## PEER REVIEW HISTORY



## ARTICLE DETAILS

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| **TITLE (PROVISIONAL)** | Turnover intention among primary health workers in China: a systematic review and meta-analysis |
| **AUTHORS** | He, Rongxin; Liu, Jinlin; Zhang, WeiHong; Zhu, Bin; Zhang, Ning; Mao, Ying |

## VERSION 1 – REVIEW

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| **REVIEWER** | Chiara Pomare<br>Australian Institute of Health Information, Macquarie University, Australia |
| **REVIEW RETURNED** | 25-Mar-2020 |

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| **GENERAL COMMENTS** | This is an interesting and important paper analysing the prevalence and determinants of turnover intentions in primary health workers in China.<br><br>A thorough edit of language is needed to ensure the writing is of a high standard (e.g., Abstract - Meta-analyses indicated that 21 factors were significantly associated with turnover intention, included - should be 'including'<br>Other issues such as capitalisation, consistent use of tense and spacing need to be addressed.<br><br>The conclusion (both in the abstract and the conclusion sub-section) is rather simplistic. Instead, the authors should provide more concrete implications for what factors should be addressed to avoid TI.<br><br>Methods - report inter-rater reliability for the title abstract screening by the two authors.<br><br>Results: direction of odds ratios should be explained.<br><br>Did all studies in the meta-analysis use a consistent measure of TI? What was this measure? And if not, how was this accounted for? Needs to be clarified in the manuscript.<br><br>The comment at the end of the discussion "There is still needed to collect more relevant studies to make more in-depth analyses in the future" is vague (and an example of poor language). Authors need to specifically state what studies are needed based on the findings of this review.<br><br>Table 1. Column title 'number' needs a better heading. It is unclear what this column includes.<br>Assessment tools - suggest changing to "TI assessment tool"<br>Similarly, "Prevalence of TI" |

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| | Figures 2 - 4 are poor quality. |

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| **REVIEWER** | Ericson Gutierrez<br>Peruvian National Institute of Health. Lima, Perú. |
| **REVIEW RETURNED** | 01-Apr-2020 |

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| **GENERAL COMMENTS** | The article needs to discuss its results with international references, for example I could cite the “Global Strategy on Human Resources for Health: Workforce 2030” https://apps.who.int/iris/bitstream/handle/10665/250368/9789241511131-eng.pdf?sequence=1 (WHO). In your first objective, “Optimize performance, quality and impact of the health workforce through evidence-informed policies on human resources for health, contributing to healthy lives and wellbeing, effective universal health coverage, resilience and health security at all levels.<br>In the recommendations for “Policy options for WHO Member States” it is recommended “Promote decent working conditions in all settings. In this sense, the document states that “Ministries of health, civil service commissions and employers should adopt gender-sensitive employment conditions, remuneration and non-financial incentives. They should cooperate lto ensure fair terms for health workers, merit-based career development opportunities and a positive practice environment to enable their effective deployment, retention and adequate motivation to deliver quality care and build a positive relationship with patients. Harm to health workers, together with gender-based discrimination, violence and harassment during training, recruitment/ employment and in the work place, should be eliminated. It is particularly important to find pragmatic solutions to overcome deeply entrenched rigidities in public sector rules and practices that hinder the adoption of adequate reward systems, working conditions and career structures for health workers, with appropriate levels of flexibility and autonomy.<br>In your study you found that “There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intention”. I suggest to discuss the results obtained with the recommendations provided by the WHO document, so that your article provides contributions of possible strategies to follow to reduce the turnover intention among primary health workers in China |

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| **REVIEWER** | Mark Harris<br>University of New South Wales, Sydney<br>Australia |
| **REVIEW RETURNED** | 05-Apr-2020 |

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| **GENERAL COMMENTS** | This is an interesting review of 16 cross sectional studies on turnover intentions of primary health care workers in China. It is an important study with implications for policy in China drawing on publications in both English and Chinese languages. There are some issues which deserve more discussion:<br>1. The primary health workers (PHWs) sampled in these studies were heterogeneous with respect to training and qualifications across the studies. The turnover intensions of an older practitioner with limited training in a rural area could be expected to be quite different from younger practitioner having received full medical training in an urban area. This deserves more discussion. |

