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)	Prevalence, pattern and socio-demographic differentials in smokeless tobacco consumption in Bangladesh: Evidence from population based cross-sectional study in Chakaria
AUTHORS	Mia, Mohammad Nahid; Hanifi, Syed; Rahman, M. Shafiqur; Sultana, Amena; Hoque, Shahidul; Bhuiya, Abbas

VERSION 1 - REVIEW

REVIEWER	Chadrashekhar T Sreeramareddy Division of Community Medicine International Medical University Bukit Jalil Kuala Lumpur Malaysia
REVIEW RETURNED	08-Jun-2016

GENERAL COMMENTS	Thank for the opportunity to review this manuscript. I read this with great interest, but the message from the article not clear, as methods are cross-sectional sample covered is representative of small geography i.e. HDSS site, only smokeless tobacco use prevalence (and its two types) are covered, the associated factors tested by multiple regression analyses are well reported. In addition to above, the writing is very poor, for example "Smokeless tobacco has adverse health effects like smoking. However, its public health threat is often “ignored” by the researcher and the policy maker" is poorly constructed sentence. Such language errors are present through out the paper. There not added value of the predicted probabilities over multivariate analyses. There are several reports by Palipudi, Sinha DN, Sreeramareddy covering national-level bigger sample data with more comprehensive pattern of tobacco use. Too many numbers are repeated under results section, instead of explaining the key results by referring to tables. Articles by Sreeramareddy, Siddiqi are glaring omissions from Reference. But i affirm that (as i have disclosed the reviewer identity) is not a conflict of interest for comments enclosed in the review here. With those comments i will be happy re-review if a revised version of this article is resubmitted to BMJ Open

REVIEWER	Prof OA Ayo-Yusuf Sefako Makgatho Health Sciences University, Medunsa, South Africa
REVIEW RETURNED	26-Jun-2016

GENERAL COMMENTS	This study reports on the correlates of use of two smokeless tobacco products seemingly popularly used in Bangladesh and demonstrated distinct gender preferences for the two different products. While, this has the potential to influence policy, the information provided remained scant. Importantly, the paper could be made significantly shorter as a brief report, as the paper mostly repeated the literature and the discussion section in particular had little to do with the interpretation of the results from this study. The paper would also need language editing before consideration for publication. The authors may want to consider other detailed comments provided below in revising their paper: Abstract Pg 2; line 14: The objectives suggest 'different types' of smokeless tobacco were to be studied, when in fact only two (Sadapatha and Zarda) products were studied. It might be more appropriate to then rather replace the phrase with '…….two commonly used smokeless tobacco in a rural…….' Pg 3; Line 3: The concluding paragraph is not supported by the data presented in the study. For instance, the statement on perceived harm related to lack of regulatory control conditions is not supported by this study's findings. The authors should stick to conclusions related to study objectives and supported by data presented in this study. Introduction Pg 4; lines 23-25: It is technically incorrect to talk about regulation to control nicotine levels, but rather change this to 'tobacco use'. i.e. '……controlling the level of tobacco use, whereas in the low-income……' Pg 4; line 35; remove the phrase 'more exposed'. Pg 5; line 8: Although, TB and COPD was added to the chronic diseases cited here, neither reference 13 nor 14 dealt with TB or COPD as they seem focused on hypertension and diabetes. The authors might want to add Ayo-Yusuf et al (2008) that reported association of snuff use with chronic bronchitis. Pg5; line 42-43: The authors emphasized that very limited studies have been conducted particularly in relation to two smokeless tobacco products they considered 'special forms of tobacco products', but failed to provide a justification for the isolation of these two particular products. Are they the most popularly used or most common products in the rural areas or in Bangladesh in general? Please provide better justification for the selection of these two products in particular. Materials and Methods Pg 6; lines 15-30: These are really not relevant to the study's objectives. I suggest removing these. Pg 7; line 30: The dependent variable for the study is stated as 'current status of SLT consumption' but in the abstract, the main outcome (which is equivalent to dependent variables), was stated as Sadapatha and Zarda. The authors need to be consistent. Pg 8; line 6: Add at least a reference here to support the statement that this approach is suggested in similar literature. Pg 8; line 38-42: I am not convinced that GEE is indicated as a way to account for lack of independence of observation so as to avoid Type I error. GEE is usually used for repeated measures with at

