Reviewer’s report

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

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Reviewer: Raphael Arku

Reviewer's report:

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.

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

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.

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).

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.

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

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).

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. 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.

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? 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. 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.

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?

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".

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

No

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
If not, please explain in your comments to the authors.

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