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| | 2. It is unclear if the studies explored the interaction effects of demographic and other factors. For example, age, education, job title and work seniority are all likely to be correlated. However, it is hard to determine from the findings how these might have interacted in their effect on turnover intention.<br>3. The discussion briefly refers to the seven factors rarely reported previously. Of these “emotional exhaustion” and “flattening of affect” are measures of mental health status. There is a very large body of literature linking mental illness especially depression with work satisfaction – both of which are in turn related to turnover intentions. The effect of mental health on turnover intentions warrants further discussion especially given the impact of the recent COVID-19 epidemic on the mental health of the health workforce.<br><br>Minor points:<br>• It is unclear what “individual value embodiment” means?<br>• The discussion refers to ‘income’ rather than ‘income satisfaction’ referred to in the tables etc. These are very different and should not be conflated.<br>• There are numerous grammatical errors in the discussion. |

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| **REVIEWER** | Seeromanie Harding<br>King's College London<br>UK |
| **REVIEW RETURNED** | 15-Apr-2020 |

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| **GENERAL COMMENTS** | 2. The concept of turnover intention needs to expanded as it is key to the paper. Explain what community facilities refer to and include frequency of determinants.<br>4. PRISMA guidelines need to reviewed for alignment e.g. explain substantial heterogeneity of >50%<br>6. Outcomes need to defined as primary and secondary<br>7. In the introduction it would be useful for the international audience to have a description of the primary health care system in China and the composition of its workers. The discussion raises the importance of regional differences in factors that influence health care workers. This would then justify the analytic approach - sub-group analyses (region, urban/rural etc), sensitivity analyses<br><br>9. See 7. The aims of the paper are very general and could be more specific e.g. relating to the different sub-group analyses. The value of the findings would be greatly enhanced by probing more systematically using sub-group analyses e.g. within the different regions are there differences in TI by occupation, what are the factors that influence any observed differences?<br>10. The presentation could be improved considerably. Paragraphs of mainly statistics are not appealing.<br>Table 1 is confusing - the row labels need to be revised e.g. Participants - one would expect this to refer to the characteristics of primary health care workers(, Prevalence - assume this is prevalence of turnover?<br>The text could be improved by adding a brief description of the studies, reporting on subgroup analyses as suggested above. TI may not have differed between doctors and nurses (p9) but do the determinants of TI differ by occupation?<br>The prevalence should be presented as % (as in the abstract)<br>11. The structure of the discussion interrupts with the coherence A reshaping with sub-titles would improve the flow - principal |

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| | findings, limitations and strengths, comparisons with other studies, implications and conclusions<br>Overall an interesting paper that could be useful for planners of primary care provision but could be improved with systematic probing relating to the contextual issues that motivates retention/turn over of primary health care workers. |

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| **REVIEWER** | Abdelrahman Ibrahim Abushouk<br>Beth Israel Deaconess Medical Center |
| **REVIEW RETURNED** | 09-Jun-2020 |

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| **GENERAL COMMENTS** | The authors performed a comprehensive systematic review, attempting to identify the rate and risk factors of turnover intentions among primary health workers in China. The methods are well-described and the conclusions are supported by the data; however, I have some concerns:<br><br>* Abstract<br>- The abstract should mention statistical values for major outcomes. Also, the recommendation made in the conclusion is quite non-specific and more specific recommendations should be added in light of the study findings.<br><br>* Introduction<br>- The authors presented their rationale that there is no systematic review on the topic from China. Yet, this is not enough. A rationale for a meta-analysis should identify the controversies among the published studies and the evidence gap in the literature.<br><br>* Methods<br>- The authors mentioned they followed PRISMA checklist. For SRs of observational studies, the MOOSE checklist is more appropriate.<br>- In the abstract, they say they searched four databases, while in the methods, they say five.<br>- I the study eligibility, replace "type of intervention" with "risk factor" considering the type of this meta-analysis.<br>- How did the authors ensure that factors are reported and extracted homogenously among the included studies? Was any statistical conversion needed?<br>- How did you modify NOS to make it only 7 points and why?<br>- Also, what was the NOS scoring based on?<br>- In the data synthesis section, I believe the authors should make it clear that the prevalence was assessed via single-arm analysis.<br>- Page 6, the authors mentioned that they analyzed associated factors as log odds ratio, but when I inspected the figure, that does not seem to be the case. Please clarify?<br>- When the authors observed heterogeneity, what was done about it?<br>- Otherwise, the data synthesis methods are well-described.<br><br>* Results<br>- "the 16 included studies was 5.25 of 7 points, indicating a moderate-average quality," What does a moderate-average mean?<br>- Page 7, line 30: So, the authors mean that small studies inflate the prevalence?<br>- Page 7, line 32: What was done to resolve that heterogeneity?<br>- In reporting the factors, it is quite confusing to put all these numbers in text. This is what tables are for. Therefore, the authors |

