Author’s response to reviews

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

Authors:

Masrie Abate (masriegetnet16.biostat@gmail.com)
Dessie Angaw (dessieabebaw96@gmail.com)
Tamrat Shaweno (babiynos@gmail.com)

Version: 1 Date: 12 Apr 2019

Author’s response to reviews:

We would like to thank the reviewers and editor for sharing their view and novel scholarly experiences. The comments are very imperative which we strongly believe in improving the manuscript. The point-by-point responses for each of the comments, questions, and the revised manuscript are provided in the attached documents. We all authors are ready to reply again for those points not raised.

1. Ethiopia is appreciably performed well to reduce child deaths over time. The pace of decline in infant mortality is faster than child mortality. It is not clear enough from this research paper - what are the gaps in existing knowledge regarding infant mortality in Ethiopia? why this research is important? why only these proximate determinants are important? This research is only found few statistically significant factors (such as multiple birth or twin, birth interval, size of the child at birth and sex of the child), which raises a question - why only a few? Among these factors, two are non-modifiable (multiple births or twin and sex of the child). Another factor "small size of infants" is based on maternal perception.
1.1. Ethiopia is appreciably performed well to reduce child deaths over time. The pace of decline in infant mortality is faster than child mortality. It is not clear enough from this research paper - what are the gaps in existing knowledge regarding infant mortality in Ethiopia? why this research is important?

Authors response: Dear reviewer, thank you for your concern, as you said Ethiopia is on a good track in reducing infant mortality. Based on the 2016 EDHS report, infant mortality rate in Ethiopia is still high as compared to some African countries (like Libya, Egypt, Morocco, Rwanda…). So, we author believe that identifying constraints/factors is very important to give a clue for the government of Ethiopia to alleviate the problem at lowest level of infant mortality rate comparable to countries having lowest mortality rate like Libya and others.

1.2. why only these proximate determinants are important? This research is only found few statistically significant factors (such as multiple birth or twin, birth interval, size of the child at birth and sex of the child), which raises a question - why only a few? Authors response: Dear reviewer, the points you raised are very crucial and valuable. We addressed all determinant factors (if you recommend as to replace the title by determinants of infant mortality in Ethiopia, 2016 Ethiopian Demographic and Health Surveys: Application of Survival Analysis) we will change it.

During univariate Cox regression analysis, those variables having p-value less than 0.25 send to multivariable Cox regression analysis. Based on our statistical analysis output, we get the above you mentioned statistically significant variables. Even, we tried to analyze all the variables after we received your comments. We didn’t get any change.

1.3. Among these factors, two are non-modifiable (multiple births or twin and sex of the child). Another factor "small size of infants" is based on maternal perception.

Authors response: Dear reviewer, the issue is very important point. Before, we send to the journal it was our big issue as you raised. After detail discussion, we all authors agreed that these non-modifiable significant variables can give direction to give a special attention on them to reduce infant mortality rate. Similarly, “small size of infants” is based on maternal condition. This finding can give direction to work on mother’s status (may be health status, nutritional conditions…) to increase the size of infants.
2. In the method, a short description of sampling and a description/a flowchart of selecting analytic sample are needed. Selection of analytic sample should contain every exclusion criterion.

Authors response: Dear reviewer, we all authors directly take your valuable comments and the points/issues are incorporated in the main document.

This is some additional points incorporated in method part

Ethiopia 2016 DHS include 645 EAs, 202 in urban and 443 in rural areas. EA area was selected by using probability proportional to enumeration area size based on the 2007 population housing census. In the second stage of selection, a fixed number of 28 households per cluster were selected with an equal probability systematic selection from the newly created household listing (Method part, page 5, line 12-15,yellow color). The flow chart is on page 6.

Fig 1. flow chart of sampling for EDHS 2016, Ethiopia

3. Definition of outcome variable is not clear enough, and paper needs a clear definition based on time-to-event data concept. In addition, measurement units and categorization of variables are missing. They are needed in method section for better understanding.

Authors response: Dear reviewer this is a fruitful and improvable comments for our paper. We incorporate the points in the main manuscript as below (Method part page 6, paragraph 1, line 1-11).