	least 4 time points. The cluster effect for complex sampling as rightly indicated needs to be accounted for and one can easily do this using the logistic regression for complex samples, which can be done in STATA using the 'svy' command for survey data analysis. Results Pg14; line 8-10: I think figures 2 and 3 are redundant given that the same information has been presented in Table 2. Except otherwise justified, these figures should be deleted. Discussion The whole discussion section needs to be reworked. Most of this section was on literature review rather than interpretation of the study's findings. I suggest brevity and perhaps a brief report would be more appropriate publication format. Conclusions See my previous comments above under abstract that also applies here.

VERSION 1 – AUTHOR RESPONSE

Reviewer 1:
Comment1: In addition to above, the writing is very poor, for example "Smokeless tobacco has adverse health effects like smoking. However, its public health threat is often "ignored" by the researcher and the policy maker" is poorly constructed sentence. Such language errors are present throughout the paper.

Answer: We have corrected the sentence like " Smokeless tobacco (SLT), consist of low-grade ingredients, has adverse health hazard like smoking tobacco. However, unlike smoking, limited initiatives have been taken to control its use in general population, despite its widespread use particularly in low-and-middle countries including Bangladesh. Thus, it is important to examine the prevalence of SLT use and its social determinants for designing appropriate strategies and programmes to control SLT use." We have reworked throughout the manucripts for correcting language errors.

Comments2: There not added value of the predicted probabilities over multivariate analyses. There are several reports by Palipudi, Sinha DN, Sreeramareddy covering national-level bigger sample data with more comprehensive pattern of tobacco use.

Answer: we have removed the predicted probability graphs from our analysis as also suggested by the second reviewer.

Comment3: Too many numbers are repeated under results section, instead of explaining the key results by referring to tables.

Answer: We have rephrased such comments to remove the repeated numbers and explained the key findings of the study.

Comment 4: Articles by Sreeramareddy, Siddiqi are glaring omissions from Reference. But i affirm that (as i have disclosed the reviewer identity) is not a conflict of interest for comments enclosed in the review here.

Answer: We would like thank the reviewer for his suggestion to include the references. We have included reference no. 4 and 39 to better justification of our results and interpretation.

Reviewer 2:

Comment 1: This study reports on the correlates of use of two smokeless tobacco products seemingly popularly used in Bangladesh and demonstrated distinct gender preferences for the two different products. While, this has the potential to influence policy, the information provided remained scant. Importantly, the paper could be made significantly shorter as a brief report, as the paper mostly repeated the literature and the discussion section in particular had little to do with the interpretation of the results from this study. The paper would also need language editing before consideration for publication.

Answer: We appreciate the comment and the suggestion. We have improved the introduction and discussion to better explain our study objective and discuss how it relates to the findings. We have rephrased first paragraph of the discussion, and also included two addition paragraphs (2 3).
Abstract:
Comment 1: pg 2; line 14: The objectives suggest 'different types' of smokeless tobacco were to be studied, when in fact only two (Sadapatha and Zarda) products were studied. It might be more appropriate to then rather replace the phrase with '…….two commonly used smokeless tobacco in a rural…….'

Answer: Done as suggested in page 2 under the objective section in abstracts.

Comment 2: Pg 3; line 3: The concluding paragraph is not supported by the data presented in the study. For instance, the statement on perceived harm related to lack of regulatory control conditions is not supported by this study's findings. The authors should stick to conclusions related to study objectives and supported by data presented in this study.

