Author’s response to reviews

Title: Factors affecting Tobacco Smoking in Ethiopia: Evidence from the Demographic and Health Surveys

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Author’s response to reviews:

Reviewer reports:

Hadii M Mamudu, PhD, MPA (Reviewer 1): Title

Factors Affecting Tobacco Smoking in Ethiopia: Evidence from the Demographic and Health Survey

Summary

The paper is about the key determinants of tobacco smoking in Ethiopia. This paper uses the Demographic and Health Surveys (DHS; 2011 and 2016) collected by the Ethiopia Central Statistical Agency to address smoking patterns related to khat use, geographical location, and other social covariates that could affect smoking. The overall sample in this study consists of 56,644 adults between the ages of 15-49. The main finding from the pooled analysis is that after control demographic, socioeconomic, and spatial factors, chewing Khat was associated with tobacco smoking.

Overall Comment

This study is important to highlight all the different factors on smoking in Ethiopia. The results of this study are extremely important to the government officials in Ethiopia and regulating or preventing smoking. Each individual, household, or community level factor could be affected differently by different policy implementation in the given area. After looking at data from the 2011 and 2016 DHS, the areas discerned included khat chewing, gender, age, marital status, education level, occupation, household wealth, members of household smoking, religion, place
of residence, and distance from sea level. While there are specific corrections to be made in this study, the overall paper is effective in not only outlining the most prominent determinants of tobacco smoking in each region but also proposes a fix to effectively combat smoking as a result of the research recorded.

1. The main issue with the paper is data management, which spurs into the analysis and results. The authors merged the 2011 and 2016 DHS without providing any information whether these two cross-sectional data were statistical different or similar. In this regard, the authors should include results of any diagnostics such as t-test, multicollinearity, auto-correlation they conducted before merging the two data. Regardless, I will suggest that the authors should conduct separate analysis for 2011, 2016, and pooled (2011 + 2016). With that, the reader can delineate some trends and differences in the results, if any.

RESPONSE: As per the reviewer’s suggestion, we ran the baseline model separately for 2011 and 2016 survey periods. We found no qualitative differences as most of the coefficients had the same sign and statistical significance in both survey periods. We also ran a test for the statistical difference of coefficients across the two surveys using a seemingly unrelated estimation method. The test results suggest that coefficients of the two regression equations are not statistically different, with the exception of age and occupation category.

Therefore, we decided to report the pooled cross-section analysis which includes a time dummy that captures time effects in tobacco smoking between the two survey years. As expected with the larger sample size, the pooled data yields higher precision of statistical estimates by reducing standard errors.

We would like to note that the results from the separate analysis for 2011 and 2016 periods are now provided in an additional file for interested readers. We have also included this as an endnote (# 11) on page 32 in the revised manuscript.

2. Given the huge gender gap in tobacco smoking and Khat use among men and women, I would urge the authors to conduct stratified analysis by gender. If the sample sizes for women are too small such that it poses analytical challenges, then the authors should consider a study about Ethiopian men, not including women. As paper stands, it subsumes the effects of the independent variables on the behavior of women, which is erroneous and over-generalizes the results.

RESPONSE: The reviewer is correct to point out that the data does not contain enough female smokers to conduct stratified analysis by gender and provide meaningful gender-based results. As per the reviewer’s suggestion, we ran a separate regression for male and female. The results show that the sign of the coefficients for most of the variables remains the same but, as expected,
the magnitude of the coefficients varies slightly. Given that the qualitative results remain the same, we felt that keeping female information within the sample is beneficial to increase inference.

In addition, including the gender/sex dummy in the regression equation allowed us to conduct a more robust analysis as we were able to control for any confounding factors in the regression equation. As the results suggest, females are less likely to smoke than males, something that wouldn’t become apparent without including the gender dummy in the specification. Therefore, we kept the original sample and specification with the gender dummy.

3. While the paper mentioned several policy changes that could be implemented to prevent and or stop the spread of smoking, it also discusses the government of Ethiopia passing the anti-tobacco bill in between the time of the two surveys examined in this study, 2011 and 2016. This bill was not taken into account in the research recorded, and it could be a significant mediator in tobacco use between the two surveys. This reinforces the previous suggestion that the authors should conduct separate analysis.

