

PEER REVIEW HISTORY

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ARTICLE DETAILS

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| TITLE (PROVISIONAL) | Prevalence of passive smoking in the community population aged 15 and older in China: a systematic review and meta-analysis |
| AUTHORS | Zeng, Jing; Yang, Shanshan; Wu, Lei; Wang, Jianhua; Wang, Yiyang; Liu, Miao; Zhang, Di; Jiang, Bin; He, Yao |

VERSION 1 - REVIEW

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| REVIEWER | David Lawrence Telethon Kids Institute, The University of Western Australia, Australia |
| REVIEW RETURNED | 28-Sep-2015 |

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| GENERAL COMMENTS | <p>This paper reports a meta-analysis of the prevalence of passive smoking in China. The authors identified almost 50 studies covering almost 400,000 smokers included in the meta-analysis. Based on the literature cited, it appears as if there is sufficient literature covering a sufficiently large sample to conduct this meta-analysis. I have some suggestions on the presentation of the results.</p> <p>I appreciate the author's main language may not be English. I found some parts of the manuscript unclear, and it may just be language issues have led me to misunderstand the methodology that has been employed in this study. Professional copy editing of the manuscript may be helpful.</p> <p>I was unclear how the studies have been weighted together. The manuscript states that crude data was used to pool the overall prevalence estimates, which I would assume to mean pooling the numerators and denominators across studies. But then I don't understand what the weight column in figure 1 represents. The weights do not seem to vary do not seem to vary in proportion to the sample size or the confidence intervals.</p> <p>I think it would be helpful if the studies were listed in the same order in both Table 1 and figure 1. The data in table 1 seem to suggest that there were some studies with order of magnitude greater sample, which one would expect to make a major contribution to the overall meta-analysis results, but I'm not sure this is the case based on inspection of figure 1.</p> <p>I would like to have seen more done to assess the quality of the various studies contributing to the meta-analysis. The authors report evidence of considerable heterogeneity across the studies, which they note is common in meta-analysis of observational studies. Some reflection on whether methodological differences between the studies could contribute to this heterogeneity would be helpful. For instance, have all studies used the same definition of passive</p> |
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| | <p>smoking? Was consideration given to giving each study a quality rating?</p> <p>The analysis of time trends in passive smoking does not seem to be particularly strong. The data from figure 2 suggest the time trend is mainly driven by changes between prevalence at the first and second time points. However, it seems to me that very few of the studies contribute to the estimates for the first time period, and I would suspect this point would have considerable variability.</p> <p>For context, it would be helpful to know if the overall smoking rate has changed in China over the study period.</p> <p>I'm not convinced all of the discussion points are supported by the data. For instance, the suggestion that greater importance should be attached to male non-smokers - the difference between males and females does not seem to be that large, and I would think both would be important. Similarly passive smoking is high in both urban and rural areas - it is not clear to me that the data support the recommendation to continue to focus on urban populations.</p> |
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| REVIEWER | Zhi-Jun Li Department of Epidemiology and Statistical, School of Public Health, Jilin University |
| REVIEW RETURNED | 11-Dec-2015 |

