Reviewer’s report

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

Version: 2 Date: 09 May 2017

Reviewer: Raphael Arku

Reviewer's report:

Comments on the revised manuscript entitled "Socioeconomic and Environmental Determinants of Under-Five Mortality in Gamo Gofa Zone, Southern Ethiopia: A matched case control study."

This paper's focus was to assess environmental and socioeconomic determinants of under-five mortality in Southern Ethiopia. Some of the key issues raised in my previous review were addressed, but others remained in the current manuscript. With this revision and clarifications, I am now able to comment specifically on the statistical approach.

Methods:

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.

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.

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).

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.

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.

**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?**
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