RESPONSE: We would like to note a couple of points regarding the reviewer’s comments:

First, the objective of this study is to explore factors affecting tobacco smoking in Ethiopia. Although it would be beneficial to evaluate the effectiveness of tobacco control policies (anti-tobacco bill) in Ethiopia, this is not part of our research objective for the following reason: The tobacco bill was introduced in 2015 and the data collection work for 2016 survey was done from January to June 2016. The current DHS surveys do not include any variables that can be used to evaluate the effectiveness of tobacco law. To clarify our study’s objective and avoid any confusions, we have removed sentences on the effectiveness of policy in the revised manuscript (Please see the last paragraph on page 8 and the first paragraph on page 9).

Second, as mentioned in the response to comment 1 above, we conducted a pooled regression analysis by introducing a time dummy which allowed us to capture time effects in tobacco smoking between the two survey years. These differences cannot be captured if surveys are analyzed independently.

4. The interpretation of the results as "predictors" connotes some form of causation, which is not true for cross-sectional data. Indeed, the main limitation of DHS data, even though they are large and nationally representative, is that the data cannot be used to establish causation. As such, the authors should revise the enter Results and Discussion sections to couch their results as determining "association" between the dependent and independent variables.
RESPONSE: As per the reviewer’s suggestion, we have revised the description of the results and discussion section.

5. The statistical notations in the Methods section do not add much information to the analysis of the data. Instead, these notations make the paper a bit harder to read. Thus, I will suggest that the authors should get rid of them.

RESPONSE: We have removed the statistical notations from the manuscript, but have provided this as ‘Additional File -1’ for interested readers.

Specific Comments

Abstract

1. Including the policy already in place for tobacco smoking in Ethiopia would have been helpful in understanding the study.

RESPONSE: The revised abstract now includes a statement on the recent tobacco policy in Ethiopia.

The background section of the Abstract now reads:” The government of Ethiopia passed an anti-tobacco bill in 2015 that includes measures governing tobacco consumption, advertising, packaging, and labelling. However, to effectively address the challenge of tobacco control, the government should consider a number of aspects of tobacco production and consumption, such as local production in rural areas, as well as the complementary nature of tobacco and khat use.”

Background

1. While this study takes into consideration the different genders and percentages of tobacco smoking in each, the introduction describes an increase khat consumption on the basis on young men stand around at khat kiosks, which is concerning.

RESPONSE: We regret that our anecdote did not sufficiently convey our message that khat consumption has been rising in Ethiopia. We have corrected this in the text by adding citations that provide stronger support for our claim. That is, we have included the following on page 5 (lines 39-51) of the revised manuscript: “Furthermore it appears that there is an increase in local consumption. Before the beginning of the 21st century, only a limited amount of khat was chewed for socialization and at religious gatherings, while currently, the number of khat chewers has increased among all socio-demographic characters [12]. The Economist (2017) also reports
that khat kiosks are spotted around all main towns along with young men chewing on street corners or in university libraries [9].

2. The introduction states that by using the 2011 and 2016 survey data, the study is able to compare the effectiveness of tobacco policies in 2015, but this is never looked at and assessed in the study.

RESPONSE: We appreciate the reviewer’s point. As mentioned in our previous response, it is not the objective of this study to evaluate the effectiveness of tobacco policy in Ethiopia. To avoid any confusion, we have removed this sentence in the revised manuscript.

Methods

1. The methods include a binary dependent variable that include any individuals above the age 15, but when measuring age factors in the study, the highest age included was only 49 years. Please be as specific as possible.

RESPONSE: This has been corrected in the revised manuscript. Page 12 (lines 4-7) of the study variable section of the manuscript now reads “… if the respondent age 15-49 did not smoke any tobacco products…”

Results

1. According to the results in the summary statistics, wide variations in smoking are expected to be due to wide variation in khat use. Gambela has the highest prevalence of smoking in Ethiopia; however, it accompanies one of the lowest percentages of khat use proving this statement false.

RESPONSE: We appreciate the reviewer for bringing this to our attention. We have deleted the sentence “Thus, wide regional variations in tobacco smoking are also expected due to variations in khat chewing practices.” in our revised manuscript to avoid any confusion.

We would like to note, however, that in the discussion section, we have highlighted that the high prevalence of smoking in the Gambela region is driven by factors other than khat chewing practices. This is because the social context is very different in the Gambela region than in other regions of Ethiopia (see page 23)

2. For Figures 1 and 2, what do the legends stand for? I will suggest that you should add a title to the legend and have a caption for each map.
RESPONSE: As per the reviewer’s suggestion, Figures 1 and 2 have been revised.