Answer: We have improved the conclusion to support our data presented in the study as follows: "Prevalence of SLT consumption was high in the study area, and socioeconomically disadvantaged groups had a higher exposure to the hazard of consuming SLT. The limitation of the existing regulatory measures for controlling the use of non-industrial SLT products should be understood and discussion for developing new strategies to control use of SLT should start."

Introduction:
Comment 3: Pg 4; line 23-25: It is technically incorrect to talk about regulation to control nicotine levels, but rather change this to 'tobacco use'. i.e. '……controlling the level of tobacco use, whereas in the low-income……'

Answer: Done as suggested in first paragraph of page 4 under the introduction section.

Comment 4: Pg 4; line 35: remove the phrase 'more exposed'.

Answer: We have removed the phrase 'more exposed' and also rephrased as This perhaps exposes the disadvantageous section of the population more to the health hazard of smokeless tobacco products (SLT) in first paragraph of page 4 under the introduction section.

Comment 5: Pg 5; line 8: Although, TB and COPD was added to the chronic diseases cited here, neither reference 13 nor 14 dealt with TB or COPD as they seem focused on hypertension and diabetes. The authors might want to add Ayo-Yusuf et al (2008) that reported association of snuff use with chronic bronchitis.

Answer: We would like to thank the reviewer for suggesting the reference. We have included the reference no. 16 as suggested by the reviewer.

Comment 6: Pg 5; line 42-43: The authors emphasized that very limited studies have been conducted

particularly in relation to two smokeless tobacco products they considered ‘special forms of tobacco products’, but failed to provide a justification for the isolation of these two particular products. Are they the most popularly used or most common products in the rural areas or in Bangladesh in general? Please provide better justification for the selection of these two products in particular.

Answer: Bangladesh is one of the main cultivators of tobacco in the world, and tobacco leaves are easily available for use as SLT. Zarda and Sadapatha are the most two common form of SLT used by the people in rural settings and people with low-socioeconomic status. A study of Azim et el also showed that among the various form of SLT, zarda and sadapatha are the most two popular form in such settings.

Method and materials:
Comment 7: Pg 6; lines 15-30: These are really not relevant to the study’s objectives. I suggest removing these.

Answer: We have removed the lines except “Over 3393 acres of agricultural land have been used for tobacco plantation which could have produced different other crops” as we think it would be relevant to this study.

Comment 8: Pg 7; line 30: The dependent variable for the study is stated as ‘current status of SLT consumption’ but in the abstract, the main outcome (which is equivalent to dependent variables), was stated as Sadapatha and Zarda. The authors need to be consistent.

Answer: Done as suggested in first paragraph of page 7 under ‘Definition of variable’ section.

Comment 9: Pg 8; line 6: Add at least a reference here to support the statement that this approach is suggested in similar literature.

Answer: We have removed it.

Comment 10: Pg 8; line 38-42: I am not convinced that GEE is indicated as a way to account for lack of independence of observation so as to avoid Type I error. GEE is usually used for repeated measures with at least 4 time points. The cluster effect for complex sampling as rightly indicated needs to be accounted for and one can easily do this using the logistic regression for complex samples, which can be done in STATA using the ‘svy’ command for survey data analysis.

Answer: GEE is usully used for correlated binary responses. And the correlated responses may occur in both longitudinal and clustered data. In longitudinal studies, one subject have 4/5 repetead observations and they are correlated as they are from the same subject. In clustered data, (household or region or school) there are multiple individuals in a cluster and hence there are multiple responses for the same outcome. These multiple responses for the same outcome from the same cluster (household) are correlated. As we includes all members aged above 13 years in a household and therefore there are multiple responses for SLT use from these members from the same household and hence there are correlated. Therefore, we used GEE to allowing for the correlation. We added explanation of using GEE and added the references in text (page 5 at the end of section statistical analysis). The references we used focused on using GEE for the clustered binary data. There are also several other references where they used GEE for clustered binary data.

