Author's response to reviews

Title: Religious Involvement and Tobacco Use in Mainland China-a preliminary study

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Author's response to reviews: see over
Letter to the Editor

Dr. Zubair Kabir

BMC Public Health

Dr. Zubair Kabir:

We appreciate the opportunity to resubmit our article titled “Religious Involvement and Tobacco Use in Mainland China-a preliminary study” (MS: 6505567231382454) for consideration for publication in the Journal. We have revised the manuscript along the lines suggested by the reviewers, and it is now stronger and clearer. We detail the changes below.

Sincerely,

Zhizhong Wang, Ph.D.

#1Reviewer’s comments:

1. No operational definitions of the smoking status were provided
   Response: We defined the smoking as “lasting at least two months when you smoked at least once per week”. See page8,line7-8

2. ICD-10 of Tobacco Use Disorders need to clearly spelled out
   Response: we have clearly spelled out the ICD-10 of Tobacco Use Disorders in whole manuscripts. See page8,line9. and tables 1-4.

3. Among the limitations- the low participation rate (2770/6476) need to be Highlighted
   Response: Subsampling was used in most surveys to reduce respondent burden by dividing the interview into two parts. Part I, administered to all respondents, assessed core mental disorders (n=5810), consequently with a participation rate (5810/6476). And part II assessed additional disorders and correlates. This paper only analyzed the random selected subsample data finished in stage II(n=2770). We suppose the participation rate same with entire sample. We have rephrased the sentence to make it more clear. See page6,line23-28.

4. Some additional information on the proposed "computer program" for selection of study participants in the methods section is necessary.
   Response: we have added more detailed description about the computer program in the text. See page7, line3-16.

5. Clear distinction of "households" and "participants" need to be made.
Response: we have rephrased the sampling method to distinct the “households” and “participants”. See page6,line21-22.

6. Not clear how the 90 trained interviewers were distributed (any gender variations?)
Response: Ninety trainees passed the final test and were selected as interviewers, forty-one of them are male, and forty-nine of them are female.

7. No further information on how a ‘faith-based anti-smoking intervention’ was available in the discussion piece.
Response: we have given further information on faith-based tobacco control interventions that successful modeled by WHO and U.S., see page 14,line12-15.

8. A flow chart of the sampling procedure could be useful
Response: we described the sampling procedure in text instead of chart because there too much tables and figures in this paper.

9. Consistent use of ”inverse” relationship is not very helpful for the readers- beta coefficients need to be interpreted both in results and in discussion instead for a better quantification of the magnitude of the effect estimates.
Response: we have rephrased the term “inverse” both in results section and discussion section. And add beta coefficients value and the explanations. See page12,line26-28, and page 12,line 7-12.

#2 Reviewer’s comments

1. Do the tables/figures appear to be genuine?
Yes, but figure 1 is not well explained in the text (page 11, 4-8) and the percentages in the text do not correlate with the figure (50.0 % current smokers among Non-Muslim males in those who attend religious activities once/week or more). Does the figure represent total numbers (male and female)? This is not clear.
Response: Yes, the text do not correlate with the figure, we made a big mistake while developing the figure. Those numbers now had corrected, and we have added more information about the figure 1, the figure presents male numbers as showed in the revised figure. See figure 1. and Page11,line 22-28.

2. Are the discussion and conclusions well balanced?
Yes, discussion and conclusions are in general well balanced and supported by the presented data. Only findings in figure 1 are not adequately discussed. Why does the percentage of smokers in Non-Muslims go up in those who attend religious activities 2-3/month. Is there an explanation for this? Why is there no difference in the percentage of current smokers between Muslims and Non-Muslims in the high religious group (attending religious activities once/week or more)?
Response: The percentage of smokers in Non-Muslims go up in those who attend
religious activities <1/month. Most participants in the Non-Muslims who are never
attend religious activities (290/545), for those who attend <1/month had a small
sample size (32/50), we supposed that the random error contribute to the go up
percentage of smokers in those attend religious activities <1/month.