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| | should report the main numbers and put the detailed numbers in tables.<br><br>* Discussion: The authors should discuss how their findings, derived from Chinese studies, can be extrapolated to the world.<br>- The discussion should also present recommendations for stake holders and policy makers to improve the retention of PHWs and eliminate the obstacles they are facing.<br><br>* Appendix<br>- In Appendix Table 1, identify in the table footnote what A1, A2, ... etc refer to among the factors? |

**VERSION 1 – AUTHOR RESPONSE**

Dear reviewer,
We would like to thank the editor and reviewers for their time and valuable comments. We appreciate that the reviewers found the study well-executed and the methodology innovative. The suggested edits and constructive criticism were invaluable and truly made this version of the paper much better. We sincerely appreciate your suggestions and did our best to incorporate them into the revised manuscript.

Thank you once again for your time and suggestions. We really appreciate it and look forward to hearing from you.

Sincerely,
Ying Mao

Comments and Suggestions for Authors:

This is an interesting and important paper analysing the prevalence and determinants of turnover intentions in primary health workers in China.

1. A thorough edit of language is needed to ensure the writing is of a high standard (e.g., Abstract - Meta-analyses indicated that 21 factors were significantly associated with turnover intention, included - should be 'including'
Other issues such as capitalisation, consistent use of tense and spacing need to be addressed.

Many thanks for your kind comment! We are sorry that there are some mistakes in writing. We have polished the paper by the AJE English language editing service.
Please check the manuscript for more details.

2. The conclusion (both in the abstract and the conclusion sub-section) is rather simplistic. Instead, the authors should provide more concrete implications for what factors should be addressed to avoid TI.

Thanks a lot for your suggestion! We have enriched the content of the conclusion (both in the abstract and the conclusion sub-section) and provided more concrete suggestions.
“The analysis highlights the problem of turnover intentions among PHWs in China. There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intentions. Policymakers should take into account all aspects of human needs that influence PHWs’ intentions to stay. As illustrated by the Global Strategy on Human Resources for

Health, it is particularly important to find pragmatic solutions to overcome deeply entrenched rigidities in public sector rules and practices that hinder the adoption of adequate reward systems, working conditions and career structures for health workers, with appropriate levels of flexibility and autonomy. Therefore, efforts can be made to improve factors both at work and outside of work. In terms of work factors, policymakers should continue to improve reward systems, the construction of infrastructure, and promotion systems. Outside of work, authorities should pay more attention to PHWs' lives and meet their living needs to increase their willingness to work and live in communities, towns and villages. We also suggest that particular attention be given to PHWs working in the community or the eastern region of China to reduce their turnover intentions by implementing evidence-based health workforce policies."

3. Methods - report inter-rater reliability for the title abstract screening by the two authors.

Thanks for your suggestion! We have described the data extraction process in the Methods- Data extraction section but did not report inter-rater reliability (IRR).
We have calculated the IRR between two authors based on their records.
"Data extraction was conducted by one author and reviewed independently by two other authors, with disagreements resolved by discussion until consensus was reached. The inter-rater reliability for title screening between two authors was 96.15%, and for abstract screening was 94.74%. The full inter-rater reliability result can be found in the Supplementary Tables S2."

4. Results: direction of odds ratios should be explained.

Thanks a lot for your suggestion! We accepted your advice. We have explained the direction of odds ratios.
"which showed that the PHWs with higher risks of TI were male, were younger, had a higher education, were unmarried, and worked in the remote region."
"which presented that the PHWs with higher risks of TI were those with shorter work seniority, higher work stress, and longer working hours."
"The results showed that PHWs who dissatisfied their job had significantly higher risks of TI."

5. Did all studies in the meta-analysis use a consistent measure of TI? What was this measure? And if not, how was this accounted for? Needs to be clarified in the manuscript.

