Author’s response to reviews

Title: Patterns and socioeconomic influences of tobacco exposure in tobacco cultivating rural areas of Yunnan Province, China

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Author’s response to reviews: see over
Cover Letter

To editors of BMC Public Health

Dear Sir or Madam:

I am submitting the revised manuscript “Patterns and socioeconomic influences of tobacco exposure in tobacco cultivating rural areas of Yunnan Province, China” (MS: 9906752556931176) to BMC Public Health. I have addressed my responses to the comments in the revised manuscript, and given a point-by-point explanation to the concerns in this cover letter.

The revisions/additions of the content are underlined.

Best wishes.

Cai Le

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Reviewer: Tim Elwell-Sutton
Reviewer's report:
I would like to thank the authors for their revisions. I feel they have done a very good job of responding to my previous concerns. Some minor points remain to be addressed. I list them below along with the comments from the original reviewers and the authors' original responses.
Reviewer #1: Yueh-Hsiu Mathilda Chiu
Minor Essential
3. Only one county each for economically advantaged and economically disadvantaged groups from Yunnan province was selected, is this county representative of each group? Please provide more information on this. Also, what is the wealth distribution of economically advantaged and disadvantaged group? Is this determined arbitrarily?
Done as what you suggested. Please Page 5, third Paragraph.
# Your definition of economically advantaged and disadvantaged on P5 needs to be amended. Currently the two groups are <$600 and >$600. One of these should be # or #. Which category do those with exactly $600 fall into?
The two groups should be <$600 and ≥$600. We have revised it. Please see Page 5, last Paragraph.

Discretionary
5. According to the questionnaire, it seems that the authors also asked about the intensity of smoking (average amount of cigarettes smoked every day). I'd suggest the authors also do an analysis examining the association between SES factors and amount of smoking, as this would provide some sort of quantitative information and strengthen the study.
Previously, we analyzed the association between SES factors and amount of smoking, but the result indicated no any relationship between SES factors and amount of smoking. So the information was not presented in this paper.
# Please do report your findings on this point. Null results may not be seem interesting but they are scientifically important. I suggest that you mention that you did test for this but found no association.
Done as what you suggested. Please see Page 9, 2nd Paragraph, and Table 3.

Reviewer #2: Tim Elwell-Sutton
Discretionary
1. Introduction, 2nd paragraph. The authors give useful information on studies of tobacco use in developed countries and mention one other study from Korea but do not directly reference any studies from China here. The authors state that “little data are available concerning the associations between SES
variables and tobacco use in the general adult Chinese population. In the results and discussion sections 2 studies of SES and smoking are referenced. Many other studies exist. For example, several studies from China show a negative association between education and smoking (Anson and Sun 2004; Wu, Liu et al. 2004; Li and Zhu 2006). A review of previous studies from China should be given to show what is already known so that the reader can understand what gap in the research this paper fills. Thanks very much for your good comments. We have cited the Chinese study and revised the sentence. Please see Page 4.

# Thank you for citing these studies. I think you also need to include a short summary of their findings.

Done as what you suggested. Please see Page 4, last Paragraph.

Minor Essential
2. Definitions. I was confused by the use of the term SHS in this paper. You give one SHS. One measure (described in the definitions section) is based on the responses of non-smokers. But in Table 3 you also present results for a different measure of SHS based on the responses of smokers. Please clarify in the definitions section that there are two different types of SHS being measured and please make this distinction clear throughout the paper.

Done as what you suggested. Please see Definitions section, Page 6.

# Thank you, this point is now clearer throughout the paper. However, I am still a little confused by why your definition of location of exposure to SHS was restricted to current smokers only. Was this question not also asked to non-smokers? I would expect it to be more important for non-smokers as SHS is their only source of exposure. Please explain.

This question was only asked to current smokers, this is the drawback of our questionnaire. We will improve it in our future work.

Minor Essential
3. Statistical Analysis. In the results, a number of p-values are given for differences between men and women based on percentage differences. Please explain in the analysis section how these p-values were calculated. Are they based on chi-squared tests?

Yes, p-values were calculated based on chi-squared tests. We have explained it in Statistical Analysis section. Please see Page 7.

# Thank you. Please also add text to the footnotes in Tables 1 and 2 to explain where these p-values come from – i.e. change the asterisked footnotes from “P<0.05” to “P<0.05 from chi-squared test for difference between sexes”.
Done as what you suggested. Please see Table 1 and 3.