Regarding survey command “svy” in Stata, yes we agreed. This command is used to obtain weighted estimate from the data based on complex survey (using multistage sampling). However, our study is not based on multistage sampling, and hence weighting is not required. However, by using GEE we

adjusted effect of correlation in the estimated standard error of the regression coefficients rather than weighted estimate of the parameter.

Results
Comment 11: Pg14; line 8-10: I think figures 2 and 3 are redundant given that the same information has been presented in Table 2. Except otherwise justified, these figures should be deleted.

Answer: We have removed the figures as suggested.

Discussion
Comment 12: The whole discussion section needs to be reworked. Most of this section was on literature review rather than interpretation of the study's findings. I suggest brevity and perhaps a brief report would be more appropriate publication format.

Answer: We appreciate the comment and the suggestion. We have improved the introduction and discussion to better explain our study objective and discuss how it relates to the findings. We have rephrased first paragraph of the discussion, and also included two addition paragraphs (2 3) in page 14 under the discussion section.

Conclusion:
Comment 13: See my previous comments above under abstract that also applies here.
Answer: We have done our conclusive remark within the discussion section.

VERSION 2 – REVIEW

REVIEWER	Chadrashekhar T Sreeramareddy International Medical University Kuala Llumpur Malaysia
REVIEW RETURNED	04-Sep-2016

GENERAL COMMENTS	Despite the edits and language corrections made writing errors still remain. For example, in abstract "Smokeless tobacco (SLT) has adverse health hazard like smoking tobacco". Interpretation of odds ratios is wrong, please amend Smokeless tobacco use not wide spread in LMICs but just in South Asian countries and 1-2 African countries, authors are advised to read papers by Sreeramareddy et al. Such inaccurate statement to be corrected. Most the literature about health hazards has been described in great detail is not not necessary, if this paper is considered as a short report. The prevalence of SLT is indeed known, authors should refer to papers by Sinha et al. Sreeramareddy et al. and Siddiqui et al. Lines 30-53 is not adequate justification, cite the suggested literature here, identify the gaps in literature, and what authors would like to contribute to exisiting knowledge, from this paper.

	The results are still detailed and overly descriptive; i suggest authors to provide 95% CIs for their prevalence estimates, and they should be estimated giving consideration for clustering at the household level. Authors should explain how the asset quintiles were obtained in this study, citing a reference is insufficient. Tables RC is incorrect, write in full reference category or indicate odds ratio as 1 (one)

REVIEWER	Olalekan Ayo-Yusuf Sefako Makgatho Health Sciences University, Pretoria, South Africa
REVIEW RETURNED	25-Sep-2016

GENERAL COMMENTS	The authors have satisfactorily address my review concerns and I have no further comments.

VERSION 2 – AUTHOR RESPONSE

Author's Response to Decision Letter for (bmjopen-2016-012765.R1)

Prevalence, pattern and socio-demographic differentials in smokeless tobacco consumption in Bangladesh: Evidence from population based cross-sectional study in Chakaria

Author's response to the reviews

Below is an exhaustive list of the substantive comments from the reviewers with our responses. We would like to thank the reviewers for these comments and feel the updated manuscript is much improved due to their input.
Reviewer 1:
Comment1: Despite the edits and language corrections made writing errors still remain. For example, in abstract "Smokeless tobacco (SLT) has adverse health hazard like smoking tobacco".
Answer: "Smokeless tobacco (SLT) has adverse health hazard like smoking tobacco" is replaced by "Health hazards associated with use of smokeless tobacco (SLT) are similar to those of smoking." Please see in the background of abstract in page 2 line 6-7. We have made language editing and corrections where necessary.