Many thanks for your kind comment!
We are sorry that we did not clarify the measure of TI in the description.
As you can see in Table 1, most of the studies used a dichotomous question to measure TI (Do you want to leave your job? Yes/No). And there are three studies used scales. Actually, in these studies, the authors gave us their measure to divide TI into binary variables, which was used in the multiple logistic regression. For example, Zhou (Ref. No.55) used the Michael & Spector Turnover intention Scale to measure TI, and defined a score of > 3 out of 5 indicated PHWs got an intention to leave.
We have revised this statement accordingly in the description of Table 1.
"Thirteen studies used a dichotomous question to measure TI (Do you want to leave your job? Yes/No), and three studies used scales."

6. The comment at the end of the discussion "There is still needed to collect more relevant studies to make more in-depth analyses in the future" is vague (and an example of poor language). Authors need to specifically state what studies are needed based on the findings of this review.

Many thanks for your kind comment!
We are sorry that we did not specifically mention what studies are needed based on the findings of this review.

We have revised this statement base on the results and findings.
“Therefore, it can be concluded that there are many facets of the TIs among PHWs that need to be explored. First, the impact of family factors on TIs requires more attention. Second, there is insufficient research on the interaction effects of demographics and other factors. More research is needed to better represent and understand how two or more determinants work together to impact the TIs of PHWs. Finally, the relationship between public health services and the TIs of PHWs in the context of the COVID-19 is a worthy research issue.”

7.Table 1. Column title 'number' needs a better heading. It is unclear what this column includes. Assessment tools - suggest changing to "TI assessment tool"
Similarly, "Prevalence of TI"

Thanks for your detailed suggestion. We have revised this statement accordingly.
We have revised the row labels of Table 1, replaced the word "Participants" with "Research sites", "Number" with "sample size", "Assessment tool" with "TI assessment tool"," Prevalence" with " Prevalence of TI".

Figures 2 - 4 are poor quality.
We have tried our best to improve the quality of the figures. Please check the manuscript for more details.

Dear reviewer,
We would like to thank the editor and reviewers for their time and valuable comments. We appreciate that the reviewers found the study well-executed and the methodology innovative. The suggested edits and constructive criticism were invaluable and truly made this version of the paper much better. We sincerely appreciate your suggestions and did our best to incorporate them into the revised manuscript.

Thank you once again for your time and suggestions. We really appreciate it and look forward to hearing from you.

Sincerely,
Ying Mao

Comments and Suggestions for Authors:

The article needs to discuss its results with international references, for example I could cite the “Global Strategy on Human Resources for Health: Workforce 2030” https://apps.who.int/iris/bitstream/handle/10665/250368/9789241511131-eng.pdf?sequence=1 (WHO). In your first objective, “Optimize performance, quality and impact of the health workforce through evidence-informed policies on human resources for health, contributing to healthy lives and wellbeing, effective universal health coverage, resilience and health security at all levels.
In the recommendations for “Policy options for WHO Member States” it is recommended “Promote decent working conditions in all settings. In this sense, the document states that “Ministries of health, civil service commissions and employers should adopt gender-sensitive employment conditions, remuneration and non-financial incentives. They should cooperate lto ensure fair terms for health workers, merit-based career development opportunities and a positive practice environment to enable their effective deployment, retention and adequate motivation to deliver quality care and build a positive relationship with patients. Harm to health workers, together with gender-based discrimination, violence and harassment during training, recruitment/ employment and in the work place, should be eliminated. It is particularly important to find pragmatic solutions to overcome deeply entrenched rigidities in public sector rules and practices that hinder the adoption of adequate reward systems,

working conditions and career structures for health workers, with appropriate levels of flexibility and autonomy.
In your study you found that “There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intention”. I suggest to discuss the results obtained with the recommendations provided by the WHO document, so that your article provides contributions of possible strategies to follow to reduce the turnover intention among primary health workers in China.

Thank you so much for the kind comment to cite “Global Strategy on Human Resources for Health: Workforce 2030”, which perfectly coincides with our research results. We accepted your advice and included in the conclusion.
“The analysis highlights the problem of turnover intentions among PHWs in China. There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intentions. Policymakers should take into account all aspects of human needs that influence PHWs’ intentions to stay. As illustrated by the Global Strategy on Human Resources for Health, it is particularly important to find pragmatic solutions to overcome deeply entrenched rigidities in public sector rules and practices that hinder the adoption of adequate reward systems, working conditions and career structures for health workers, with appropriate levels of flexibility and autonomy [63]. Therefore, efforts can be made to improve factors both at work and outside of work. In terms of work factors, policymakers should continue to improve reward systems, the construction of infrastructure, and promotion systems. Outside of work, authorities should pay more attention to PHWs’ lives and meet their living needs to increase their willingness to work and live in communities, towns and villages. We also suggest that particular attention be given to PHWs working in the community or the eastern region of China to reduce their turnover intentions by implementing evidence-based health workforce policies.”

