

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Trends and determinants of home delivery in Ethiopia: Further multivariate decomposition analysis of 2005 to 2016 Ethiopian Demographic Health Surveys
AUTHORS	Tiruneh, Sofonyas; Molla, Ayenew; Yigizaw, Seblewongel; Sisay, Malede; Tessema, Zemenu

VERSION 1 – REVIEW

REVIEWER	Karlijn Massar Maastricht University, The Netherlands
REVIEW RETURNED	16-Dec-2019

GENERAL COMMENTS	<p>The authors report on 3 waves of a national demographic survey in Ethiopia to investigate whether home deliveries have increased or decreased throughout the years (2005-2016), and which variables might be predictive of this change. This is an interesting and important research topic, and the authors show that several variables - some of which are, importantly, changeable by health promotion or health education interventions - are indeed predictive of the observed decline in home deliveries in these years. Although I am unsure whether this statistical analysis is the correct one and I was daunted by the formulas in the text (they are better off in supplemental materials, in my opinion), the results are pretty clear-cut and the authors interpret them accordingly. However, there are many issues with the quality of the written English, which makes it at times hard to understand the authors' intention/point they are trying to convey to the readers. Moreover, the paper does not list any limitations of this research, nor offers starting points or suggestions for possible interventions targeting the variables they have identified as most important for behavior change.</p> <p>Moreover, the authors mainly repeat their findings in the discussion section, without trying to interpret what the underlying causes of such behaviors might be. For example, they report that women with more children ('higher birth orders') are more likely to deliver at a health facility. This to me suggests that experience - or past behavior - is an important factor which also necessitates that first-time pregnant women should be targeted more intensively.</p>
-------------------------	---

REVIEWER	Tieba Millogo African Institute of Public Health, Burkina Faso
REVIEW RETURNED	16-Feb-2020

GENERAL COMMENTS	Trends and determinants of home delivery in Ethiopia: further multivariate decomposition analysis 2005 to 2016 Ethiopian Demographic and Health Surveys
-------------------------	---

	<p>The authors reported in this paper on the trends of home delivery and contributing factors in Ethiopia over a decade. If the drivers to home delivery are very well-known, the contribution of each factor in a context where multiple interventions to tackle the issue are underway is informative and may help policymakers and implementers to tailor their strategies. The topic is relevant, and we congratulate the authors for also choosing a statistical method that suits overall to their investigative objectives. However, the English language of the paper is poor and need to be much improved. Aside from the statistical section, the methods section lacks required strengths because of important missingness that hamper the valid assessment of the overall study methods. I recommend the authors revise the methods section to fully and meaningful describe their methods. This is my major comment and issue with this paper. Below are the details of my review</p> <p>Title Contrary to what the authors reported in the Strobe checklist, the study design is missing in the title;</p> <p>Abstract Line 33-34: check the sentence for meaning, some rephrasing might be necessary</p> <p>Strengths & limitations</p> <p>Line 9: the power should be calculated and reported. What is the power (post-hoc analysis) of this study? Line 13: replace planners by policy makers Line 15: check the sentence, there is probably a word that was skipped and overall similar sentences are found across the paper</p> <p>Study design, area and period</p> <p>The study design is missing in this section. The authors need to explicitly state the design of their study. To me this is a repeated cross-sectional study.</p> <p>Source and study populations Line 40-50: same idea being repeated a number of times and important need of rephrasing. The study population needs to be further described with all sound inclusion and non-inclusion criteria. The fact that this is a secondary data analysis does not make it any the less necessary for the authors to fully describe the methods that were used in the primary study (or referring readers to where such information is available) and in their own secondary analysis.</p> <p>The data collection was described for the only 2016 survey, there is a need to describe the data collection process in general within the DHS framework, emphasizing the fact that procedures and outcome measurement particularly remains the same over the time, allowing the comparison being made. The study variables are not well described. For the outcome variable, were multiple measurements per woman taken into account?? Or the outcome variable was solely based on the preceding birth event? What if the woman had multiple birth events in the time-window ?? was the history of previous place of delivery taken into account? the predictor variables need to be stated.</p> <p>Statistical analyses seem correct and correctly interpreted to me. However, there is no evidence of significant change in the trends in the results presented. Authors may want to use a chi2 test for trends</p>
--	--

