

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Association between Passive Smoking and Mental Distress in Adult Never-smokers: a Cross-sectional Study
AUTHORS	Wang, Rui; Zhang, Peng; Lv, Xin; Gao, Chunshi; Song, Yuanyuan; Li, Zhijun; Yu, Yaqin; Li, Bo

VERSION 1 - REVIEW

REVIEWER	Ivy Shiue Northumbria University, UK University of Edinburgh, UK University of Georgia, USA
REVIEW RETURNED	29-Feb-2016

GENERAL COMMENTS	"Many studies have suggested that passive smoking is a risk factor for various somatic diseases, but few studies explored the effects of passive smoking on mental distress only conducted with small sample size or specific groups." This does not make sense. Why is a small sample size needed when similar data has been already published?
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REVIEWER	Kyu-Tae Han Department of Public Health, College of Medicine, Yonsei University
REVIEW RETURNED	04-Mar-2016

GENERAL COMMENTS	<p>Background</p> <ol style="list-style-type: none">1. The aim of study was not substantially provided in background section. Therefore, you have to describe background information such as statistics about mental health in China.2. Also, there were needed to review previous studies related to association between passive smoking and mental health. <p>Methods</p> <ol style="list-style-type: none">1. This study is a kind of cross-sectional studies, thus, you have not to interpret the results like longitudinal studies. <p>Results</p> <ol style="list-style-type: none">1. You used other covariates in this study, but there were not showed other results about covariates. Therefore, you have to show the results about covariates in all tables.2. In addition, there were needed to test multicollinearity between each variable. <p>Discussion</p> <ol style="list-style-type: none">1. There is needed to suggest the alternatives based on results in
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	<p>this study. Please explain these. 2. Above all, you have to suggest interesting findings about between mental health and passive smoking.</p>
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REVIEWER	Mariska Bot VU University Medical Center, The Netherlands
REVIEW RETURNED	05-Apr-2016