Dear reviewer,
We would like to thank the editor and reviewers for their time and valuable comments. We appreciate that the reviewers found the study well-executed and the methodology innovative. The suggested edits and constructive criticism were invaluable and truly made this version of the paper much better. We sincerely appreciate your suggestions and did our best to incorporate them into the revised manuscript.

Thank you once again for your time and suggestions. We really appreciate it and look forward to hearing from you.

Sincerely,
Ying Mao

Comments and Suggestions for Authors:

This is an interesting review of 16 cross sectional studies on turnover intentions of primary health care workers in China. It is an important study with implications for policy in China drawing on publications in both English and Chinese languages. There are some issues which deserve more discussion:

1. The primary health workers (PHWs) sampled in these studies were heterogeneous with respect to training and qualifications across the studies. The turnover intensions of an older practitioner with limited training in a rural area could be expected to be quite different from younger practitioner having received full medical training in an urban area. This deserves more discussion.

Many thanks for your kind comment. We all agree with you that this issue deserves further discussion. As we can observe in the results, PHWs who were male, younger, had a higher education, were

unmarried, and worked in the remote region showed a higher risk of TI. Based on these results, we can expect that a young practitioner had received full medical training may with a higher TI than an older practitioner with limited training. We have given more discussion related to this situation.
“It can be concluded that different types of PHWs have unique characteristics of TI. Accordingly, we can sum up the high-risk population among PHWs. For example, the turnover intentions of an unmarried young practitioner who received full medical training could be expected to be higher than a married older practitioner with limited training. The policymakers and medical institutions managers should formulate or adjust retention measures based on these characteristics.”

2. It is unclear if the studies explored the interaction effects of demographic and other factors. For example, age, education, job title and work seniority are all likely to be correlated. However, it is hard to determine from the findings how these might have interacted in their effect on turnover intention.

Many thanks for your kind comment! Based on the studies we included in this review, most of the studies conducted the binary logistic regression on turnover intentions but did not explore the interaction effects of demographics and other factors. Furthermore, a few studies explored the mediating effect of job satisfaction between work stress and turnover intention by the SEM. We all agree that some factors are all likely to be correlated, which means the interaction effects are truly exist. At present, there is insufficient research on the interaction effects of demographics and other factors. This paper has pointed this out at the Literature Gaps section in the discussion.
“Second, there is insufficient research on the interaction effects of demographic and other factors. More research is needed to represent better and understand how two or more determinants work together to impact the TIs of PHWs.”

3. The discussion briefly refers to the seven factors rarely reported previously. Of these “emotional exhaustion” and “flattening of affect” are measures of mental health status. There is a very large body of literature linking mental illness especially depression with work satisfaction – both of which are in turn related to turnover intentions. The effect of mental health on turnover intentions warrants further discussion especially given the impact of the recent COVID-19 epidemic on the mental health of the health workforce.

Thank you for raising the question. As you said, there is a vast body of literature linking mental illness especially depression with work satisfaction. In recent years, Chinese academics have paid more and more attention to the mental health of the health workforce due to the increasing work stress and violence against doctors. However, few studies have been done on the impact of mental health on TI among PHWs, but significant results have been obtained. In the context of COVID-19, all PHWs have been mobilized to fight the epidemic, which will undoubtedly increase PHW's mental stress. We have given the further discussion on this issue.
“Among these factors, “emotional exhaustion” and “flattening of affect” are measures of mental health status. In recent years, the mental health status of the health workforce has deteriorated due to increasing work stress and violence [59,60]. Some studies also found that mental health has significant associations with job satisfaction and job burnout [18,26,61]. In the context of COVID-19, all PHWs have been mobilized to fight the epidemic, which will undoubtedly have a negative impact on their mental health status [62,63]. The risk of TIs caused by mental health problems cannot be ignored.”

Minor points:
• It is unclear what “individual value embodiment” means?

We are sorry that we did not make it clear. In this part, the meaning of “individual value embodiment” was the feeling of accomplishment that PHWs get from the job. We decided to use the word “sense of accomplishment” to make it easier to understand.

• The discussion refers to ‘income’ rather than ‘income satisfaction’ referred to in the tables etc. These are very different and should not be conflated.

