

PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Occupational noise-induced hearing loss in China: a systematic review and meta-analysis
AUTHORS	Zhou, Jiena; Shi, Zhihao; Zhou, Lifang; Yong, Hu; Zhang, Meibian

VERSION 1 – REVIEW

REVIEWER	Hong Zhang Merck & Co., Inc., USA
REVIEW RETURNED	11-May-2020

GENERAL COMMENTS	<p>The authors conducted a fixed-effect meta-analysis of the prevalence and risk of noise-induced hearing loss in China. 88 studies from 1993 to 2019 were included. Both pan study-types (cross-sectional, cohort and hot-spot) and per study-types results were presented. The meta prevalence rates, odds ratios of risk factors and effect sizes of noise level and exposure duration etc. were highlighted and discussed. However, there are a few statistical concerns that need to be clarified.</p> <ol style="list-style-type: none">1. P.2 Abstract Results, lines 42-54: the reported odds ratios are derived from the analysis of HFNIHL only. This should be made clear.2. P.11 lines 20-22, P.13 lines 15-17. The authors seemed to imply that the meta prevalence of HFNIHL is estimated from fixed-effect models. This is confusing – isn't the meta prevalence just the average of the prevalence in each study weighted by its sample size? Please clarify.3. P.11 lines 20-22, P.13 lines 15-17. The authors also mentioned that the meta prevalence is "significantly higher" in one risk group than the other. Statements like this need statistical test to back up.4. Table 2. When comparing complex and Gaussian noise, there seems to be a remarkable subgroup effect of the type of factory. The ORs are much larger in the machinery while they are close to one when the Gaussian noise is from clothing/textile and complex noise from hardware/metal/steel factories. The authors should comment on this subgroup effect and its impact on the interpretation of the presented meta OR = 2.88.5. P.16 (Summary of the epidemiological characteristics of occupational NIHL) and Figure 2. The authors seem to group the studies according to the risk factor of interest (6 groups for noise level and 3 groups for exposure duration). Then a linear regression was performed on the average HFNIHL% ~ numeric values of the groups (1-6 or 1-3). This procedure seems rather unusual and somewhat arbitrary:<ol style="list-style-type: none">5.1 What is the interpretation of the estimated regression coefficient?5.2 If the definition of the group changes, would the estimated regression coefficients be different?
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	5.3 Why not present the scatter plots of the noise levels/exposure duration and HFNIHL% and conduct regression at the study level (i.e. not grouping them)?
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REVIEWER	Dr.Phayong Thepaksorn Sirindhorn College of Public Health, Trang Faculty of Public Health and Allied Health Sciences Proboromarajchanok Institute, Thailand
REVIEW RETURNED	21-Jun-2020

GENERAL COMMENTS	<p>The authors conducted a number of literature reviews on occupational noise-induced hearing loss (NIHL) in China. The study aimed to analyze the prevalence and characteristics of occupational NIHL in the Chinese population using data from several relevant studies. The written and communication are well demonstrated and clearly enough for research presentation in particular circumstances. Here are some recommendations for improvement the paper as follows:</p> <p>P9, Line 54: In data analysis and extraction, ORs were calculated based on the characteristics of the studies, including.....please give more details. In addition, it would be good if the authors could present flowchart of the selection of articles for meta-analysis and systematic review (Fig 1).</p> <p>P23, Line 25: ...Moreover, only four cohort studies..., but they are only three cohort studies in table 1, please re-check. In the limitations of this study, please state and clarify more for several studies were conducted in cross-sectional studies. Therefore, it would be limited in determining the relationships between cause or occupational exposure and relevant outcomes of NIHL, for example.</p> <p>P43, flow chart of selection the articulated for meta-analysis. Is it only meta-analysis or systematic and meta-analysis reviewed articles?</p> <p>P49, in table 4 of meta-analysis of cross-sectional studies with references to NIHL among manufacturing workers, it would be good if the authors could present in forest plots used in a meta-analysis to represent effect sizes and their associated precisions. A forest plots are displaying the magnitude and uncertainty (95% confidence interval, CI) of each effect size in the dataset, as well as reporting the associated numerical values and a reference to the original papers.</p> <p>Even though several studies are conducted among young age workers (30-40 years old), the effects of aging, may reduce the prominence of the “notch.” Therefore, in older individuals, the effects of noise may be difficult to distinguish from age-related hearing loss without access to previous audiograms. NIHL is generally observed to decrease hearing sensitivity in the higher frequencies, also called an audiometric notch, especially at 4000 Hz, but sometimes at 3000 or 6000 Hz. The symptoms of NIHL are usually presented equally in both ears. In addition, the relative risk of NIHL increases with long-term noise exposures above 80 dB and increases significantly as exposures rise above 85 dB.</p> <p>In overall recommendations, I recommend to accept the article after indicated minor corrections.</p>
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VERSION 1 – AUTHOR RESPONSE