GENERAL COMMENTS	<p>This paper describes the association between passive smoking and mental distress, both assessed with a face-to-face interview, in adults of northeast China. A multistage stratified random cluster sampling was used to get a representative sample. Although the issue of the association between passive smoking and mental health/depression has been studied in several samples, this study has a very large sample size compared to previous studies. A limitation of the study is the use of interview data for passive smoking. It would have been stronger if biological measures were added (e.g. cotinine). Nonetheless, this paper addresses an important public health issue, and the authors did a nice job in studying this issue. However, there are several issues that needs to be addressed.</p> <p>Main:</p> <ol style="list-style-type: none"> 1. Please provide more information on the exact content of the smoking questions that were used in the interview. This is needed for the reader to evaluate the (quality of the) data collection of the passive smoking. 2. This study used face-to-face interviews to obtain information about smoking, passive smoking and mental health. Please describe data on the likelihood that persons gave social desirable answers to these questions. I can imagine that in an interview setting, persons will give more socially desired answers compared to a questionnaire. Is it possible that this would influence your results? 3. Why is the GHQ categorized into categories low, moderate risk and high risk? Artificially categorizing a continuous score might result in loss of information and loss of power. Please add the results for a continuous GHQ-12 score. 4. Participants are classified into 3 groups based on GHQ scores, but table 1 distinguishes only 2 groups. Please clarify and adapt. 5. Please describe briefly how the confounders were measured. 6. P6. The text suggests that you tested interaction between each covariate * GHQ for all covariates. Could you explain the rationale for doing that for all covariates? How did you deal with multiple testing in this respect? I think it is stronger to only test interaction for the covariates for which you hypothesize an interaction can be expected. If you test many or all, you should take measures to deal with multiple testing. 7. P10. A significant interaction term was observed ($p=0.048$), however, because both GHQ and income were categories, there should be more than one p value for interactions here. 8. In table 2 the results are stratified by gender, in other tables they're not. If there is no statistical interaction, there is probably no reason to stratify for gender in table 2. 9. I think the discussion can be improved at several points. <ul style="list-style-type: none"> - I think the previous findings on this topic needs to be highlighted more. Did they find similar associations (same strength), in what kind of populations, what kind of passive smoking measures were
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	<p>used? Please elaborate on this.</p> <ul style="list-style-type: none"> - Furthermore, in the first paragraph on P12 some explanations are given. If passive smoking is a proxy for stressful living, this would implicate that there is no causal effect by passive smoking (in contrast to the conclusion of the paper). How does the Chinese traditional Confucianism relate to your observations? Please elaborate on these explanations. - The second paragraph on P12 focuses on the relationship between socio-demographics and passive smoking, but this was not the aim of your paper, and thus not really relevant. - P12. Please revise the interpretation of the interaction, because passive smoking is not mentioned here. - P13: why was this re-analysis done? In the statistical methods it appears that chronic diseases were included in the analyses. However, chronic diseases is not shown in the tables as covariate. This is very confusing. Please put all the analyses in the methods and results section. <p>Minor:</p> <p>10. Please check the language throughout the full paper. Here are some examples, but there are more in the text:</p> <ul style="list-style-type: none"> - P2. first sentence abstract. Please rephrase. - P2. "correlation was not close" (close to what)? - P4. "At present, there were" (there are?) - P4. "but the conclusions of these studies are not coincident" (do you mean similar? Or 'do not coincide') - P4. First sentence third paragraph: unclear what is meant here, please rephrase - P6. Multilogistic (multiple logistic?) - P12. Please rephrase: there were some studies have explored the linkage - P12. Please rephrase: couldn't explain the association 'persuasively'. - P12. Healthy harm (health harm?) - P13. Please check first sentence new paragraph <p>11. This paper often use the word 'effect'. However, this is an observational cross-sectional study. It is therefore hard to study (causal) effects, therefore, 'association' or 'relationship' should be used instead.</p> <p>12. I would switch table 3 and 4, because table 4 shows the unadjusted results, whereas table 3 adjusts for potential confounders.</p> <p>13. P2. Abstract: the conclusion that the Chinese government should increase awareness especially among the low-income groups is not justified. This data is not shown in the result section of the abstract.</p> <p>14. P6. Statistical analyses: please remove the word 'univariate' before logistic regression analyses, because multiple confounders were entered.</p> <p>15. P7. Please provide exact p value instead of $p > 0.05$.</p> <p>16. P12. Please define a 'positive lifestyle'.</p>
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VERSION 1 – AUTHOR RESPONSE

Reviewer 1:

Ivy Shiue

Institution and Country

Northumbria University, UK

University of Edinburgh, UK

University of Georgia, USA

The reviewer's comment:

I. The reviewer's question is "Many studies have suggested that passive smoking is a risk factor for various somatic diseases, but few studies explored the effects of passive smoking on mental distress only conducted with small sample size or specific groups." This does not make sense. Why is a small sample size needed when similar data has been already published?

The authors' answer: Thanks to reviewer's comments, this ambiguous sentence in abstract has been revised.

Reviewer 2:

Kyu-Tae Han

Institution and Country

Department of Public Health, College of Medicine, Yonsei University

The reviewer's comment:

I. Background

(1). The aim of study was not substantially provided in background section. Therefore, authors have to describe background information such as statistics about mental health in China.

(2). Also, there were needed to review previous studies related to association between passive smoking and mental health.

The authors' answer: reviewer's comments are very valuable and helpful to our manuscript. We have added statistics about mental health in China and information of association between passive smoking and mental health based on prior studies to background as well as the section of discussion.

II. Method

Reviewer question is: This study is a kind of cross-sectional studies, thus, you have not to interpret the results like longitudinal studies.

The authors' answer: this study is a kind of cross-sectional study, a lack of longitudinal data is a limitation for the study. We have stated this in the limitation part. In addition, to make our study readable, we have studied prior longitudinal studies and made a further discussion subsequently.

III. Result

(1). You used other covariates in this study, but there were not showed other results about covariates. Therefore, you have to show the results about covariates in all tables.

The authors' answer: Thanks to reviewer's comments. Initially, the aim of this re-analysis was to examine the robustness of result after balancing the possible effect of some chronic disease, but the information of these diseases were not complete. After discussion, we removed the confusing statements from the discussion part.