Discussion

1. As in the introduction, a section in the results references men alone. The paper states that, "the likelihood of smoking increased by 11 times if a household member smokes inside the house." This says a household member but then is only explained by men rather than men or women in the household.

RESPONSE: We have revised the sentence to reflect the reviewer’s comment.

Page 21, lines 14-32 now read:

“It is not surprising that we find that the likelihood of smoking increased by 11 times if a household member smokes inside the house. In many rural parts of Ethiopia, while men smoke in both public places and in their homes, women mainly smoke in their homes which often consist of small and poorly ventilated rooms [46]. The combined DHS data (2011 and 2016 survey) suggests that 52% of female respondents reported to smoke inside the house. One expects that the imitation and emulation of behaviors of the family members to take place. Being exposed to smoking on a regular basis would entice other family members to smoke themselves.”

2. The discussion talks about the lower smoking rate in 2016 compared to the 2011 surveys. The discussion explains that this is because the survey does not track smoking behavior and randomly selects different households. However, the discussion does not discuss the policies put into place in Ethiopia in 2015 that may mediate the results observed in 2016 vis-à-vis those in 2011.

RESPONSE: We have revised our discussion section by including a discussion on the recent tobacco control bill in Ethiopia.

On Page 22 (lines 51-59) and Page 23 (lines 4-14) of the revised manuscript:

“Fourth, although the Ethiopian parliament ratified the WHO framework on tobacco control in 2014 and eventually passed the anti-tobacco bill in 2015 prohibiting smoking in public places, it could be overreaching to attribute the results on time effects to these regulations. More specifically, while the WHO framework prohibits tobacco advertising and promotion, sales of single cigarettes and sales to minors, and controlling of illicit trade of tobacco products, in Ethiopia all these banned practices continue to be widespread not only due to lack of awareness among users about their banning, but also the lack of any enforcement measures [46]. Similarly, smoking in public places was widely observed in the capital city.”
Conclusion

1. One conclusion made by the study is that by controlling khat consumption the government could control tobacco consumption. The survey asked if khat had been chewed 30 days prior to the survey and if the participant had smoked tobacco, but it did not ask which activity came first. Khat usage might not be the gateway but the other way around.

RESPONSE: We appreciate the reviewer’s point that the association between khat chewing and tobacco smoking could work the other way around. We would like to draw the attention on the following two points to address the reviewer’s comment.

Firstly, the evidence provided from the literature suggests that exposure to khat chewing is associated with tobacco smoking. Please see Section 1.1, Page 6, lines 21-53 of the revised manuscript for further details.

Secondly, our methodological approach addresses any bias in our estimation of khat due to the reverse association between khat chewing and tobacco smoking. As outlined on Page 11, lines 9-36 of the revised manuscript, the use of two-stage residual inclusion estimation method produces consistent estimates of the parameters with endogenous regressors. More specifically by including residuals of khat as regressors from the first stage of the equation, we address the endogeneity of khat regressor. In other words, the used estimation methods account for the possibility of reverse association of khat chewing and tobacco smoking (as per the reviewer’s point).

Minor Corrections

1. Spell out numbers 0-9

RESPONSE: Numbers have been spelled out in the revised manuscript.

2. Using %, instead of "per cent" will be better.

RESPONSE: This has been changed in the revised manuscript.

3. Always bring comma (,) before "respectively".

RESPONSE: This has been corrected in the revised manuscript.
4. Include information on town/region/country for the STATA version 14.

RESPONSE: The region for Stata has been added to Page 11, line 44. “STATA version 14 (StataCorp, College Station, TX) was used for all data analysis.”

5. P. 28 ln. 11- add brackets around the number 17 in "…practice17, it…”

RESPONSE: Number 17, which is now number 18 in the revised manuscript, refers to a footnote and should have been superscripted. This has been corrected in the revised manuscript.

6. The title for Table 1, "Summary Statistics" is inadequate as a table should be self-contained.

RESPONSE: The title for Table 1 now reads “Summary Statistics for Dependent and Independent Variables”

7. It's not clear the significance of reporting the “Minimum" and "Maximum"; please remove them from the Table 1. In any case, can you merge Tables 1 and 2?

RESPONSE: As per the reviewer’s suggestion, we removed the minimum and maximum values from Table 1. Given the distinct information included in separate Table 1 and 2, we would like to continue reporting that information separately and keep both Table 1 and 2. We felt that providing the information separately, instead of merging tables, is more user-friendly for our readers.

