Author’s response to reviews

Title: The association between exposure to secondhand smoke and psychological symptoms among Chinese children

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Author’s response to reviews:

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Responses to Editors’ and Reviewers’ comments

Comments to the Author:

Your manuscript “Exposure to secondhand smoke and mental health among Chinese children” (PUBH-D-18-03245R1) has been assessed by our reviewers. They have raised a number of points which we believe would improve the manuscript and may allow a revised version to be published in BMC public health.

Editor comments:

1. In the section ‘Funding’, please also describe the role of the funding bodies in the design of the study and collection, analysis and interpretation of data and in writing the manuscript.

Corrected. The funders had no role in the study design, data collection, analysis or interpretation of data, decision to publish, or preparation of the manuscript.

We have added this text to the funding section. [Funding section, Line 325-327, Page 18-19]
2. Please represent authors’ names using their full initials, not their full name, in the Authors’ contributions section.

Corrected

3. We note that some abbreviations used in the manuscript text have not been included in the ‘List of Abbreviation’ section. Please ensure that all abbreviations present in the text are listed in this section.

Corrected

4. Please write out SHS in full during the first use in the abstract

Corrected

5. In the “Authors contributions” section please refer to the authors using initials only (e.g. HW, YTZ, FL etc)

Corrected

6. Please spell out the full first name of every author in the author’s list

Corrected

Reviewer #1 comments:

General comments

This manuscript describes associations between parental reports of secondhand smoke exposure and psychological symptoms in a sample of Chinese children. A significant strength of this study is the large, representative sample of children in China. The manuscript is generally well-organized and fairly well written (although a careful reading by someone who is a native speaker of English would help with some awkward wording). There are, however, numerous inconsistencies in the presentation of these findings as well as points that need clarification throughout this manuscript. Furthermore, a significant limitation of this study is the failure to assess prenatal exposure to tobacco. Children who were prenatally exposed to tobacco have higher rates of secondhand smoke exposure but the mechanism by which prenatal exposure
impacts psychological functioning is very different than the way that SHS would impact those same symptoms. Thus, it is impossible to say that the findings in this study are due to SHS and not prenatal exposure. Although the authors briefly acknowledge this limitation, they brush it aside by indicating that they did consider confounders. Unfortunately, those confounders did not include this significant confound. Although I understand that the authors do not have data on prenatal exposure, they should more thoughtfully discuss this limitation and the impact that it has on the conclusions that can be drawn in this study.

Authors’ response: Thanks indeed for your valuable comments and suggestions for improving our paper. We agree completely that exposure to SHS especially during pregnancy may have detrimental effects on emotional and behavioral problems among children.[1, 2] Unfortunately, we did not have this vital variable information. In view of your concern, we have now added more information about this limitation in the Discussion to clarify this point, as given below:

In the Discussion,

Lastly, the association between SHS exposure and worse psychological outcomes among children could be explained in part by prenatal exposure to tobacco.[1, 2] Unfortunately, the lack of prenatal tobacco exposure measurement prevented us from examining this impact. For maternal SHS from home sources, due to the fact that smoking behavior is often difficult to change, children who are exposed to SHS at home are likely to have been exposed to SHS in utero. In these cases, due to the high correlation between in utero and postnatal exposure to SHS, the observed association remains true.[1] In cases where maternal SHS exposure was from workplace, fetal exposure may have occurred but the child may not be exposed to SHS. Taking all these scenarios into account, the association between child SHS exposure and mental health in our study is likely to be underestimated. Nonetheless, our study is unable to distinguish between fetal and child exposures to SHS. [Discussion section, Line 255-268, Page 14-15]

1) The title needs to be rewritten. It currently reads as if the children were exposed to mental health

Authors’ response: The title has been amended as followed “The association between exposure to secondhand smoke and psychological symptoms among Chinese children”. [Title page, Line 2-3, Page 1]

2) The phrase “general mental health” is used throughout the manuscript and is awkward. I would suggest using a term like “level of mental health (or psychological) symptoms”
Authors’ response: Thank you for the suggestion. We have changed the mental health to psychological symptoms throughout the manuscript as shown in the track change version.

3) Related to this last point, it is confusing and misleading to include prosocial behaviors with the other subscales. The other domains include more dysfunctional behaviors. It appears that higher scores on the prosocial domain indicate more prosocial behaviors which is positive. If that is indeed how this domain should be interpreted, the findings of this domain should be discussed differently than the other domains. Furthermore, there should be some discussion in the discussion section about this domain. Why is higher SHS associated with more prosocial behaviors? If the higher scores on the prosocial domain indicate fewer prosocial behaviors, this interpretation of those scores needs to be clarified throughout the manuscript.