(2). In addition, there were needed to test multicollinearity between each variable.

The authors' answer: thanks to reviewer's professional suggestion, after discussion, we realized that artificially categorizing a continuous score into three groups might result in loss of information and loss of power. Thus we revised this part by adding the result for a continuous GHQ-12 score.

Multicollinearity test between each variable was subsequently conducted. But the result of collinearity diagnostics was not the main subject we planned to present to readers; thus we showed the result in a supplementary document following the main document.

IV. Discussion

(1). There is needed to suggest the alternatives based on results in this study. Please explain these.

(2). Above all, you have to suggest interesting findings about between mental health and passive smoking.

The authors' answer: reviewer's comments are very constructive for our manuscript. This study is the first one to explore the association between SHS exposure and mental health in northern China, we also focused on where people exposed to SHS regularly to provide effective information for government making smoke free policies. We revised the discussion part for a better understanding.

Reviewer 3:

Mariska Bot

Institution and Country

VU University Medical Center, The Netherlands

The reviewer's comment:

Main:

I. Please provide more information on the exact content of the smoking questions that were used in the interview. This is needed for the reader to evaluate the (quality of the) data collection of the passive smoking.

The authors' answer: thanks to reviewer's rigorous comments, we have added specific questions to the section of method.

II. This study used face-to-face interviews to obtain information about smoking, passive smoking and mental health. Please describe data on the likelihood that persons gave socially desirable answers to these questions. I can imagine that in an interview setting, persons will give more socially desired answers compared to a questionnaire. Is it possible that this would influence your results?

The authors' answer: reviewer's comments are rigorous and professional. The situation that persons give more socially desired answers to an interview setting exists in psychological investigations. To control the false negative, some previous studies even excluded participants with low scores. As a result of the low education background (rate of primary school and below: 22%), self-administered questionnaire was not feasible in this survey. The result may be influenced by the false negative, although we have selected private places and provided enough time for the interview. This limitation has been added to the end of discussion part.

III. Why is the GHQ categorized into categories low, moderate risk and high risk? Artificially categorizing a continuous score might result in loss of information and loss of power. Please add the results for a continuous GHQ-12 score.

The authors' answer: reviewer's comments are very constructive for our manuscript. We have revised this part and changed table 4 to a statistical chart; continuous GHQ-12 scores been also added to results.

IV. Participants are classified into 3 groups based on GHQ scores, but table 1 distinguishes only 2 groups. Please clarify and adapt.

The authors' answer: thanks to reviewer's helpful comments, we have revised the part of definition of major variables to make our manuscript more rigorous and comprehensible. Participants were classified into 2 groups: the cut-off score for mental distress was established at GHQ-12 score ≥ 3 .

V. Please describe briefly how the confounders were measured.

The authors' answer: potential confounders were measured by multivariable logistic regression analyses; this has been added to the method part.

VI. P6. The text suggests that you tested interaction between each covariate * GHQ for all covariates. Could you explain the rationale for doing that for all covariates? How did you deal with multiple testing in this respect? I think it is stronger to only test interaction for the covariates for which you hypothesize an interaction can be expected. If you test many or all, you should take measures to deal with multiple testing.

P10. A significant interaction term was observed ($p=0.048$), however, because both GHQ and income were categories, there should be more than one p value for interactions here.

The authors' answer: thanks to reviewer's enlightened suggestion, after discussion, all the authors confirmed that interaction between each covariate * GHQ for all covariates will decrease the accuracy of our result. Thus we removed the confusing results and statements that far from our initial aim.

VII. In table 2 the results are stratified by gender, in other tables they're not. If there is no statistical

interaction, there is probably no reason to stratify for gender in table 2.

The authors' answer: some prior studies suggested a different distribution of where people exposed to SHS regularly between two genders, this study also observed a difference in this aspect. We have supplemented discussion and added related references to our manuscript.

VIII. I think the previous findings on this topic needs to be highlighted more. Did they find similar associations (same strength), in what kind of populations, what kind of passive smoking measures were used? Please elaborate on this.

