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 editor

1. Please frame your abstract and introduction so the link between human rights issues and this study is more clear

Thank you very much suggesting the point. Your suggestion is accepted and the abstract and the introduction have been revised accordingly (line number 20-22 and 44-48)

Editorial Policies

In accordance with BioMed Central editorial policies and formatting guidelines, all manuscript submissions to BMC International Health and Human Rights must contain a Declarations section which includes the mandatory sub-sections listed below.

Declarations

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

All these components of declarations are addressed in the manuscript (line number 423-456)

Response to reviewer 1

The goal of this study is to assess the link between deaths among children younger than five years old and socioeconomic and environmental factors in Southern Ethiopia. Using a case control study design, the authors found that factors such as low education and marital status of mothers, and occupation of fathers were related to under-five deaths in Southern Ethiopia. While this study is not innovative and the research methods are not new, it could provide information on child survival in the region. But as submitted, major improvements are needed. Comments are listed as follows in no order of importance.

Thank you very much for your comments and suggestions which are very crucial for the development and improvement of the manuscript.

In general, the report requires a complete editorial revision to make it grammatically sound and readable to the interested audience. Below are specific comments.

Thank you for the suggestion, the manuscript has gone through editorial revision.

In the background section, discussions on how/why children's death rates are still concentrated in sub-Saharan Africa in general, and how mortality trends in Ethiopia in particular compares within this context should be provided. Featuring, in brief, past and current death rates/trends in Ethiopia and any national/local policy efforts in improving child survival would help place this study in perspective. The authors could also touch on any within-country (Ethiopia) differences in child survival and its social and environmental factors, and where Gamo Gofa stands relative to other zones.

Thank you very much for the suggestion. We have revised the manuscript accordingly (line number 48-52)

While on page 6, the authors stated that changes in life style across nations (e.g. due to globalization) would require continuous investigation of both the established and emerging risk factors for child mortality, hence their study. Yet there was no discussion (if any) on possible emerging risk factors for child health in the study area. In line with their statement, I suggest the
authors should include household's main cooking fuel as a determinant in their analysis. While the authors focused on whether the household had kitchen or not, despite its important role (e.g. in exposure, ventilation, etc), I think cooking fuel type would be a better predictor of household air pollution exposure. Household air pollution from solid fuel is increasingly recognized as a risk factor that affects children's disease occurrence and mortality. Several observational studies, and at least one randomized trial, have shown its effects on pneumonia, the leading cause of child mortality. Thus, household use of solid fuel is included as an effective intervention, together with vaccines and antibiotics, for pneumonia prevention. There is also increasing evidence that it is a risk factor for birth weight which increases the risk of neonatal and child mortality. Further, solid fuel use was also included as an MDG target, together with unimproved water and sanitation, and is included in the SDGs as well. (Authors can see https://bmcpublichealth.biomedcentral.com/articles/10.1186/1471-2458-13-S3-S8; WHO/UNICEF http://www.who.int/maternal_child_adolescent/documents/global_action_plan_pneumonia_diarrhoea/en/). A recent study from Ghana also found an indication of the link between household cooking fuel and under-five mortality (http://journals.plos.org/plosmedicine/article?id=10.1371%2Fjournal.pmed.1002038#pmed.1002038.ref035).

We appreciate your concern and thank you for your detail description of the problem and suggestion. We strongly agree with you that, household's main cooking fuel is a determining factor of pneumonia and thereby child survival. But in our study household's main cooking fuel was not differentiating factor between cases and controls as 99.7% of the households were using wood, animal dung or charcoal as source of fuel for cooking. So we haven’t included it in the model. However, we have expanded this part further to make it more clear (line number 384-388).

I disagree with the authors' assertion that "this paper focused on the distal (socioeconomic) factors and environmental contamination related factors as the other proximate factors were the focus of another paper of the authors (this work is part of a PhD thesis of the principal author)". This seems to suggest that the authors purposely excluded other potential/relevant factors in view of future papers, even if the data was available. I am not satisfied with their response to the initial review from Editors on information regarding healthcare such as facility deliveries and complete vaccination schedule, on child's birth order, the mother's parity and the children's gender/sex, which can have significant impact on child deaths. In addition, and as noted in their own discussion, other important factors like ANC, insecticide-treated nets use, and nutritional supplementation should be considered. I support the initial reviewer's comment that "the lack of any discussion of these risk factors is concerning". Similarly, while the cases and controls were matched on age, and sex was controlled for in the multivariate analysis, the authors should consider matching the cases and controls also by sex, vaccination status, and whether the birth
was at home or health facility as already suggested. These may be important in such settings as socio-cultural and/or religions practices could affect a particular gender and/or health seeking behaviors/choices.

We appreciate your concern. As outlined in the conceptual framework (page number 7-8) based on the Henry Mosley and Lincoln Chen’s analytic framework for the study of determinants of childhood mortality, distal factors affect childhood mortality through proximate factors. According to this notion, if we controlled proximal factors in the model of distal factors, the odds ratio wouldn’t have showed the true effect of distal factors on mortality. Because, part of the effect of distal factor on mortality would be explained by the proximate factor which was included in the model. So they need to be analyzed separately (further explanation on how to analyze hierarchical factors could be observed in the following link: https://www.ncbi.nlm.nih.gov/pubmed/9126524

Controlling distal factors in the model of the proximate factors may also help to control the effect of some proximate factors which could be at least partly affected by distal factors. Besides, presenting all factors in a single manuscript may compromise the efforts of presenting the factors in detail. So we opted to present other factors in a separate paper.