Authors’ response: Thanks for your comments. For SDQ total and subscales (emotional symptoms, conduct problems, hyperactivity-inattention and peer problems), a higher score is indicative of more problems, whereas higher prosocial behavior scores indicate lower difficulties. The prosocial behaviors domain assesses resources rather than problems. For this domain, the reverse coding was applied.[3] In our study, we have found that higher SHS exposure was associated with less prosocial behaviors. To make this point clear, we have added a description on the coding system for the prosocial behaviors subscale. [Method section, Line 131-132, Page 8]

4) In the analysis, the authors indicate that they adjusted for sex but then they conduct analyses to examine sex difference. This does not make sense.

Authors’ response: Thanks for your comments. In this study, we also want to explore whether the association varies by sex. However, the association of SHS exposure and psychological symptoms among children did not vary by sex (p values for interaction terms were all >0.05), thus we did not stratify but we did adjust for sex in the models.

5) Care should be taken out to use gender and sex interchangeably. They have different meanings.

Authors’ response: Thanks for catching this. We have corrected this. [Result section, Line 198, Page 12]

6) The term mental disorder is used in the manuscript when I think the authors mean mental health. In the discussion, the authors also use the term “mentally troubled”. Since it includes
prosocial behaviors, this is misleading. Furthermore, it feels somewhat judgmental to use this term. For example, not everyone would agree that ADHD symptoms are synonymous with being mentally troubled.

Authors’ response: Thanks for your comments. We agree the term ‘mentally troubled’ is a little arbitrary and is not appropriate. As discussed above, we have changed the description to psychological symptoms throughout the manuscript.

7) In the introduction, the authors briefly review some animal studies but do not indicate how the brain areas associated with SHS are tied to mental health.

Authors’ response: Thanks for your comments. We have added more description in the Introduction as below:

These brain areas are consistently demonstrated to be involved in emotional and behavioral regulation.[4] [Introduction section, Line 75-77, Page 6]

8) Were there other anthropometric measures (as the wording at the bottom of p.6 suggests)? If so, why were they not included in these analyses?

Authors’ response: In the SCHEDULE study, anthropometric measures included weight, height and waist circumference. In our analysis, we have adjusted for body mass index in children.

9) The description of the SDQ says that it assesses the 5 most important domains of psychiatric problems. I am not sure that everyone would with this. Furthermore, as mentioned above, prosocial behavior is not a psychiatric problem.

Authors’ response: Sorry for the imprecise statement. The SDQ was designed to assess emotional and behavioral difficulties and strengths of children and adolescents with the following dimensions: emotional symptoms, conduct problems, hyperactivity-inattention, peer relationship problems and prosocial behavior.[3] As discussed above, we have rewritten relevant sentences and changed these items to psychological symptoms throughout the manuscript.

10) The term “mode of delivery” is often used without clarification. Developmentalist, who will likely have interest in an article like this, will interpret this as mode of delivery at birth rather than mode of delivery of SHS. Changing this phrase to “mode of SHS delivery” would eliminate any possible confusion.
Authors’ response: Thanks for your comments. In the manuscript, by mode of delivery we mean the type of childbirth including vaginal delivery and caesarean delivery. To make it clear, we have changed this term to mode of birth throughout the manuscript.

11) Similarly, place of birth was assessed (by province in China?) but the reasons for assessing this were never discussed. Do these geographic differences indicate different cultures, ways of living, etc.? More clarification of this measure and how to interpret it is needed so that readers understand the findings related to place of birth. In addition, why was place of birth used rather than the location of the school? Is there some reason to believe that birthplace is more important than where the child is currently living?

Authors’ response: Thanks for your comments. In China, places of birth could reflect the degree of child growth environment.[5] Shanghai is a more developed economy and has been the center of finance in China. Based on the information collected in this study, place of birth was categorized as Shanghai born and non-Shanghai born (the other provinces in China). Non-Shanghai born children might have a lower standard of living than children born in Shanghai which might also influence the associations of SHS exposure and psychological symptoms.[6] In our study, all participants are living in Shanghai now. Moreover, when we have additionally adjusted for the location of the school, results remain similar as shown below.

Adjusted associations of secondhand smoke exposure with psychological symptoms among children in the SCHEDULE study in China*

