Author's response to reviews

Title: Prevalence, distribution and social determinants of tobacco use in 30 Sub-Saharan African Countries

Authors:

Chandrashekhar T Sreeramareddy (chandrashekharats@yahoo.com)
Pranil Man Singh Pradhan (pranil.pradhan@gmail.com)
Shwe Sin (drshwesin@gmail.com)

Version: Date: 15 September 2014

Author's response to reviews:

Dear Editor
BMC Medicine
BMC series Journals
One behalf of our authors team, i first thank the journal for considering our manuscript for publication in BMC Medicine and quick review. After going through both reviewers' comments which are favorable and constructive, addressed most of the comments and revised the manuscript file accordingly. In addition, we included new data that become available after this manuscript was submitted to BMC Medicine. If any comments were unaddressed or new comments/suggestions during further consideration, we will address them.

Comoros DHS 2012 has collected information about tobacco use and in Mali latest data of DHS, 2012 has become available provided for both men and women (Mali 2006 data was available for women only). We included revised number in tables 1 & 2. We also updated the analysis particularly pooled data, after revising the re-categorization of religions according to reviewer Giovino's suggestion. In effect, numbers in tables 3 & 4 changed almost entirely, but did not highlight except for overall prevalence.

All changes are highlighted in R1 version of MS and point-by-point replies are explained below.

Title:
Prevalence, distribution and social determinants of tobacco use in 29 Sub-Saharan African Countries

Reviewer:
Dhirendra Narain Sinha
Reviewer's report:

My comments are as follows
1. Is the question posed by the authors new and well defined? Yes. Some country data is already in public domain but that does not affect the country group presentation.

2. Are the methods appropriate and well described, and are sufficient details provided to replicate the work? Yes

3. Are the data sound and well controlled? Yes but they should present prevalence by five year age group such as 15-19, 20-24 and so on and so forth, than the content of the article will be referenced at more as that data is used for standardized calculations

4. Does the manuscript adhere to the relevant standards for reporting and data deposition? yes

5. Are the discussion and conclusions well balanced and adequately supported by the data? In discussion they should include more literature search and comparison and validation with similar data and data from other different sources

6. Do the title and abstract accurately convey what has been found? yes

7. Is the writing acceptable? yes with the revisions suggested

Authors' replies

Point 1: We agree about some data, available already in public domain, we have provided references for them. However, our analyses includes more recent data, and a different approach to analysis i.e. social distribution and association of smoking and smokeless tobacco use with socio-demographic factors.

Point 3: In some countries men aged up to 64 years were included, but in all countries women aged 15-49 were included. We have clarified other reviewer's comment about 40-49/64 years included in the tables.

Having smaller age groups, will give smaller samples in age group particularly for men' data resulting in wider confidence intervals. So we retain the same age groupings.

Reviewer: Gary A Giovino

Reviewer's report:

The authors use data from 2006-2013 Demographic Health Surveys to estimate the prevalence of smoking and smokeless tobacco use in 29 Sub-Saharan African countries. There are many errors that need correcting.

Minor Essential Revisions:

1. The report misrepresents the Global Adult Tobacco Survey. It suggests, for example, in a statement in the second paragraph on page 3, that GATS does not adequately report SLT use, supporting that statement with reference 19. Reference 19 combines smoking and smokeless into a measure of tobacco use. A paper in Lancet by Giovino and colleagues provides estimates on SLT use, as
do other GATS papers and GATS country reports (on a CDC website). Also, the paper implies that GATS does not report data on countries in sub-Saharan Africa. This is true only for published papers. Data for Nigeria and Uganda are available on-line and representative surveys are being conducted in many SSA countries using GATS questions. While this level of detail does not need to be added, the implication that GATS does not report on smokeless tobacco and that GATS is not active in SSA needs correcting.

Authors' reply:

We agree with concern of the reviewer. We checked CDC website, to update our knowledge about countries covered by GATS. We feel that details about existing literature is required in background to highlight gaps in existing literature. As suggested, we rectified the mistakes and highlighted them as track changes. We included that two countries Nigeria and Uganda only were covered in Sub Saharan Africa by GATS and corrected reference 19 did not report smokeless tobacco separately. A new reference which is a systematic review was also included.

2. Page 3, para 1: it's "Framework Convention on Tobacco Control," not "Framework Control for Tobacco Control"

Authors' reply:

Rectified throughout the manuscript

3. The text and tables often refer to the 40-49 year old age group as >= 40 years. This is potentially misleading and should be changed to always read 40-49 years old.

Authors' reply:

Men's data in some countries, interviewed those aged up to 59 or 64 years. So we had used as >= 40 years. We agree this may be misleading. In all countries, women aged 15-49 years only were interviewed. To apply the category 40-49 for both men and women we rewrote age groups as 40-49 years but included a foot note to indicate men aged up to 59 or 64 years were interviewed in some countries.