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| GENERAL COMMENTS | <p>Comments and suggestions for the manuscript are as follows:</p> <ol style="list-style-type: none"> 1. Abstract: Pages3, Lines 26-46, authors should give the heterogeneity of meta-analysis. 2. Introduction: authors should analysis the status of passive smoking in China, and address the meaning and importance of this meta-analysis. 3. Methods: Pages6, Lines 6-14, authors had performed the MOOSE guidelines and PRISMA, but why you did not evaluate the quality of eligible studies? Pages8, Lines 24-29, why authors performed meta-analysis by Stata and Review Manager? we all know that anyone of them can do all that analysis, why using both of them? Cai L, et al(2014) and Cai L, et al(2012), whether the former study included the later population? According your inclusion criteria, why the study by Yao T, et al(2012) did not included in the meta-analysis? Eligible studies were not incomplete and should be updated in English. 4. Heterogeneity: there were heterogeneity (I²>90%) in the pooled analysis, subgroup analysis, and sensitivity analysis, what caused the heterogeneity and there was not any relevant explanation, so how to say that the meta-analysis was stable? Eligible studies were in Chinese and English, most in Chinese, so if authors consider that languages should be stratification analysis for heterogeneity. 5. Publication bias: Pages10, Lines 51-57, However, the estimated public bias indicated that more studies were necessary to accurately pool the prevalence of passive smoking among males (Egger's test, p=0.002). If the statically significant publication bias was found, the 'trim and fill' method was used to adjust for it, why you did not perform? Pages12, Lines20-23, the assessment of publication bias should be addressed at here, if the statically significant publication bias was found, the 'trim and fill' method was used to adjust for it, and |
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| | <p>whether alter the conclusion?</p> <p>6. Discussion: Page14, Lines 14-21, 'Additionally,, suggests that more attention should be paid to tobacco exposure in the labour force population', how can you get this? And, should the authors further discuss the meaning and importance of the meta-analysis to public health, could the authors also discuss the recent efforts by China government on tobacco control in public and work places?</p> <p>7. Tables and Figures: Left align the left column and indent sub-categories to make it easier to read. Figures legends should be marked under the photos.</p> <p>8. The article should be revised and needs substantial improvement before considering a re-submission.</p> |
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VERSION 1 – AUTHOR RESPONSE

Reviewer: 1 (7 points)

Reviewer Name: David Lawrence

Institution and Country: Telethon Kids Institute, The University of Western Australia, Australia

1. I appreciate the author's main language may not be English. I found some parts of the manuscript unclear, and it may just be language issues have led me to misunderstand the methodology that has been employed in this study. Professional copy editing of the manuscript may be helpful.

Re: Thank you for your suggestion. We have sent the revised manuscript to a professional copyediting service.

2. I was unclear how the studies have been weighted together. The manuscript states that crude data was used to pool the overall prevalence estimates, which I would assume to mean pooling the numerators and denominators across studies. But then I don't understand what the weight column in figure 1 represents. The weights do not seem to vary in proportion to the sample size or the confidence intervals.

Re: If we used the fixed model with inverse variance (I-V) method, we would obtain another forest plot (Figure 1.1), whose weights varied in proportion to the sample size. Due to the high heterogeneity among studies ($I^2 > 75\%$), the random effects model with the DerSimonian and Laird (D-L) method was used for the meta-analysis. This model assumes that the treatment effects observed in the trials are a random sample from the distribution of treatment effects with a variance τ^2 . This is in contrast to the fixed effect model, which assumes that the observed treatment effects are all estimates of a single treatment effect. The D-L method incorporates an estimate of the between-study variation τ^2 into both the study weights (which are the inverse of the sum of the individual sampling variances and the between-studies variance τ^2) and the standard error of the estimate of the common effect.

Where there is excess variability (heterogeneity) among study results, random effect models typically produce more conservative estimates of the significance of the treatment effect (i.e., a wider confidence interval) than do fixed effect models. As they give proportionately higher weights to smaller studies and lower weights to larger studies than do fixed effect analyses, random models may show differences from fixed models in the estimate of the treatment effect.

Figure 1.1 Forest plot generated using the fixed model with the I-V method

References:

1. Jonathan A. C. Sterne. Meta-analysis in Stata: an undated collection from the Stata journal. USA: Stata Press 2009:6.

2. Cochrane handbook for systematic reviews of intervention version 5.1.0, updated march 2011.

3. I think it would be helpful if the studies were listed in the same order in both Table 1 and figure 1. The data in table 1 seem to suggest that there were some studies with order of magnitude greater

sample, which one would expect to make a major contribution to the overall meta-analysis results, but I'm not sure this is the case based on inspection of figure 1.