	<p>to support the assertion in results line 35-36.</p> <p>Results</p> <p>Table 1: in the title it is stated “women that had a live birth in the 5 years preceding.....” this is the first time we are being told that giving birth alone was not sufficient, but only live births events were considered. Please clarify as this has an important implication because home delivery and health facility delivery are expected to differ importantly as to childbirth outcomes...</p> <p>Table 1: how were the percentages computed? Based on predictors' groups I guest..so the row total at the bottom of the table is not correct and useless, please delete.</p>
--	---

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

Reviewer Name: Karlijn Massar

Although I am unsure whether this statistical analysis is the correct one and I was daunted by the formulas in the text (they are better off in supplemental materials, in my opinion)

Noted! Thank you for the comment. We can add as supplementary material but not as easy as a short cut. But the model was fitted through a STATA “mvdcmp” package as the function written in the main document. The code in STATA as follows

```
“mvdcmp year: logit place_delivery i.age_cat i.religion_cat i.edu_stat_mom i.edu_stat_has
i.occ_stat_mom i.occ_stat_has i.anc_visit i.b_ord i.dis_HI i.wealth_index i.parity i.residence [pw=wt]”
```

For example the overall decomposition SATAT output of Endowment “E’ and coefficient ”C” was

However, there are many issues with the quality of written English, which makes it at times hard to understand the authors' intention/point they are trying to convey to the readers.

Thank you! We have noted the issue. To the best of our knowledge, we had to try to update the spelling, grammar and written English problem. As well we are going to update.

Moreover, the paper does not list any limitations of this research, nor offers starting points or suggestions for possible interventions targeting the variables they have identified as most important for behavior change.

Noted! We had to put the limitation of cross-sectional study as limitation of study.

Moreover, the authors mainly repeat their findings in the discussion section, without trying to interpret what the underlying causes of such behaviors might be. For example, they report that women with more children ('higher birth orders') are more likely to deliver at a health facility. This to me suggests that experience - or past behaviour - is an important factor which also necessitates that first-time pregnant women should be targeted more intensively.

Thank you for this invaluable comment! We act accordingly.

Reviewer: 2

Reviewer Name: Tieba Millogo

The English language of the paper is poor and need to be much improved. Aside from the statistical section, the methods section lacks required strengths because of important missingness that hamper the valid assessment of the overall study methods. I recommend the authors revise the methods section to fully and meaningfully describe their methods. This is my major comment and issue with this paper.

We thank you for the suggestions at large. For the comments, we act accordingly.

Contrary to what the authors reported in the Strobe checklist, the study design is missing in the title;

Thank you! We had included the study design in the abstract part.

Abstract

Line 33-34: check the sentence for meaning, some rephrasing might be necessary

Noted! Act accordingly.

Strengths & limitations

Line 9: the power should be calculated and reported. What is the power (post-hoc analysis) of this study?

Noted! Simply as the sample size increase the power of the effect estimates also increase as we think so.

Line 13: replace planners by policymakers

Noted! Corrected accordingly.

Line 15: check the sentence, there is probably a word that was skipped and overall similar sentences are found across the paper

Noted!

Study design, area and period

The study design is missing in this section. The authors need to explicitly state the design of their study.

Thank you! Corrected accordingly!

Source and study populations

Line 40-50: same idea being repeated a number of times and important need of rephrasing. The study population needs to be further described with all sound inclusion and non-inclusion criteria. The fact that this is a secondary data analysis does not make it any the less necessary for the authors to fully describe the methods that were used in the primary study (or referring readers to where such information is available) and in their own secondary analysis.

Noted and corrected accordingly.

The data collection was described for the only 2016 survey, there is a need to describe the data collection process in general within the DHS framework, emphasizing the fact that procedures and

outcome measurement particularly remains the same over the time, allowing the comparison being made.

Thank you! we had corrected accordingly!

The study variables are not well described. For the outcome variable, were multiple measurements per woman taken into account?? Or the outcome variable was solely based on the preceding birth event? What if the woman had multiple birth events in the time-window ??

Noted! For the outcome variable ascertainment if the women had multiple birth events in the time-window (preceding five years during the survey) it was only taken the recent birth history preceding the survey window. No multiple measurements.

was the history of previous place of delivery taken into account? the predictor variables need to be stated.

Noted! It was noted account for the previous birth history.

