categories) is better than the current form for this model. Provided that marital status has p-value less than our set value (0.10), we maintained previous form of marital status (three categories) for table 3 (model of environmental contamination related factors).

Response to Editor Comments:

As you can see Reviewer 1 continues to raise concerns about your model. We have discussed this (and your response to Reviewer 2) with our editorial board. They feel that follow Reviewer 1’s advice in re-executing the statistical model, or follow Reviewer 2’s advice using propensity score matching (PSM). Our Editorial Board think that either approach would be adequate to address the overall concerns about the model.

We appreciate your concern and time in discussing on the issue. We accepted the suggestion and re-executed two models for the distal factors (one without proximate factors and the other with relevant and significant proximate factors both for under-five and infants). We have reflected this revision in the method (line number 247-252). We presented both models so that the readers will have the full picture. Except for slight changes in the values of odds ratios, there are no significant differences in the two models. However, marital status in the recent form (in two categories) tend to be significantly affecting both under-five and infant mortality in the model with proximate factors. Similarly, the odds of death among both under five children and infants
whose father were merchant was significantly higher than those whose fathers were farmers in the model with proximate factors. Accordingly we revised table 2 (previous odds ratio values are also slightly changed in this table as marital status in the current model was re-coded as two categories) and the corresponding narration in the abstract (line number 31-34), results (line number 285-311) and discussion (359-368).

Editorial Policies

Declarations

- Ethics approval and consent to participate
- Consent to publish
- Availability of data and materials
- Competing interests
- Funding
- Authors' Contributions
- Acknowledgements

All the issues related with the above declarations already are addressed in the manuscript (line number 432-465)