With regard to matching, as the matching was done before data collection, matching the two groups (cases and controls) in many factors is resource intensive and sometimes not possible (it may be difficult to find a control which is similar to the case in many variables). So based on financial capacity we had we determined to match the two groups by the two very critical variables (age and residence/cluster). We tried to control the other factors during analysis at appropriate level.

I think the statement that "this work is part of a PhD thesis of the principal author" should be deleted from the text.

Thank you for the suggestion, we have omitted the phrase accordingly (line number 117)

In the background section, I suggest the authors make lines 85-93 (page 6) the last paragraph for this section. As written now, the 'conceptual framework' is there. This can be moved to somewhere earlier in the background section(if at all needed).

Thank you very much once again for the point. We have made the description of the conceptual framework a separate section and the paragraph you suggested has become the last paragraph for the background session (line number 105-118).
As currently written, no information on the specific social and environmental factors assessed, and how they are handled/classified are stated in the methods section. The reader is left to guess, and only finds these in the results/tables. These should be clearly indicated in the methods so the reader knows what to expect in the results section.

Thank you for the suggestion and we have expanded that part and indicated the socioeconomic and environmental contamination related factors which were assessed (line number 206-210). We further explained how wealth index and maternal power indexes were developed, which we believe need to be explained. We feel that, the other variables are self-explanatory and easily understandable from the tables in the result section, just to avoid redundancy.

Also, a brief statement on the household assets used to compute the wealth index should be provided. Same for maternal power. By the way, any explanation why wealth index was classified as poor, average, and rich? Are these relative to the mean/median of all the subjects/community? Quintiles are commonly used classifications.

Thank you very much for the comments. We have accepted the comments and expanded the operational definition of both wealth index and maternal power (line number 212-224).

The key description of the statistical analysis approach (line 219-226, last paragraph on page 11) is poorly written and lack important details. As is, it is not clear how the statistical analysis was conducted. If separate models were conducted for under-five vs. infant mortality, could the authors make that clear, and how the models were specified?

Thank you very much again for the suggestion. We have revised the paragraph to make it more clear (line number 246-253)

For example, in Table 2, maternal marital status compares single and other women to married women (same in Table 1), and yet the authors talked about separated/divorced/widowed women (similar for other variables like kitchen vs. separate kitchen). Clear definitions and consistency of their use throughout the text would improve readability of the manuscript.

We have accepted the comments and revised accordingly (table 1, 2 and 3)

Could the authors also define what they meant by infants? What share of the under-five deaths were infants? This section should be rewritten; in its current form, it is hard to fully understand and comment on the results as presented.
Thank you for the suggestion. In order to make the terms clear, we have added operational definition for under-five and infant mortality in the measurement and operational definitions session of the method (line number 225 & 226)

In line 245 (page 13) and in Table 1, it is unclear how education has been categorized: "Illiterate or read and write only"? You can't possibly have both "Illiterate" and "read and write only" as one category. For general audience, could education be identified as none, primary, secondary, higher?

Thank you for raising that point. We collected educational status data as 1) illiterate, 2) read and write only, 3) Grade 1 to 6, 4) Grade 7 to 8, 5) Grade 9 to 12 and 6) Above grade 12. However, the number of observations for “read and write only” and “Above grade 12” were very few. So we merged “read and write only” with “illiterate” and classified as “no formal education”, similarly, we merged “Above grade 12” with “Grade 9 to 12” and classified as “Grade 9 and above”. Besides, as there might be differences in classifying education status as primary, secondary, tertiary from place to place, we feel that using actual grading may help minimize the confusion which might arise.

In the first paragraph of the discussion section, authors should move to a new paragraph on strengths/limitation (currently not discussed) the statement "However, as information was collected retrospectively systematic errors such as recall and social desirability biases may affect some of the findings. Besides, in controlling confounding factors, especially in the second model (for environmental contamination related factors), we may not be exhaustive in addressing other confounding factors. However, it is possible to assume that the confounding effect of proximate variables could be at least partially controlled by controlling distal factors, which assumed to be operating through these proximate factors".

Thank you for the suggestion and we made it a separate paragraph (line number 329-334)

Response to Reviewer 2

I find the case-control method as explained in this paper to be too simplistic. And I don't think this is a right approach to select the control group. What if this case-control study was performed by using multivariate matched sampling methods that incorporate the propensity score?

There are interesting papers about there on case-control studies and using the propensity score matching approach. More importantly, prior to matching, a table showing the covariate
imbalance (difference in covariate means prior to matching) between the case and the control is deemed important.

Thank you very much for raising the point. In our study, the matching was already done before our main data collection using the two variables (age of the child and setting (cluster)). What we did was: First, we identified all potential controls for a given case (born within one month and living in the same locality) then, we randomly selected two controls for each case. We feel that, in order to apply multivariate matched sampling that incorporate the propensity score, it requires to have additional information such as additional child characteristics in order to determine the propensity score. But we didn’t have such information about the children prior to our main data collection. Besides, even though we had the information, matching the two groups using multiple variables may become resource intensive and sometimes infeasible (which is one of the limitations of multivariate matched sampling methods). So we matched the two groups by the two crucial variables (age and locality) and tried to control other variables at analysis stage using appropriate statistical methods.

Similarly, as information were collected after matching, a table showing the covariate imbalance (difference in covariate means prior to matching) between the case and the control is unavailable.

- Reference 10, line 456: Change to lower case.

Thank you very much for pointing that. We have corrected it accordingly (line number 503-505)

- Table 1: Add a column assessing the difference between alive and dead.

Thank you once again for the suggestion. We have added a column showing whether there is significant difference between the two groups in terms of a given variable, using p-value from chi-square (table 1, line number 595-596). Additionally, we have added maternal decision power in the background information instead of husband education (table 1 and 277-278), as it was included in the multi variable model.