Minor Essential
9. Table 4, logistic regression analysis. Given that there are huge differences in smoking behaviour between men and women, adjusting for sex is insufficient. Ideally, you should test for statistical interactions between sex and SES and then present sex-stratified analysis if interactions are found. Alternatively, you could simply present sex-stratified analysis without testing for interactions since it is known a priori that patterns of smoking are very different in men and women in this population.

We have tested for statistical interactions between sex and SES, but no interaction between sex and SES variables, so we don’t present sex-stratified analysis. We have added some sentences to explain it. Please see Results section, Page 8.

# Thank you for the changes made here. Please also mention the tests for interaction in the statistical analysis section and explain the rationale – i.e. due to large differences in smoking behaviour between men and women. Also in reporting the results of the interaction tests on P9 please delete the word “sensible”.

Done as what you suggested. Please see Page 7, Statistical analysis section, and Page 9, 2nd Paragraph.

Minor Essential
10. Table 4, logistic regression analysis. You have adjusted models for a range of possible confounders (age, sex, tobacco cultivating status and alcohol drinking). I would strongly suggest that all models should also be mutually adjusted for other measures of SES to allow the reader to see the independent associations of the three different measures of SES included (ethnicity, education and income).

Done as what you suggested. Please see Table 4.

# Thank you for the changes made here which have improved the logistic regression analysis but there are still problems with the results presented in Table 4.

It appears to me that all four models in fact contain the same variables. If this is the case, you only need to present one model. Unless you have some specific reason for including several models with different adjustments, I would suggest that you simply present one model for each outcome and present results for all relevant variables included in that model.

Done as what you suggested. Please see Table 4.
The “n=” numbers at the top of each column look incorrect to me. It looks like you have given the number of positive outcomes (e.g. the number of current smokers) as opposed to the actual number of individuals included in the analysis. N should be the total number of people included in the analysis regardless of whether their outcome was 0 or 1.

We have corrected the mistake. Please see Table 4.

Minor Essential
12. Discussion, paragraph 1 and paragraph 5. Throughout the discussion section please make a clear distinction between SHS exposure (reported by non-smokers) and SHS reported by smokers. See point 2 above.
Done as what you suggested. Please see paragraph 1 and paragraph 5.
# Thank you for this. I would suggest some editing of the language in the first sentence of para 1. I suggest: “The findings indicate a high prevalence of smoking and exposure to SHS among non-smokers. There was also a high rate of exposure to others’ smoke among current smokers in public places…”
Done as what you suggested. Please see Page 9, Discussion section, 1st Paragraph.

Minor Essential
13. In Table 2. Please make clear what the denominators are for your calculation of percentages. I think you have calculated nicotine dependence as a % of smokers and SHS as a % of non-smokers but please make this clear in the table and in the results text.
Done as what you suggested. Please see Table 2.
# I do not see info on the denominators in Table 2 though you have added it to Table 3. I suggest that for table 2 you simply change the column headings or add footnotes. E.g. for the heading “Prevalence of nicotine dependence” add “amongst smokers” and for the heading “Prevalence of SHS” add “Amongst non-smokers”.
Done as what you suggested. Please see Table 2.

Minor Essential
17. Since a stratified sampling procedure was used for this sample, the sample may not be fully representative of the target population (e.g. the rural population of Yunnan province). The validity of the analysis could be improved
by weighting the analysis to reflect the characteristics of the target population (by age, sex and education, for example). Weights could be applied to percentages, estimates of prevalence and logistic regression analysis, though it would particularly improve the validity of the prevalence estimates. The “survey” package in R allows calculation of percentages and logistic regression using weights.

Done as what you suggested. We don’t weight the characteristics of the study population, whereas the prevalence of current smoking, nicotine dependence and exposure to SHS was estimated based on weighted proportions. Please see Statistical Analysis section, Results section, and Table 2 and 4.

# Thank you very much for taking up this suggestion which does improve your results and must have involved a lot of extra work. One question remains. You mention in that you weighted using data from the national census. Did you weight to the national population or to the population of Yunan Province. I think weighting to the Provincial population might be more appropriate since that is really your ‘target population’. In either case, please mention in the methods section exactly which population weights were used.

We weighted to the Yunnan provincial population. We have corrected the sentence. Please see Page 7, Statistical analysis section.

**Quality of written English:** Needs some language corrections before being Published

One of our authors, Dr. Abhinav Goyal, has thoroughly reviewed and copyedited the manuscript. Dr. Goyal is a native English speaker, academic physician, and epidemiologist who is experienced in publishing manuscripts in Western journals.