Reviewer’s report

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

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Reviewer: Pamela Schuetze

Reviewer's report:

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 second-hand 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 confounds. Unfortunately, those confounds 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.

Other comments/issues are itemized below.

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

* 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".

* 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.

* In the analyses, the authors indicate that they adjusted for sex but then they conduct analyses to examine sex differences. This does not make sense.

* Care should be taken not to use gender and sex interchangeably. These have different meanings.

* 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.

* 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.

* 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?

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

* The term "mode of delivery" is often used without clarification. Developmentalists, 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.

* 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?

* The selection of confounds 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?

* How many children/parents were invited to participate but declined to consent? What are the implications of this?
* 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.

**Are the methods appropriate and well described?**
If not, please specify what is required in your comments to the authors.

Yes

**Does the work include the necessary controls?**
If not, please specify which controls are required in your comments to the authors.

Yes

**Are the conclusions drawn adequately supported by the data shown?**
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No

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If an additional statistical review is recommended, please specify what aspects require further assessment in your comments to the editors.

I am able to assess the statistics

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