Re: The reviewer's comments are very helpful. As a large number of studies (46) are included, the overall forest plot was difficult to read. We have created a new Figure 1 as suggested and used '(a)' or '(b)' to distinguish cases of the same last name of an author in the same year.

4. I would like to have seen more done to assess the quality of the various studies contributing to the meta-analysis. The authors report evidence of considerable heterogeneity across the studies, which they note is common in meta-analysis of observational studies. Some reflection on whether methodological differences between the studies could contribute to this heterogeneity would be helpful. For instance, have all studies used the same definition of passive smoking? Was consideration given to giving each study a quality rating?

Re: Thank you for the helpful comments. As one of our included criteria, all studies defined passive smoking as a non-smoker being exposed to another person's tobacco smoke for at least 15 minutes daily for more than 1 day per week. In addition, we have added information on the quality assessment to the Methods section as follows:

The Agency for Healthcare Research and Quality (ARHQ) methodology checklist, STROBE statement²² and Patricia L. Loney's methodological scoring system²³ were integrated into nine items used to evaluate the methodological quality of the studies. Quality scoring for studies was not performed because it was not possible to weigh different items. However, the tool suggested by the Cochrane Collaboration was used to present the overall quality of the included studies. Each item was assessed as 'low risk', 'unclear risk' or 'high risk'. The evaluation items were as follows:

- Setting: State the research sites, times and places of data collection.
- Selection: Enrol a random sample or a whole population.
- Sample: Use an adequate sample size (>300 subjects).
- Criteria: List inclusion and exclusion criteria for test and control subjects or refer to previous publications.
- Variables: List all outcomes, exposures and potential confounders.
- Measurements: Define each variable of interest or provide details of the methods of assessment.
- Bias: Describe how confounding was assessed and/or controlled.
- Response and completeness: Summarize response rates and completeness of data collection.
- Characteristics: Describe the characteristics of the study subjects.

5. The analysis of time trends in passive smoking does not seem to be particularly strong. The data from figure 2 suggest the time trend is mainly driven by changes between prevalence at the first and second time points. However, it seems to me that very few of the studies contribute to the estimates for the first time period, and I would suspect this point would have considerable variability.

Re: The reviewer's comment is reasonable. Studies in the first time period (1995-1997) were inadequate, and no studies from 1998 to 2001 were eligible for inclusion. We moved Table 2 to Supplement Table S1 to present all analyses of the 46 eligible studies, but did not include the three studies conducted between 1995 and 1997 in the trends analysis to show the relatively continuous and long-term trends in the prevalence of passive smoking. A new Figure 3 was created, and the rationale was added to the statistics analysis in the Methods section.

6. For context, it would be helpful to know if the overall smoking rate has changed in China over the study period.

Re: The reviewer's comment is very helpful. Significant efforts towards controlling second-hand smoke in public places have recently been made in China. There are currently no results from relevant national surveys conducted after 2013 about passive smoking. However, there are some studies on the effect of tobacco control in China. A multi-stage sampling study conducted in 21 Chinese cities with 16,866 urban residents showed that the reported smoking prevalence in smoke-

free public places was 41.2%, which was similar to our pooled result (46.5%, 95%CI (31.9-61.0)). More information on this has been added to the Introduction sections.

7. I'm not convinced all of the discussion points are supported by the data. For instance, the suggestion that greater importance should be attached to male non-smokers - the difference between males and females does not seem to be that large, and I would think both would be important. Similarly passive smoking is high in both urban and rural areas - it is not clear to me that the data support the recommendation to continue to focus on urban populations.

Re: Thank you for the helpful comments. We reconsidered the discussion as suggested. The evidence was insufficient to support our conclusions. We have added new information to the Discussion section as follows:

This result may be valuable from a public health standpoint as it suggests that although we are always concerned with tobacco exposure of females in China, attention should also be given to male non-smokers, who have a greater likelihood of passive smoking in the workplace and in public areas.