Responses to Prof. Hong Zhang

1.P.2 Abstract Results, lines 42-54: the reported odds ratios are derived from the analysis of HFNIHL only. This should be made clear.

Response: Thank you for the suggestion. The reported odds ratios are indeed derived from the analysis of HFNIHL only. We have made it clear in the Abstract part (to see line 16-21, p2, in red).

2.P.11 lines 20-22, P.13 lines 15-17. The authors seemed to imply that the meta prevalence of HFNIHL is estimated from fixed-effect models. This is confusing – isn't the meta prevalence just the average of the prevalence in each study weighted by its sample size? Please clarify.

Response: Thank you for your advice. Indeed, the meta prevalence is not estimated from the fixed-effect model, it is the average of the prevalence in each study weighted by its sample size. The sentence has re-written in the revised manuscript (to see line 9-11, p11, and line 5-7, p13, in red).

3.P.11 lines 20-22, P.13 lines 15-17. The authors also mentioned that the meta prevalence is "significantly higher" in one risk group than the other. Statements like this need statistical test to back up.

Response: Thank you for the suggestion. The statistical method is the Chi-square test, which has been added in the revised manuscript (to see line 9-11, p11, and line 5-7, p13, in red).

4.Table 2. When comparing complex and Gaussian noise, there seems to be a remarkable subgroup effect of the type of factory. The ORs are much larger in the machinery while they are close to one when the Gaussian noise is from clothing/textile and complex noise from hardware/metal/steel factories. The authors should comment on this subgroup effect and its impact on the interpretation of the presented meta OR = 2.88.

Response: Thank you very much for the careful check on the statistical results. After re-examining the process of data statistics and analysis, we found that in the machinery subgroup, the total number of "271" subjects was wrongly input to "217", leading to the error of the increase in OR when performing meta-analysis. Therefore, the exact OR for the subgroup is "2.94", instead of "82.3", and the overall weighted OR for complex noise affecting HFNIHL prevalence was "1.95" (to see table 2, in red). In addition, the OR in another machinery subgroup was 9.13, which was relatively higher than subgroups. The reason might be related to the complexity of the temporal structure of noise generated from mechanical processes, which might make complex noise from the machinery industry a greater contribution to HFNIHL than other industries. We have made an explanation in the discussion section (to see line 18-22, p20, in red).

5.P.16 (Summary of the epidemiological characteristics of occupational NIHL) and Figure 2. The authors seem to group the studies according to the risk factor of interest (6 groups for noise level and 3 groups for exposure duration). Then a linear regression was performed on the average HFNIHL% ~ numeric values of the groups (1-6 or 1-3). This procedure seems rather unusual and somewhat arbitrary:

Response: Thank you for your suggestions and comments. As the reviewer mentioned, the procedure of the grouping seems rather unusual and somewhat arbitrary. We decided to delete the Fig 2 regarding the linear regression equations.

5.1 What is the interpretation of the estimated regression coefficient?

Response: Thank you for your comments. The regression coefficient is a parameter of the influence of an independent variable on the dependent variable. In this study, we attempt to observe the relationship between HFNIHL% and risk factors using multiple approaches, such as ORs and determining coefficient (R²). R² serves as a parameter to reflect the degree of correlation between HFNIHL% and noise level or exposure duration through establishing their linear regression equations.

The related descriptions have been deleted based on the reviewer's comment.

5.2 If the definition of the group changes, would the estimated regression coefficients be different?

Response: Thank you for the question. As the reviewer mentioned, the R2 will be different if the definition of the group changes because of insufficient sample size. The related descriptions have been deleted based on the reviewer's comment.

5.3 Why not present the scatter plots of the noise levels/exposure duration and HFNIHL% and conduct regression at the study level (i.e. not grouping them)?