The outcome (infant survival time) measured in months between birth and the 1st birth day within the preceding five years from the survey year. In this finding, those infants who died between birth and the 1st birth day were events and those who were still alive and did not reach their 1st birthday were censored. The predictors included in this study were: educational level of mother’s (No education, primary, secondary and above), age of mother at first birth (&lt;20,20-30,&gt;30), place of residence (urban, rural), Region (Tigray, Afar, Amhara, Oromia, Somalia, Benishangul, SNNPR, Gambela, Harari, Diredawa, Addis Ababa), Wealth index (poor, middle, rich), place of delivery (home, health institutions), birth order (&lt;3, 3-4,&gt;4), multiple births (single, multiple), Size of child at birth (small, average, large, don’t know), number of antenatal care visit (&lt;4, &ge;4), smokes cigarettes (yes, no), sex of child (male, female), Preceding birth interval (&lt;24, 24-47,&gt;47), Sex of household head (male, female), When child put to breast (immediately, not immediately).
4. In this paper, Cox PH model is fitted for examining the associated factors with mortality, but the EDHS survey is a multistage cluster survey. Due to sampling design, data is clustered in nature and need to adjust the cluster level variation using Frailty model or Cox PH mode with random effect. I recommend reanalyzing data by using the frailty model.

Authors response: Dear reviewer thank you for your valuable comment. As you stated, the EDHS data survey is a two multistage cluster survey. With no doubt it is good that showing cluster level variation using Frailty model. But, our primary/main/objective was not to show the cluster variation other than identifying the determinant factors of infant mortality. Though we admit your valuable comment that can strength the quality of our finding, we agreed that to continue on showing the primary objective using Cox PH model.

5. Overall results are not convincing enough and make many confusions. Firstly, why only very few factors are significant statistically, need a broad description on this issue in the discussion? What about possible collinearity among predictors?

Authors response: Dear reviewer we feel your concern. We tried to justify the issue of why few variables are statistically significant on Authors response 1.2. Regarding to collinearity we have checked all variables having p-value less than 0.25 in univariate analysis using variance inflation factor (VIF). we didn’t find that VIF greater than three in intercorrelation test. So, we found that Collinearity was not our issue.

6. In this paper, "Number of antenatal care visit" is categorized as "&lt;3" and "&gt;= 3". But why "&lt;3" in place of "&lt;4"?

Authors response: Dear reviewer, thank you for your detail observation. After we received your comment, antenatal care visit (ANC) is recoded in to &lt;4 and ≥ 4 (page 9-10, table 1 yellow color and page 11-12, table 2).

7. In "Size of the child at birth" variable, why "Don't Know" is a category? It does not make any sense

Authors response: thank you for your concern. Of course, it looks doesn’t give sense. But We all authors believe that using this category in the given variable is essential for two reasons. The first one is not to miss the eligible individuals who respond I don’t the size of the child at birth. Secondly, we author believed that mothers who don’t know the size of the child at birth may not give especial care to reduce mortality of the neonates/infants.
8. "Multiple birth" variable is a significant factor, but it should interpret carefully due to very few numbers of observations in multiple birth category.

Authors response: thanks for your concern. We tried to interpret carefully while we rewrite the discussion part.

9. Table 1 should contain the only percentage of death by the categories of risk factors, no other columns.

Authors response: thank you very much for your valuable comments. We addressed your issue accordingly (possible to check at result part, table 1, page 9 and 10)

10. Table 2 should contain a single column for 95% CI

Authors response: thank you very much for your valuable comments. We addressed your issue accordingly (possible to check at result part, table 2, page 11 and 12).

11. Figures are made for only significant factors, but they are presented before model results which creates confusion. Need a clear description or rationale - why only a few figures are presented? Moreover, the figures look very ordinary. Please revise the figures by adjusting the y-axis limit and size

Authors response: Dear reviewer with great thanks we considered the comment. We intentionally made figures presentation for only significant variables to show good visualizing for these very important variables on infant mortality. But, internalizing your comment we all authors decide to display only the overall estimates of the Kaplan-Meier survivor function (we incorporate only one figure to the main manuscript).

Regarding to figure adjustment we tried to modified. We hope it is visible as compared to the previous figure (see fig 2).

12. Discussion section needs to rewrite and to give more focus on the intervention design and policy implications.

Authors response: Dear reviewer thank you for your valuable direction for us to rewrite and to focus on the intervention design and policy implications. Based on your comment we rewrite the discussion part accordingly (yellow color indicated that additional points are incorporated in the discussion part, see discussion part, page 13 and page 15)
13. Limitations of this study are also missing, please write a possible limitation of the study.

Authors response: limitation was addressed on the last paragraph of the discussion part (see page 15, yellow colored, last paragraph)

Thank you very much indeed!!!

Yours sincerely

Dessie Abebaw (BSc in OHS, BSc in PHO and MPH in Epidemiology and Biostatistics)