The authors' answer: reviewer's comments are very valuable and helpful to our manuscript. We have added information of association between passive smoking and mental health that draw from prior studies to the section of discussion.

i Furthermore, in the first paragraph on P12 some explanations are given. If passive smoking is a proxy for stressful living, this would implicate that there is no causal effect by passive smoking (in contrast to the conclusion of the paper). How does the Chinese traditional Confucianism relate to your observations? Please elaborate on these explanations.

The authors' answer: thanks to dear reviewer's valuable suggestion. The inappropriate expression has been revised and further explanation of traditional Confucianism has been added as well.

ii The second paragraph on P12 focuses on the relationship between socio-demographics and passive smoking, but this was not the aim of your paper, and thus not really relevant.

P12. Please revise the interpretation of the interaction, because passive smoking is not mentioned here.

The authors' answer: reviewer's constructive comments made us discuss the major themes of this paper again. After discussion, we revise the confusing statements that far from the subject of this study and revised the emphasis of the discussion part.

iii P13: why was this re-analysis done? In the statistical methods it appears that chronic diseases were included in the analyses. However, chronic diseases is not shown in the tables as covariate. This is very confusing. Please put all the analyses in the methods and results section.

The authors' answer: the aim of this re-analysis was to examine the robustness of result after balancing the possible effect of some chronic disease, but the information of these diseases were not complete. After discussion, we removed the confusing statements from discussion.

Minor:

I. Please check the language throughout the full paper.

The authors' answer: We have checked through the manuscript to improve our English writing, and we are still finding native speakers for help.

II. This paper often use the word 'effect'. However, this is an observational cross-sectional study. It is therefore hard to study (causal) effects, therefore, 'association' or 'relationship' should be used instead.

The authors' answer: thanks to reviewer's professional advice, we have revised the improper use of words in our manuscript.

III. I would switch table 3 and 4, because table 4 shows the unadjusted results, whereas table 3 adjusts for potential confounders.

The authors' answer: this part has been revised according to dear reviewer's suggestion.

IV. P2. Abstract: the conclusion that the Chinese government should increase awareness especially among the low-income groups is not justified. This data is not shown in the result section of the abstract.

The authors' answer: thanks to reviewer's carefulness, we revised this in the corresponding part.

V. P6. Statistical analyses: please remove the word 'univariate' before logistic regression analyses, because multiple confounders were entered.

The authors' answer: we have revised this accordingly in method part.

VI. P7. Please provide exact p value instead of $p > 0.05$.

The authors' answer: exact p value was added to the table.

VII. P12. Please define a 'positive lifestyle'.

The authors' answer: the explanation was added to brackets after 'positive lifestyle' accordingly.

VERSION 2 – REVIEW

REVIEWER	Kyu-Tae Han Department of Public Health, College of Medicine, Yonsei University
REVIEW RETURNED	20-May-2016

GENERAL COMMENTS	The revised manuscript was well described compared to previous manuscript. The question about this study was mostly solved during this round. However, more detailed discussion would increase the quality of study.
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REVIEWER	Mariska Bot VU University Medical Center, The Netherlands
REVIEW RETURNED	25-May-2016

GENERAL COMMENTS	The authors did a good job in responding to and clarifying the issues raised by the reviewers. I have a small limitation, however. In the discussion, the authors state that their finding is in line with many previous studies. However, there are also studies showing no significant association between mental health and exposure to SHS. This is not reflected now in the discussion.
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VERSION 2 – AUTHOR RESPONSE

Reviewer 2:

Kyu-Tae Han

Institution and Country

Department of Public Health, College of Medicine, Yonsei University

The reviewer's comment:

I. More detailed discussion would increase the quality of study.

The authors' answer: Reviewer's comments were very important and helpful to our manuscript. We have reviewed some prior research and added information to the discussion part.

Reviewer 3:

Mariska Bot

Institution and Country

VU University Medical Center, The Netherlands

The reviewer's comment:

I. In the discussion, the authors state that their finding is in line with many previous studies. However, there are also studies showing no significant association between mental health and exposure to SHS. This is not reflected now in the discussion.

The authors' answer: Thanks to reviewer's valuable comments, we have reviewed prior research and added information to the discussion part.