8. You should include sample sizes (N, n) in all the tables.

RESPONSE: Sample sizes are now included in all tables.

Reviewer 2 (Reviewer 2): PEER REVIEWER ASSESSMENTS:

OBJECTIVE - Full research articles: is there a clear objective that addresses a testable research question(s) (brief or other article types: is there a clear objective)?

Yes - there is a clear objective
DESIGN - Is the current approach (including controls and analysis protocols) appropriate for the objective?

No - there are minor issues

EXECUTION - Are the experiments and analyses performed with technical rigor to allow confidence in the results?

Yes - experiments and analyses were performed appropriately

Statistics - Is the use of statistics in the manuscript appropriate?

Yes - appropriate statistical analyses have been used in the study

INTERPRETATION - Is the current interpretation/discussion of the results reasonable and not overstated?

No - there are minor issues

OVERALL MANUSCRIPT POTENTIAL - Is the current version of this work technically sound? If not, can revisions be made to make the work technically sound?

Probably - with minor revisions

PEER REVIEWER COMMENTS:

GENERAL COMMENTS: This is a well-written and well-designed study that looks at the association between tobacco smoking and individual, household, and community factors. The authors have used nationally representative data, combined from two waves, to explore this question, and have designed robust models to test their hypothesis. I have a few minor comments that could help provide more clarity to the manuscript.
REQUESTED REVISIONS:

Page 4, lines 9-17 could do with some references.

RESPONSE: The claims are based on an understanding of the local economy and the in-situ observations of one of the co-authors (Dr. Samuel Gamtessa) during his time of residence and more recent visits to Ethiopia. We were unable to find any additional published sources to support these claims. In an attempt to address the reviewer’s comment, we have added the source of our information (Gamtessa, 2018) and have listed in the reference as ‘in situ observations’. We are open to any other suggestions on how to better address this valid point.

Page 4, lines 31-36: I am a bit worried about this statement, which is commonly used by the tobacco industry - the health risks of tobacco are just as bad, whether or not it is illicit. Citations are needed here, or a removal of the sentence.

RESPONSE: We see and appreciate the reviewer’s point. We have deleted this sentence in the revised manuscript.

What is the likelihood of duplicate responses in both surveys? How can the authors account for this in their study design?

RESPONSE: The likelihood of duplicate responses in the DHS is negligible as each household is selected randomly in each survey period and each household has a unique identifier. Another form of duplication could be when neighboring respondents copy each other responses. Unfortunately, we don’t know the likelihood of this particular occurrence. While it’s possible that the same households could be randomly selected in both survey years, this would not be considered duplication since surveys are conducted five years apart and many respondents’ characteristics would change during this time span.

In terms of terminology, I think the authors mean "sex" instead of "gender".

RESPONSE: We have replaced “gender” with “sex” in the revised manuscript.

In the discussion, can the authors provide examples of policies that take into account local and social contexts?

RESPONSE: On pages 26-29 of the Conclusion section, we have included examples of policies relating to the local and social contexts. For instance, to effectively reduce tobacco consumption
in the eastern region of Ethiopia where the khat consumption prevalence is highest in the country, policymakers could launch education campaigns to raise awareness about harmful effects of khat consumption along with tobacco consumption. In this region, efforts to deter khat consumption would have spillover effects for deterring tobacco smoking.

In contrast, in the Gambela region where khat prevalence is relatively low but there is a high social acceptability of tobacco smoking, we suggested that direct measures that deter tobacco smoking such as price incentives through excise taxes, banning smoking in public places should be paired with indirect measures that lower social acceptability of smoking practices and increase people’s inclinations to comply with these regulations. These include educational campaigns that raise awareness about the adverse health and economic effects of tobacco smoking. To elaborate further, we have added the following sentences on Page 27, lines 31-46 of the revised manuscript: “Following the 2015 regulation, there was a lack of a nationwide awareness campaign on the dangers of smoking [46]. Hence there continues to be a need for awareness campaigns. These public campaigns can utilize various media sources such as radio, TV, cell text messages, or social media campaign where available. They may be run in partnership with community organizations that engage youth, agricultural development extensions activities, rural health posts, and be part of broader health education added to the school curriculum.”

We have also included an explanation on pages 27-28 as to how these measures may affect religious groups differently, which may help target appropriate units. To further elaborate we added the following sentence on Page 28, line 12 of the revised manuscript: “However, it is important that these educational campaigns avoid any perceived stigma toward a specific community, but instead are administered in partnership with religious community leaders.”