<table>
<thead>
<tr>
<th>Psychological symptoms</th>
<th>Secondhand smoke exposure</th>
<th>Reference</th>
<th>OR (95% CI)</th>
<th>1-2 hours/daily OR (95% CI)</th>
<th>≥3 hours/daily OR (95% CI)</th>
</tr>
</thead>
<tbody>
<tr>
<td>General psychological problem</td>
<td>None</td>
<td>1</td>
<td>1.49 (1.21 to 1.84)</td>
<td>2.17 (1.66 to 2.82)</td>
<td>2.34 (1.71 to 3.20)</td>
</tr>
<tr>
<td></td>
<td>&lt;1 hour/daily</td>
<td></td>
<td></td>
<td>1.88 (1.39 to 2.53)</td>
<td>1.98 (1.38 to 2.84)</td>
</tr>
<tr>
<td></td>
<td>1-2 hours/daily</td>
<td></td>
<td></td>
<td>1.69 (1.26 to 2.25)</td>
<td>2.09 (1.50 to 2.92)</td>
</tr>
<tr>
<td>Emotional symptoms</td>
<td></td>
<td></td>
<td></td>
<td>1.44 (1.17 to 1.77)</td>
<td>2.08 (1.63 to 2.65)</td>
</tr>
<tr>
<td>Conduct problems</td>
<td></td>
<td></td>
<td></td>
<td>1.41 (1.23 to 1.63)</td>
<td>1.36 (1.14 to 1.61)</td>
</tr>
<tr>
<td>Hyperactivity-inattention</td>
<td></td>
<td></td>
<td></td>
<td>1.33 (0.98 to 1.80)</td>
<td>1.61 (1.12 to 2.30)</td>
</tr>
<tr>
<td>Peer relationship problems</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Prosocial behaviors</td>
<td></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
</tbody>
</table>
12) The selection of confounders was clearly explained. However, it appears that other covariates were included in some analyses? Why? How were these selected? Why were they considered separately from confounders?

Authors’ response: Thanks for your comments. Based on a previous literature review, these covariates were selected as potential confounders of the association between SHS exposure and psychological symptoms. To illustrate possible confounding and remove the effect of these potential confounders, we present the results of two models. After having adjusted for sex and age in model 1, we additionally adjusted for parents’ education, household income, mode of delivery, body mass index z-score in model 2, even though the results remained unchanged.

13) How many children/parents were invited to participate but decided to consent? What are the implications of this?

Authors’ response: In the SCHEDULE study, we invited 17,624 participants and 17,571 of them have consented to join in the survey. Response rate refers to the number of participants who consented and completed the survey divided by the number of participants who make up the total group. The high response rate indicates the good representativeness of our study population.

14) The possibility that there is shared genetic variance between smoking and mental health should be discussed. In other words, children with SHS exposure may have more psychological symptoms because of shared genetics with the parents and not because of the SHS. I think that the authors may be trying to discuss this on p.14, but it is not a clear explanation of this idea of shared genetics. In addition, they talk about maternal exposure to SHS which was not measured in this study.

Authors’ response: Apologies for being unclear here. The association between SHS exposure and psychological problems could be partially explained by passive genotype-environment correlation. For example, mothers who pass down the generic variants for psychological symptoms to their children might also have an increased risk of being exposed to SHS, SHS exposure could more likely to be considered as genetic risk that parents transmit to children rather than a risk factor for children’s psychological symptoms.[7] In addition, the lack of prenatal exposure to SHS should be one limitation in our study. We have added these texts in the Discussion. [Discussion section, Line 283-286, Page 16]
Reviewer #2 comments:

General comments

Generally, a well-written manuscript that addresses an important public health topic. A genuine pleasure to read. Methodology appears appropriate and statistics appear relatively robust. Along with the large sample size, the results are probably valid and reliable. Results are very well presented.

Requested revisions

Introduction

nice and concise. Summarizes literature well and provides some conceptual thinking to the relationship between SHS and mental health in youth. No specific comments here.

Methods

Very appropriate for a cross-sectional study. Large sample size and good sampling method. My only comment is whether the measure for SHS exposure was taken from the literature, or was it generated by the authors/study designers?

Authors’ response: Thanks very much for your comments. The frequency of SHS exposure was categorized into four levels: not exposed, <1 hour/day, 1-2 hours/day and ≥3 hours/day, which was taken from the literature and used successfully in other studies.[8, 9]

Results

how was the response rate calculated? This is very high and appears to be at the student-level? With multi-stage sampling one needs to multiple the student response rate by the school rate to achieve the total response rate.

Authors’ response: In the SCHEDULE study, a multistage cluster sampling approach had been employed. At the school level, we have randomly selected 26 public primary school and all of them were willing to participate in the survey. Finally, we invited 17,624 participants and 17,571 of them consented to join the survey. The response rate refers to the number of participants who consented and completed the survey divided by the number of participants who made up the total group. The high response rate indicates the good representativeness of our study population.
Discussion

Very insightful and well-written. The authors have identified the limitations to their study and explained how and why they may affect their results.

Perhaps a paragraph on the research and/or policy implications would be suitable given the robustness of the results and the implications for tobacco control and mental health?

Authors’ response: Thanks indeed for your comments and suggestion. We have added information on policy in the Conclusion and Policy Implications as follows:

Smoke free police is the most effective way to reduce SHS exposure at public and private places. Our findings can be used to inform future SHS control polices and reinforce the need for public education campaigns. These campaigns could be implemented to raise the public awareness of the harmful psychological effect of SHS exposure. [Conclusion and Policy Implications section, Line 298-306, Page 16-17]

Additional requests/suggestions:

The authors should ensure the paper is proof read and corrected by a native English academic. There are several grammatical errors and spelling mistakes throughout the manuscript.

Authors’ response: Thanks for your comments. As suggested, we have revised our manuscript with the help of a native English academic.

References