Third, passive smoking was measured by self-reporting in all eligible studies. The much greater health consciousness in urban areas could have led to more self-reports of passive smoking⁶³, and the prevalence may have been underestimated in rural areas. With the trend of urbanization and the massive annual migration to urban areas in China for jobs, tobacco control policies should focus on both populations.

Reviewer: 2 (8 points)

Reviewer Name: Zhi-Jun Li

Institution and Country: Department of Epidemiology and Statistical, School of Public Health, Jilin University

Comments and suggestions for the manuscript are as follows:

1. Abstract: Pages3, Lines 26-46, authors should give the heterogeneity of meta-analysis.

Re: Thank you for the comments. We have added information on heterogeneity as suggested.

2. Introduction: authors should analysis the status of passive smoking in China, and address the meaning and importance of this meta-analysis.

Re: Thank you for the helpful comments. We have added this information to the Introduction as suggested.

3. Methods:

1) Pages6, Lines 6-14, authors had performed the MOOSE guidelines and PRISMA, but why you did not evaluate the quality of eligible studies?

Re: We have added information on the quality assessment to the Methods section as follows:

The Agency for Healthcare Research and Quality (ARHQ) methodology checklist, STROBE statement²² and Patricia L. Loney's methodological scoring system²³ were integrated into nine items used to evaluate the methodological quality of the studies. Quality scoring for studies was not performed because it was not possible to weigh different items. However, the tool suggested by the Cochrane Collaboration was used to present the overall quality of the included studies. Each item was assessed as 'low risk', 'unclear risk' or 'high risk'. The evaluation items were as follows:

- Setting: State the research sites, times and places of data collection.
- Selection: Enrol a random sample or a whole population.
- Sample: Use an adequate sample size (>300 subjects).
- Criteria: List inclusion and exclusion criteria for test and control subjects or refer to previous publications.
- Variables: List all outcomes, exposures and potential confounders.

- Measurements: Define each variable of interest or provide details of the methods of assessment.
- Bias: Describe how confounding was assessed and/or controlled.
- Response and completeness: Summarize response rates and completeness of data collection.
- Characteristics: Describe the characteristics of the study subjects.

2) Pages 8, Lines 24-29, why authors performed meta-analysis by Stata and Review Manager? We all know that anyone of them can do all that analysis, why using both of them?

Re: Thank you for the comments. We used Stata to perform all meta-analysis.

3) Cai L, et al(2014) and Cai L, et al(2012), whether the former study included the later population? According your inclusion criteria, why the study by Yao T, et al(2012) did not included in the meta-analysis? Eligible studies were not incomplete and should be updated in English.

Re: We have thought about the reviewer's comment carefully and offer the following explanation. We had reviewed the studies of Cai L, et al (2014) and Cai L, et al (2012), which had different sampling methods and extracted data. The sample was much larger in Cai L, et al (2014) than in Cai L, et al (2012). There was insufficient evidence to consider them as one study, and therefore, we included both studies in our meta-analysis.

We did not include the study of Yao T, et al (2012) because it used a different definition of passive smoking. Yao T, et al (2012) defined passive smoke exposure as a person being exposed to SHS at home if they lived with at least one current smoker, while our study defined SHS as a non-smoker being exposed to another person's tobacco smoke for at least 15 minutes daily for more than 1 day per week.

4. Heterogeneity:

1) There were heterogeneity ($I^2 > 90\%$) in the pooled analysis, subgroup analysis, and sensitivity analysis, what caused the heterogeneity and there was not any relevant explanation, so how to say that the meta-analysis was stable?