Comment2: Interpretation of odds ratios is wrong, please amend
Answer: We have made the correction as suggested. The previous interpredtation "Odds of female using sadapatha were 3.5 times of male, and odds of male using zarda were 3.6 times of female" is replaced by "the odds of being a sadapatha user were 3.5 fold greater for female compared to male and the odds of being a zarda user were 3.6 fold greater for male compared to female." Please see the result section of abstract in page 3 line 3-6.
Comment3: Smokeless tobacco use not wide spread in LMICs but just in South Asian countries and 1-2 African countries, authors are advised to read papers by Sreeramareddy et al. Such inaccurate statement to be corrected
Answer: We have corrected as "its widespread use particularly in South-and-Southeast Asian countries including Bangladesh" instead of "its widespread use particularly in low-and-middle income countries including Bangladesh" and added the suggested reference. Please see in the background of abstract in page 2 line 11-12 and the reference no. 4, 30 and 31.

Comment4: Most the literature about health hazards has been described in great detail is not necessary, if this paper is considered as a short report.
Answer: We have excluded line 15-22 of page 5 about describing health hazard as suggested by reviewer. We have also restructured the second paragraph of introduction in page 5 line 3-27 as "As reported in many studies SLT products contain high toxicity, mutagenicity and cancer-causing elements and roughly 4,200 low-graded chemicals, which would disproportionately affect health. Therefore, like smoking tobacco, SLT are also associated with several adverse health outcomes in general including cancer, cardiovascular disease, oral pain, chronic disease (hypertension, diabetes, TB, COPD), and life-treating complications in reproductive stages of women with adverse pregnancy outcomes (stillbirth, low birth weight). Despite adverse health hazards of SLT, unlike smoking tobacco, very limited initiatives from the government and non-governmental agencies have been undertaken to control its use."

Comment5: The prevalence of SLT is indeed known, authors should refer to papers by Sinha et al. Sreeramareddy et al. and Siddiqui et al. Lines 30-53 is not adequate justification, cite the suggested literature here, identify the gaps in literature, and what authors would like to contribute to exisiting knowledge, from this paper.
Answer: We would like thank the reviewer for his suggestion to include the references. We have added these lines "SLT use prevalence in most of the South Asian countries is much higher than in sub-Saharan Africa, Central and Western Asia and other developed countries. In South and Southeast Asia SLT was used in diverse forms particularly in Bangladesh, where an increasing array of SLT products such as sadapatha is widely available" in the third paragraph of introduction section in page 5 lines 30-53. The suggested references also cited. Please see reference no. 4, 22-26.

Comments6: The results are still detailed and overly descriptive; i suggest authors to provide 95% CIs for their prevalence estimates, and they should be estimated giving consideration for clustering at the household level.

Answer: We are highly appreciated the reviewer for this valuable comment. We have included 95% CI which is considering for clustering at the household level in table 1 in page 9 line 27. We also deleted the lines 3-25 of page 9 as per reviewer suggestion.

Comments7: Authors should explain how the asset quintiles were obtained in this study, citing a reference is insufficient.
Answer: In principal component analysis, the first component (a linear combination of the assets weighted by the coefficient or factor loading) that possesses maximum variability in the data was used to calculate the score or wealth index. The wealth index was then used to categorize the individuals into five equal groups (or quintile). The first quintile is the poorest 20% of the households and fifth quintile is the wealthiest 20% of the households. Please see the second paragraph of page 7. We also included reference number 34, 35 and 36 as suggested by reviewer.
Comments8: Tables RC is incorrect, write in full reference category or indicate odds ratio as 1 (one)
Answer: Done as suggested. We have corrected odds ratio 1 as reference category instead of RC. Please see the table 2 in page 11.

VERSION 3 – REVIEW

REVIEWER	Chandrashekhar T Sreeramareddy Division of Community Medicine International Medical University Bukit Jalil, Kuala Lumpur Malaysia
REVIEW RETURNED	21-Nov-2016

GENERAL COMMENTS	The authors have addressed most of my comments on the previous version.However, there are writing errors through the paper, which range from technical, typos and language errors. I have highlighted a few in the abstract. Such errors may be looked for in the main paper. Examples: to investigate, SD ±13.6, compenent, multivariable logistic regression analysis etc.