Thanks for your kind comment. Based on the studies which were included in this review, some studies investigated the income of PHWs. Meanwhile, other researchers explored income satisfaction in their studies. It caused two factors that came together in this article, but income belongs to job characteristic factors and income satisfaction as part of job satisfaction factors. In order to avoid confusion, we have changed the word ‘income’ to ‘remuneration’.

• There are numerous grammatical errors in the discussion.

We are sorry that we made numerous grammatical mistakes in the discussion. We have polished the paper by the AJE English language editing service.

Dear reviewer,
We would like to thank the editor and reviewers for their time and valuable comments. We appreciate that the reviewers found the study well-executed and the methodology innovative. The suggested edits and constructive criticism were invaluable and truly made this version of the paper much better. We sincerely appreciate your suggestions and did our best to incorporate them into the revised manuscript.

Thank you once again for your time and suggestions. We really appreciate it and look forward to hearing from you.

Sincerely,
Ying Mao

Comments and Suggestions for Authors:

2. The concept of turnover intention needs to expanded as it is key to the paper. Explain what community facilities refer to and include frequency of determinants.

Many thanks for your kind comment!
We have expended the concept of turnover intention.
“Turnover, a behavior of actually leaving, was an important value in human resources management and maintenance of the current workforce. Turnover intention (TI) is defined as the probability that an employee will leave his or her job within a specific period; TI is considered to one of the best predictors of turnover behavior.”
Sorry, we did not present the word "community facilities" clear. In China, primary health care institutions in the community include health service centers or stations in urban areas. So the community facilities refer to primary health care institutions in the community. To not mislead the reader, we changed "community facilities" to "community primary health care institutions". We tried to point this out because there is no study explored the turnover intention of PHWs between different primary health care institutions in China.

4. PRISMA guidelines need to reviewed for alignment e.g. explain substantial heterogeneity of >50%.

Many thanks for your kind comment and suggestion!
We have reviewed the PRISMA guidelines again, and explained substantial heterogeneity of >50%. We have pointed it out in the limitation section.

“Significant heterogeneity among the individual studies was found when the subgroup analysis and the part of the meta-analysis were performed. The main reason is the heterogeneity between different studies in research region and research site.”

6. Outcomes need to defined as primary and secondary.

Thank you for raising the question.
We have tried to define the primary and secondary outcomes of this study:
1) Primary outcome: summarizing the prevalence of overall/subgroup turnover intention.
2) Secondary outcome: identifying determinants of turnover intention.
Please check the manuscript in the section of data synthesis and statistical analysis.
“The primary outcome in this review was the difference in the prevalence or relative risk of TI among different groups.”
“The secondary outcome of this study was the association between factors and TI among PHWs in the form of the odds ratio.”

7. In the introduction it would be useful for the international audience to have a description of the primary health care system in China and the composition of its workers. The discussion raises the importance of regional differences in factors that influence health care workers. This would then justify the analytic approach - sub-group analyses (region, urban/rural etc), sensitivity analyses

Thank you for raising the question.
As mentioned above, primary health care institutions include community health service centers or stations in urban areas, township health centers and village clinics in rural areas. These institutions offer primary health care services, including basic medical and public health services to residents in their communities.
We have given a description of the primary health care system in China and the composition of its workers in the manuscript.
“In China, the PHC services including basic medical and public health services, are provided by community health centers and stations in the urban areas and by township health centers and village clinics in rural areas. These four types of PHC institutions constitute the essential part of China’s three-tertiary health care delivery network. PHWs working inside include doctors, nurses, public health workers and administrative staff, most of them have to play multiple roles.”

9. See 7. The aims of the paper are very general and could be more specific e.g. relating to the different sub-group analyses. The value of the findings would be greatly enhanced by probing more systematically using sub-group analyses e.g. within the different regions are there differences in TI by occupation, what are the factors that influence any observed differences?

Thanks for your kind comment!
What you mentioned is true of significant research value, and we also considered it in the process of subgroup analysis. However, due to the limitation and shortage of current studies, it is hard to compare TI by occupation within different regions. The significant heterogeneity among the individual studies evident after the subgroup analysis are performed. So the results of the comparison will be lacking in scientific evidence. Further comparative studies can be carried out when the number of studies increases in the future. We have pointed it out in the limitations and strengths and literature gaps.
“Due to the limitation and shortage of the current studies, it is hard to conduct a further study.”
“Therefore, it can be concluded that there are many facets of the TIs among PHWs that need to be explored. First, the differences in TI by occupation within the different regions or institutions need to be explored.”

