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.

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Reviewer #1: Yueh-Hsiu Mathilda Chiu

1. In Introduction or Study region section, please provide the information on smoking legislation/policy in Yunnan province.

Done as what you suggested. Please Page 5.

2. [Methods, Study region] The authors reported that tobacco is cultivated in 45 counties in Yunnan -- please provide information on how many counties in total are in Yunnan province.

Done as what you suggested. Please Page 5, second Paragraph.

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.

4. Since the data were from two different counties with different economic environment, I suggest that the authors also adjust for this factor in the multivariable logistic regression.

Done as what you suggested. Please see Table 4.

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.

6. Nicotine dependence was categorized by FTND scores. Were the cutpoints that the authors used determined arbitrarily/a priori? Or was this suggested by
previous studies? I've seen slightly different cutpoints used in other studies. Please provide more information and citations.

We checked the cutpoints determined in our paper, it is the same cutpoints determined by other studies (For example: Heatherton TF, Kozlowski LT, Frecker RC. The Fagerström Test for Nicotine Dependence: a revision of the Fagerström Tolerance Questionnaire. Br J Addict 1991, 86:1119-27.)

In order to avoid confusion, we rewrite these sentences. Please see Page 7, 1st Paragraph.

7. The numbers and percentages in [Table 2] are somehow confusing. Take first column (current smoking) for example, the number of current smoking men was 2780, but what does 68.3% mean (what is the denominator?)? Likewise, for “All” current smokers, the number was 2817, but what does 32.5% mean? All the percentages listed in this table are confusing. In addition, some numbers do not add up correctly (for example, “nicotine dependence” block, 2366 men and 27 women are 2393 in total, not 2395). Please fix the table or include a footnote/description about how the percentages were calculated, and also please double-check the numbers.

68.3% means the prevalence of current smokers is 68.3%. I have explained it in Table 2. Please see underline words in Table 2.

Sorry for the mistake! We have corrected the wrong numbers.

8. For SHS exposure analysis, it seems that the authors only restricted to non-smokers. I'd like to note that the smokers can also be exposed to SHS, and the health effects of SHS and direct smoking might be different so it is also interesting to look at SHS in smokers.

Yes, in our paper, exposure to SHS was defined as non-smokers that reported exposure to environmental tobacco smoke at home or work for a minimum of 15 minutes at least one day per week.

Thanks for your very good suggestion! The results of SHS in smokers also have been presented in Table 3 (“In the past seven days, the conditions under which others have smoked in front of you”).

9. [Table 3 & Table 4]: Please provide total n for each of the 3 columns.
Done as what you suggested. We also corrected some wrong percentages in Table 3. Please see Table 3 and 4.

10. [Methods, 2nd paragraph]: Please spell out the full terms of GDP.

Done as what you suggested. Please see Page 5.

Reviewer #2: Tim Elwell-Sutton

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.

2. Definitions. I was confused by the use of the term SHS in this paper. You give one definition in this section but you appear to measure two different types of 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.

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
4. Table 2 reports prevalence giving counts and percentages. Confidence intervals should be given for the prevalences reported in Table 2 and the method of generating them should be described in the analysis section: see Li, Hsia et al. (2011) for an example. I would also suggest that for comparing prevalences between men and women, the confidence intervals should be used rather than p-values.

Done as what you suggested. Please see Statistical Analysis section, Page 7 and Table 2.

5. In Table 1, please report p-values (or indicate statistical significance through asterisks) for differences between men and women and indicate the type of test used to calculate them. You have reported statistical significance in the text, please show it in the tables also.

Done as what you suggested. Please see Statistical Analysis section, Page 7 and Table 1.

6. In Table 2 please include confidence intervals for prevalence estimates (see point 2 above). In reporting the results of table 2, I would suggest reporting CIs rather than p-values. If CIs overlap, it may not be valid to say that prevalence is higher in one group than another.

Done as what you suggested. Please see Table 2.

7. In Table 3 please report p-values as suggested for Table 1.

Done as what you suggested. Please see Table 3.

8. Table 3. The meaning of the last question is not clear to me. It reads “In the past seven days, the location of someone smoked”. Please clarify the English. Perhaps it should be “In the past seven days locations where you have smoked”. It would also help to give an English translation of the actual question used in a footnote.

We have changed this sentence as “In the past seven days, the conditions under which others have smoked in front of you”.
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.

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.

11. The discussion section should contain more comparison with previous studies of SES and smoking behaviour in China – see point 1 above.

Done as what you suggested. Thank you very much for providing us these very useful references! We have cited them in this paper.

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.

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.
14. Results, paragraph 3. You report nicotine dependence as being found in 30.5% of the study population. This is not a figure found in Table 2. Please clarify. If you choose to report nicotine dependence as a percentage of smokers in Table 2, then it must also be reported that way in the results text.

We have corrected the wrong number. Please see Page 8, 2nd Paragraph.

15. Table 3. Please clarify how percentages have been calculated. I think these are all % of smokers but please state that clearly and give the number of smokers for each column in the first row of the table.

Done as what you suggested. We also corrected some wrong percentages in Table 3. Please see Table 3.

16. Discussion paragraph 10. You state that “the association of individual income and smoking in controversial. The reason for this discrepancy is unclear. The exact nature of the association between income and smoking needs further investigation.” Please clarify this point. The meaning is unclear. Why is the association “controversial”? Which discrepancy are you referring to? And why does the association between income and smoking need further investigation?

To avoid confusion, we removed the sentence “However, the association of individual income and smoking is controversial. The reason for this discrepancy is unclear. The exact nature of the association between income and smoking needs further investigation.”, and add one sentence as “The reasons for inconsistent effect of income on Chinese smokers are unknown.”

Please see Page 12, 1st Paragraph.

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.

18. Table 4. I would suggest that you also report odds ratios for tobacco cultivating status.

Done as what you suggested. Please see Table 4.

Finally, we would like to thanks for Tim Elwell-Sutton’s very good comments, we have learned more statistical analysis methods during the revision period.