Statistical analyses seem correct and correctly interpreted to me. However, there is no evidence of significant change in the trends in the results presented. Authors may want to use a chi2 test for trends to support the assertion in results line 35-36.

Noted!

Results

Table 1: in the title it is stated “women that had a live birth in the 5 years preceding.....” this is the first time we are being told that giving birth alone was not sufficient, but only live births events were considered. Please clarify as this has an important implication because home delivery and health facility delivery are expected to differ importantly as to childbirth outcomes...

Corrected accordingly!

Table 1: how were the percentages computed? Based on predictors' groups I guest..so the row total at the bottom of the table is not correct and useless, please delete.

Noted! The percentages each predictor category was calculated using the frequency for each predictor variable divided by the total sample size for each respective survey years. To put the frequency of each survey year predictors the table format is difficult. But we can get the respective frequency for each predictor though multiplying the percentage to total sample size.

VERSION 2 – REVIEW

REVIEWER	Tieba Millogo African Institute of Public Health Burkina Faso
REVIEW RETURNED	13-Apr-2020

GENERAL COMMENTS	I wish to really congratulate the authors for submitting a revised version of their paper which I found much better. However, I still think that the English language needs to be deeply revised. I would suggest the use of a professional editing service. Furthermore, I do not agree with the authors' comments on the statistical power of their study. Combining multiple surveys will increase the sample size and then the statistical power under the same hypotheses. We don't
-------------------------	--

	know what these hypotheses were, what was the calculated statistical power?. It is harder in such a context to accept the statistical power among the strengths of the study. The authors have maintained the "significant decrease in the rate of home delivery" in their paper without any statistical evidence for that. they may want to add confidence intervals to the changes in table 2 with a row for total change, that should suffice to show the statistical significance.
--	--

VERSION 2 – AUTHOR RESPONSE

Reviewer: 2

I wish to really congratulate the authors for submitting a revised version of their paper which I found much better. However, I still think that the English language needs to be deeply revised. I would suggest the use of a professional editing service.

Noted, thank you! To the best of our knowledge, We had tried to deeply revise the issue of English writing errors. On top of this, we had invited for professional in the English language in our university and incorporate the comments from the professional.

Furthermore, I do not agree with the authors' comments on the statistical power of their study.

Combining multiple surveys will increase the sample size and then the statistical power under the same hypotheses. We don't know what these hypotheses were, what was the calculated statistical power?. It is harder in such a context to accept the statistical power among the strengths of the study.

Thank you! Yes, we had modified the phrase “ the power of our findings”. We want to say as the sample size is large and a nationally representative that wold increases the generalizability issue. We hope that is the way what we write in the wrong way. We think now this in touch with your question.

The authors have maintained the "significant decrease in the rate of home delivery" in their paper without any statistical evidence for that. they may want to add confidence intervals to the changes in table 2 with a row for total change, that should suffice to show the statistical significance.

Noted thank you! We had incorporated the prevalence of home delivery with a corresponding confidence interval. As well, we had incorporated the confidence interval in simple decomposition change in table 2. Since the confidence interval didn't lie through the survey years, which indicates a significant change.

VERSION 3 – REVIEW

REVIEWER	Tieba Millogo African Institute of Public Health, Burkina Faso
REVIEW RETURNED	06-Jun-2020

GENERAL COMMENTS	I congratulate the authors for the paper and for improving the current version. I would have recommended publication if not because of the English langage. I had already raise this issue in my previous comment and still I felt it was not appropriately taken into account. The paper really needs a thorough proof-reading as some sentences are just difficult to understand because of skipped words I assume...However, I think this last revision should be done at the satisfaction of the reviewers. No need for me to revised another version of the paper.
-------------------------	---

VERSION 3 – AUTHOR RESPONSE

We had sent our third revision of our research manuscript entitled with “Trends and determinants of home delivery in Ethiopia: Further multivariate decomposition analysis of 2005 to 2016 Ethiopian Demographic Health Surveys” in your journal BMJ Open as a research article.

Below is a list of issues addressed in the revised manuscript:

- 1) The manuscript was proofread by senior professional copyeditor from Australia to improve the quality of the manuscript.
- 2) Besides, Our senior staff's editors were also made copyediting this manuscript.
- 3) Collectively, We think that at this stand the manuscript will meet the minimum journal requirement.