10. The presentation could be improved considerably. Paragraphs of mainly statistics are not appealing.
Table 1 is confusing - the row labels need to be revised e.g. Participants - one would expect this to refer to the characteristics of primary health care workers(, Prevalence - assume this is prevalence of turnover?
The text could be improved by adding a brief description of the studies, reporting on subgroup analyses as suggested above. TI may not have differed between doctors and nurses (p9) but do the determinants of TI differ by occupation?
The prevalence should be presented as % (as in the abstract)

Thanks for your detailed suggestion!
We have revised the row labels of Table 1, replaced the word “Participants” with “Research sites”, “Number” with “Sample size”, “Assessment tool” with “TI assessment tool”,” Prevalence” with“ Prevalence of TI”.
As we mentioned above, the composition of PHWs includes doctors, nurses, public health workers and administrative staff, most of them have to play multiple roles. Most studies focused on the overall PHWs, and compared the difference between doctors, nurses and other PHWs within their studies. So it is hard to conduct a subgroup analysis by occupation and observe the difference of the determinants between various occupations.
We have presented the prevalence of TI as % in the text.
Please check the manuscript for more details.

11. The structure of the discussion interrupts with the coherence A reshaping with sub-titles would improve the flow - principal findings, limitations and strengths, comparisons with other studies, implications and conclusions
Overall an interesting paper that could be useful for planners of primary care provision but could be improved with systematic probing relating to the contextual issues that motivates retention/turn over of primary health care workers.

Many thanks for your recognition and kind suggestions!
We accepted your advice and included in the discussion.
We have added three sub-titles to reshape the structure of the discussion:
- Principal findings,
- Limitations and strengths,
- Literature Gaps.

Dear reviewer,
We would like to thank the editor and reviewers for their time and valuable comments. We appreciate that the reviewers found the study well-executed and the methodology innovative. The suggested edits and constructive criticism were invaluable and truly made this version of the paper much better. We sincerely appreciate your suggestions and did our best to incorporate them into the revised manuscript.

Thank you once again for your time and suggestions. We really appreciate it and look forward to hearing from you.

Sincerely,
Ying Mao

The authors performed a comprehensive systematic review, attempting to identify the rate and risk factors of turnover intentions among primary health workers in China. The methods are well-described and the conclusions are supported by the data; however, I have some concerns:

* Abstract
- The abstract should mention statistical values for major outcomes. Also, the recommendation made in the conclusion is quite non-specific and more specific recommendations should be added in light of the study findings.

Thanks for your kind comment!
There are more than twenty factors that were included in this review, limited by the length of the abstract, we cannot mention statistical values for the factors which were significantly associated with turnover intention.
We have added some specific recommendations in the conclusion section.
“This study highlights the problem of turnover intention among PHWs in China. There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intention. Efforts should be made to improve conditions in both work-related areas and areas outside of work. Policymakers should continue to improve reward systems, the construction of infrastructure, and promotion systems and pay more attention to PHWs’ lives outside of work and meet their living needs. Particular attention should be given to PHWs in the community and the eastern region of China.”

* Introduction
- The authors presented their rationale that there is no systematic review on the topic from China. Yet, this is not enough. A rationale for a meta-analysis should identify the controversies among the published studies and the evidence gap in the literature.

Thanks for your kind comment!
We accepted your advice and included it in the introduction.
In addition to there is no systematic review on the topic from China, we also added more rationales for this review.
“In China, many empirical studies have been conducted. However, there is no consistent conclusion on the prevalence and determinates of TI among PHWs in China.”

* Methods
- The authors mentioned they followed PRISMA checklist. For SRs of observational studies, the MOOSE checklist is more appropriate.

Thanks a lot for your kind comment and detailed suggestion!
- We added a MOOSE checklist in the appendix. Please check the manuscript for more details.

- In the abstract, they say they searched four databases, while in the methods, they say five.

Sorry, we made a silly mistake. There are four English language databases (PubMed, EMBASE, Cochrane Library, PsycINFO) we used.

- I the study eligibility, replace "type of intervention" with "risk factor" considering the type of this meta-analysis.

We accepted your advice and replace "intervention" with "risk factor".

- How did the authors ensure that factors are reported and extracted homogenously among the included studies? Was any statistical conversion needed?