Response: Thank you for your advice. We conduct the scatter plot according to your suggestion. And we find that the linear regression between HFNIHL% and noise level or exposure duration obtained from the scatter plot was not positive. The reason is also related to the insufficient sample size. We cannot obtain the detailed original data from each study. Therefore, we decide to give up the analysis of the linear regression and its R2. However, it does not affect our observation on the relationships between HFNIHL% and risk factors. Their relationships are clear in Table 5 when using the overall weighted ORs.

Response to reviewers' comments:

Response: 2, to Dr. Phayong Thepakorn

1.P9, Line 54: In data analysis and extraction, ORs were calculated based on the characteristics of the studies, including.....please give more details. In addition, it would be good if the authors could present flowchart of the selection of articles for meta-analysis and systematic review (Fig 1).

Response: Thank you for your advice. The description of "including the characteristics of the subjects (e.g. sex, age, and exposure duration), type of noise (complex noise vs Gaussian noise), and exposure characteristics (noise exposure vs no noise exposure, co-exposure of noise and chemicals vs noise exposure)" has been added in the "Data analysis and extraction" section (to see line 21-22, p8, in red).

This paper already provided a flow chart of the selection of literature (to see Figure 1). In addition, and a precise search strategy was also added as a supplementary file according to the editor's comment (to see the supplementary file).

2.P23, Line 25: ...Moreover, only four cohort studies..., but they are only three cohort studies in table 1, please re-check. In the limitations of this study, please state and clarify more for several studies were conducted in cross-sectional studies. Therefore, it would be limited in determining the relationships between cause or occupational exposure and relevant outcomes of NIHL, for example.

Response: Thank you for your advice. Indeed, Only 3 cohort studies have been included in this paper, which have been re-checked (to see line 10, p22, in red).

A description of the limitation has been added in the revised manuscript (to see line 11-13, p22, in red).

3.P43, flow chart of selection the articles for meta-analysis. Is it only meta-analysis or systematic and meta-analysis reviewed articles?

Response: The reviewed articles are included in the systematic and meta-analysis (to see line 15, p8 and line 5, p9, in red).

4.P49, in table 4 of meta-analysis of cross-sectional studies with references to NIHL among manufacturing workers, it would be good if the authors could present in forest plots used in a meta-analysis to represent effect sizes and their associated precisions. A forest plot is displaying the magnitude and uncertainty (95% confidence interval, CI) of each effect size in the dataset, as well as reporting the associated numerical values and a reference to the original papers.

Response: Thank you for your suggestion. The forest plot was added as an appendix in the revised manuscript (to see Figure 2). And the related description has been added in the results part (to see line 10-12, p10, in red).

VERSION 2 – REVIEW

REVIEWER	Hong Zhang Merck & Co., Inc., USA
REVIEW RETURNED	03-Aug-2020

GENERAL COMMENTS	Thank you for providing all the required clarifications. I have no further comments. The only suggestion is that it would be much appreciated if the author can provide a point-by-point response to the comments.
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REVIEWER	Dr.Phayong Thepaksorn Siridhorn College of Public Health, Trang Faculty of Public Health and Allied Health Sciences Praboromarajchanok Institute Thailand
REVIEW RETURNED	29-Jul-2020

GENERAL COMMENTS	Just only minor revisions are required as follows: P4, Line 15: Keywords: “Review” should use the word “Systematic review” P43, in forest plot: overall (I-squared =90.6% p= 0.000), the p-value should demonstrate as p <0.001. In overall recommendations and communication to the readers, the authors have revised and corrected as suggestions and I recommend it for publication with minor changes.
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VERSION 2 – AUTHOR RESPONSE

Response: 2, to Dr.Phayong Thepaksorn

1.P4, Line 15: Keywords: “Review” should use the word “Systematic review”

Response: Thank you for your suggestion. The keyword “Review” has been changed to “Systematic review” (to see line 15, p4, in red).

2.P43, in forest plot: overall (I-squared =90.6% p= 0.000), the p-value should demonstrate as p <0.001.

Response: Thank you for your suggestion. The “p= 0.000” in the forest plot has been modified to “p<0.001” (to see P43, figure 2)

Responses to reviewers’ comments:

Responses to Prof. Hong Zhang

1.I have no further comments. The only suggestion is that it would be much appreciated if the author can provide a point-by-point response to the comments.

Response: Thank you for your advice. We have checked our previous responses to the comments, which have been the point-by-point responses. We make sure we respond point -to- point in the following responses.