Reviewer’s report

Title: Proximate determinants of infant mortality in Ethiopia, 2016 Ethiopian Demographic and Health Surveys: Results of Survival Analysis

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Reviewer: M. Delnord

Reviewer's report:

Thank you for inviting me to review this manuscript entitled "Proximate determinants of infant mortality in Ethiopia, 2016 Ethiopian Demographic and Health Survey: Application of Survival Analysis". The issue of under 5 childhood mortality is important in the field of perinatal and public health. Infant mortality is one of the key population health indicators that is associated with the performance of national health systems.

I have read the revised manuscript, and will provide my comments reading linearly through the text. There are general comments tied to the methods that require clarification as well the as the discussion which requires strengthening in terms of implications for policy and practice in Ethiopia. There are other minor edits to make throughout the manuscript that are related to English and wording.

I suggest to modify the title of the study to: "Results of a survival analysis" as opposed to "Application of survival analysis".

In the abstract/
there is some redundant information in the methods section "the study used data from the 2016 Ethiopian Demographic Health Survey"is mentioned twice. In the Results section of the abstract, and throughout the authors may want to refer to multiples only (which by definition include twins) as opposed to "multiple births or twins". Therefore, line 38 this should read the "results were higher for multiple births versus singletons".

In the conclusion of the abstract, the correct terminology is "infants low birth weight" (defined as birth before 2500g) as opposed to "infants below the normal weight".
Main manuscript/

In the background section, "the infant mortality rate" is not really a probability - it is defined by the number of infant deaths per 1000 live births.

Line 29-33, p3 references are missing.

In Methods, please define the "EA" abbreviation in the text. I would also specify the exact years when mentioning "the period of five years preceding the date of the survey" (line 56 p5)

Regarding the included variables, there needs to be a better explanation of the inclusion of the variable "size of child at birth" also in relation to the reference used for "average". Typically this variable should be collected and/or reported with respect to gestational age and/or birth weight in order for readers to understand how results from one population could be compared to another. What is considered average for a preterm baby vs. a baby born at 40 or 41 weeks will differ. There will also be sex differences, and the mother who just delivered will not necessarily be knowledgeable enough/or in a state to discriminate between GA:BW/ and or the increments of size...in particular for first time or younger mothers, or if there were complications during delivery. There needs to be more information in order for readers to understand how to interpret the results for this determinant.

I would also reconsider the variable "when child put to breast". I would encourage the authors to better explain what is the underlying hypothesis for using this variable with respect to infant mortality? As a side note this variable is most likely confounded by mode of delivery (Cesarean section deliveries often times having a later onset of breastfeeding vs "immediately"). This variable also does not speak to the "duration of breastfeeding" which is more clearly associated with overall infant health.

For the variable "smokes cigarette", it would be useful to have a bit more detail also as to how this variable was measured. (i.e. do authors have number of cigarettes smoked/day, did the mothers smoke during pregnancy?)
Finally, what is the underlying assumption also for looking at the link between "sex of household head" and infant mortality. For the analysis, I would check inclusions against known risk factors in the literature and have a better justification of other included variables that have not previously been explored in the literature.

In the results section, perhaps the information in paragraph 1 could be included in table format or added as columns to current Table 1. In the current Table 1, the denominator should appear (whether women or babies in the table headers).

Line 51 p 8, replace 'the lowest deaths of infants" with the "lowest infant mortality rate".

In the reporting of the Kaplan Meier estimates, the authors should provide more detail (i.e. exact time points) in the text and/or change the scale of the figure (fig 2). As it stands now, the reporting is not precise enough.

In the reporting of the Cox proportional Hazards, I would again add the N(denominator) in the table, and specify if there was any missing information on some of the variables? If so, this should be mentioned in the methods section (proportion of missing or analysis done on complete cases only?)

In the discussion, the results are well explained. There are some additions that could bring out more the importance of this study. For example, the discussion on the size of the child at birth is missing important information on Gestational age/birthweight/sex and the associations between these underlying determinants. Also how was the size of the child at birth collected in the study? I'm not sure I understand the inclusion of the "don't know" category...is this missing GA information? Or poor maternal recall?

I would suggest to add more in the limitations section based on the reviewers' comments, and in relation to sample size or other methodological limitations.

Other minor comments relate to rewording:
- I would specify which "other setting"s the authors refer to in line 22 p13

- Lines31-32 are not very clear. I am not sure I understand what the authors mean by "all infant deaths occurred at the end of the observation period"? The observation period from what I understood was N-5 years, so it would make sense that all infant deaths would occur before then... As suggested earlier, perhaps adding the dates of the study would help clarify.

- Line 37 p 13, not just "due to poor monitoring and short follow up time " but poor antenatal care in general or lack of access to care also.

- Reword "showed that the better birth space of infants had a better survival". i.e. infants born after longer interpregnancy intervals had better odds of survival. When referencing the literature on interpregnancy interval, please add the number of months used in the other studies and/or identified as the optimal interpregnancy interval (also 24 months?..this may vary by studied birth outcome).

- Reword line 25 p 15 "being male sex of the infant had a statistically significant impact on infant mortality" with for i.e. "male sex was significantly associated with infant mortality".

Overall, this is an interesting study with valuable population based results for Ethiopia. However before publication, variables included in the study need to better detailed/justified, and the reporting of the survival analysis in the figures and tables also needs to be more precise with some further language edits. I would encourage the authors to strengthen the discussion with broader public health arguments and recommendations that will help bring the study results in context. This would require more emphasis on what are the implications of this study specifically for Ethiopia either with respect to antenatal/perinatal care policies in urban vs rural, or for mother with higher risk pregnancies. Perhaps authors may want to consider infant mortality/perinatal health trends in Ethiopia also and previous survey results in other years. Moreover, although some variables were not statistically significant (i.e level of education or maternal age), I would also discuss these inconclusive findings as these determinants are those typically associated with poorer birth outcomes in the literature (this could be relevant for the limitation section also).

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