Re: Thank you for the helpful comments. The previous explanations of heterogeneity in the section might be insufficient and not obvious. We have added a more comprehensive explanation and limitations in the Discussion section. Details are as follows:

There are some limitations in this meta-analysis. First, the heterogeneity between studies was substantial despite the strict inclusion and exclusion criteria. Subgroup, trend, and sensitivity analyses were performed to explore the high heterogeneity but with no conclusive results. Therefore, the more conservative random effects meta-analysis model was used. The high heterogeneity might have been due to the confounding effects of the variations in geographical distribution of the eligible studies, and these could not be extracted based on characteristics such as age in different genders, education level, ethnicity, and passive source because many of the included studies reported passive smoking as an additional outcome.

2) Eligible studies were in Chinese and English, most in Chinese, so if authors consider that languages should be stratification analysis for heterogeneity.

Re: Thank you for the comments. As we all know, non-national cross-sectional studies that describe the prevalence of disease are not easy to publish in English journals, regardless of their quality. Our meta-analysis explored the epidemiology and distribution of passive smoking in China; therefore, it was inevitable that we included more cross-sectional studies (44 of 46 studies) written in Chinese. In addition, there were six nationwide studies in Chinese included in our analysis. Therefore, we did not add a subgroup analysis of language.

5. Publication bias: Pages 10, Lines 51-57, However, the estimated public bias indicated that more studies were necessary to accurately pool the prevalence of passive smoking among males (Egger's test, $p=0.002$). If the statically significant publication bias was found, the 'trim and fill' method was used to adjust for it, why you did not perform? Pages 12, Lines 20-23, the assessment of publication bias should be addressed at here, if the statically significant publication bias was found, the 'trim and

fill' method was used to adjust for it, and whether alter the conclusion?

Re: The reviewer's comment is very helpful. We used the 'trim and fill' method to adjust for publication bias. The results showed that no trimming was performed, and data were unchanged. Specifically, the pooled prevalence of males was moderate despite of the existing publication bias (Egger's test, $p=0.002$). We added relevant information on 'trim and fill' to the Methods and Results section, as follows:

Publication bias was evaluated by Egger's test. If bias existed, the 'trim and fill' method was used to adjust for the publication bias.

The results of the 'trim and fill' method indicated that the pooled prevalence of males was moderate despite the existent publication bias (Egger's test, $p=0.002$) (Figure S2).

Figure S2 'Trim and fill' method

6. Discussion:

1) Page14, Lines 14-21, 'Additionally,, suggests that more attention should be paid to tobacco exposure in the labour force population', how can you get this?

Re: Thank you for the helpful comments. We reconsidered the inappropriate expression and revised it as 'young and middle-aged populations'.

2) And, should the authors further discuss the meaning and importance of the meta-analysis to public health, could the authors also discuss the recent efforts by China government on tobacco control in public and work places?

Re: Thank you for the helpful comments. We added information to the Introduction and Conclusion section as suggested.

7. Tables and Figures:

1) Left align the left column and indent sub-categories to make it easier to read.

2) Figures legends should be marked under the photos.

Re: Thank you for the comments. We have made adjustments as suggested.

8. The article should be revised and needs substantial improvement before considering a re-submission.

Re: Thank you for all comments. We have made many improvements as suggested.

VERSION 2 – REVIEW

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| REVIEWER | David Lawrence Telethon Kids Institute, The University of Western Australia Australia |
| REVIEW RETURNED | 03-Feb-2016 |

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| GENERAL COMMENTS | <p>Thank you for the opportunity to review the revision of this paper.</p> <p>I think the authors have addressed all of the concerns raised by the reviewers. The paper is now much easier to read. I thank the authors for clarifying the methods that they used in their analysis.</p> <p>Only a couple of typographical errors.</p> <p>p. 13 l. 5 "publication bias" instead of "public bias" p. 14 l. 6 "significantly alter" instead of "significant alter"</p> |
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VERSION 2 – AUTHOR RESPONSE

Reviewer Name: David Lawrence

1. Only a couple of typographical errors.

p. 13 l. 5 "publication bias" instead of "public bias"

p. 14 l. 6 "significantly alter" instead of "significant alter"

Re: Thank you for your comments. We have revised these errors as suggested and checked the manuscript again.