4. The categorization of religion is incorrect in spots. For example, Anglican is a Protestant religion and Animism is not Christian. Also, the Results section does not discuss the findings on religion.

Authors' reply:

We reclassified the religions considering reviewer Giovino’s suggestion after which we pooled the data. This plus updated date changed number and % of social variables, etc in tables 3 & 4. We have included results on religion.

5. Regarding Figures 1 and 2, the Figure titles should indicate that these are among people who use tobacco products. There is a problem because there is
no indication that anyone uses more than one tobacco product, which many undoubtedly do. Thus, the figures likely misrepresent reality.

Authors' reply:

We have modified the title to include 'among tobacco using men in 29 Sub Saharan African countries' and added a foot note "% respondents using multiple tobacco products was small and is not presented"

6. In Table 4, there is no indication of what variables were statistically controlled for.

Authors' reply:

We have added suggested detail as a foot note

7. Page 7, second sentence: The phrase "age was associated with age" doesn't make sense.

Authors' reply:

We rectified the typo. It should read as "smoking and SLT use were associated with age"

7. Page 7, para 1: The results section discusses adjusted odds ratios as if they were relative risks (e.g., in terms of "times higher"). This may be acceptable for women, because the prevalence estimates are low. But it is not statistically accurate for men, because the prevalence estimates are higher. I am referring to the concept that odds ratios approximate the relative risk when prevalence is low.

Authors' reply:

We agree with suggested concept of epidemiology in interpretation of odds ratios and relative risks. However, we feel 'times higher' as the best way to present the results uniformly for both men and women.

Discretionary revisions:

8. Methods section of Abstract: It is not clear why there is "(64)" after "15-49" in line 2.

Authors' reply:

As per the explanation given to comment 3

9. Results section of the Abstract: in discussing adjusted odds ratios for education and wealth, indicate the direction of the relationships.

Authors' reply:

We intend to write a sentence about the direction of association but the word limit does not permit us to do so. We feel that odds ratio provided in brackets could indicate the direction of association i.e. OR <1 or >1.
10. Results section: I suggest that one decimal point is sufficient in reporting prevalence estimates in the text and tables.

Authors’ reply:

We have provided two decimals where possible. As sample sizes were large we plan to retain 2 decimals since it would be more precise.

11. Page 5, 3rd paragraph, first third sentence: I suggest inserting “most” so it reads, “In most East African countries, ...”.

Authors’ reply: The suggested changes were made.

12. Page 6, first two complete sentences: indicate that these data are shown in Figure 1 and also that the estimates are among people who used tobacco.

Authors’ reply: The suggested changes were made.

13. Page 6, para 2: In Gabon women also mainly smoked cigarettes.

Authors’ reply: The suggested changes were made.

14. Page 6, para 2, define "high" (i.e., provide the percentage cut-point used) for chewing tobacco and for snuff.

Authors’ reply: The suggested changes were made.

15. Table 3: I suggest an "Overall" line of data.

Authors’ reply: We include overall prevalence of smoking and SLT use among men and women at the beginning i.e. top of the table.

16. Page 6, third paragraph: Indicate in the first sentence that the data are from Table 3.

Authors’ reply: The suggested changes were made.

17. Pages 7 - 9: I think the discussion section provides too much repetition of results and not enough interpretation.

Authors’ reply:

The first paragraph is a reiteration of principal findings followed by discussion in which we have interpreted and compared the findings with existing literature. While doing so some repetitions were inevitable.

18. Page 7, paragraph 2: the survey did not assess use of manufactured cigarettes - only cigarettes. I suggest dropping the word "manufactured"

Authors’ reply: The suggested changes were made.

19. Page 8, paragraph 1: GATS provides estimates for Nigeria and Uganda, on the website.
Authors' reply: We have included Nigeria and Uganda here because data was available from CDC website.

20. Page 8, para 3: It is not clear to this reviewer why the age differences observed would indicate a cohort effect.

Authors' reply:

current smoking which increases from the age of 15 years onwards up to 40-49 years is due to cohort effect which means that smoking was less likely to be initiated in more recent decades.

21. Page 8, para 3, last sentence: perhaps the suppression of hunger is another reason why poor people use tobacco.

Authors' reply:

We also feel it could possibly be the reason, but we could not find evidence for the same. So we did not write this in discussion.

22. Page 9, top paragraph: while the religions mentioned do not promote tobacco use, I believe that Islam proscribes against use more strongly than the other religions.

Authors' reply:

We agree, but Hookah smoking is widely practiced and culturally acceptable in many Arab countries. Hence we did not mention that Islam Proscribes against tobacco use as it contradict the reality.

23. Page 9, para 3, last sentence: the FCTC is in force in Zimbabwe.

Authors' reply:

We deleted Zimbabwe from here.

24. Page 10, last sentence: add mention of promoting cessation.

Authors' reply:

We include this statement

25. General comment: the paper needs a copy edit.

Authors' reply:

We have proof read the whole article once again. Some typos and omissions were corrected.