First, three authors independently extracted the factors from all included studied. Second, three authors compared their extracted factors, with disagreements resolved by discussion until consensus was reached. There is no statistical conversion.

- How did you modify NOS to make it only 7 points and why?
- Also, what was the NOS scoring based on?

We referenced this modified Newcastle-Ottawa Scale based on the PA Modesti's study [32] which included seven aspects to score. According to the characteristics of these included studies which were carried out in China, we simplified the scoring method, set one score points for each aspects, and so made it only 7 points.
"Specifically, a study with a sample size of less than 1000 was regarded as having poor representativeness of the sample (score = 0, otherwise = 1); a cross-sectional study with a response rate lower than 80% or without reporting a response rate was considered a poor-quality study (score = 0, otherwise = 1). Meanwhile, if statistical methods used in the study was exact, we considered statistical test to be appropriate (score = 1, otherwise = 0), even if there was no further multivariate analysis. Three authors independently scored all included studied, with disagreements resolved by discussion until consensus was reached."

- In the data synthesis section, I believe the authors should make it clear that the prevalence was assessed via single-arm analysis.

We have clarified the prevalence was assessed via single-arm analysis.

- Page 6, the authors mentioned that they analyzed associated factors as log odds ratio, but when I inspected the figure, that does not seem to be the case. Please clarify?

Sorry, we made a mistake. As the data presented in the figure, we identified the association between factors and TI among PHWs in the form of the odds ratio.

- When the authors observed heterogeneity, what was done about it?
- Otherwise, the data synthesis methods are well-described.

Heterogeneity may be due to the presence of one or two outlying studies with results that conflict with the rest of the studies. When observed heterogeneity, first, we conducted a thorough investigation of heterogeneity. We performed the analyses both with and without outlying studies as part of a sensitivity analysis. Based on these results, we tried to exclude studies from a meta-analysis or conduct a subgroup analysis. Second, we performed a random-effects meta-analysis to incorporate heterogeneity among studies.

* Results
- "the 16 included studies was 5.25 of 7 points, indicating a moderate-average quality," What does a moderate-average mean?

Thanks a lot for your kind comment and detailed suggestion!
The "a moderate-average quality" means the average quality of 16 included studies are moderate. We replaced this sentence with "the 16 included studies was 5.25 of 7 points, indicating a moderate research quality,"

- Page 7, line 30: So, the authors mean that small studies inflate the prevalence?

In this part, we presented the difference of TI between vast and small studies to give readers a clearer understanding of the prevalence of TI among Chinese PHWs. But we cannot draw the conclusion that small studies inflate the prevalence due to the heterogeneity.

- Page 7, line 32: What was done to resolve that heterogeneity?

As the question we answered above, we tried to resolve that heterogeneity. But in this section, we cannot conduct a further subgroup analysis due to the limitation and shortage of the current studies.

- In reporting the factors, it is quite confusing to put all these numbers in text. This is what tables are for. Therefore, the authors should report the main numbers and put the detailed numbers in tables.

We have deleted some numbers and only keep the value of OR in text. The detailed numbers were presented in figures.

* Discussion: The authors should discuss how their findings, derived from Chinese studies, can be extrapolated to the world.
- The discussion should also present recommendations for stake holders and policy makers to improve the retention of PHWs and eliminate the obstacles they are facing.

Thanks a lot for your kind comment!
We discussed our findings and also presented recommendations for stakeholders and policy makers in the conclusion section.
“The analysis highlights the problem of turnover intentions among PHWs in China. There is a significant association between demographic factors, job characteristic factors, job satisfaction factors and turnover intentions. Policymakers should take into account all aspects of human needs that influence PHWs’ intentions to stay. As illustrated by the Global Strategy on Human Resources for Health, it is particularly important to find pragmatic solutions to overcome deeply entrenched rigidities in public sector rules and practices that hinder the adoption of adequate reward systems, working conditions and career structures for health workers, with appropriate levels of flexibility and autonomy. Therefore, efforts can be made to improve factors both at work and outside of work. In terms of work factors, policymakers should continue to improve reward systems, the construction of infrastructure, and promotion systems. Outside of work, authorities should pay more attention to PHWs’ lives and meet their living needs to increase their willingness to work and live in communities, towns and villages. We also suggest that particular attention be given to PHWs working in the community or the eastern region of China to reduce their turnover intentions by implementing evidence-based health workforce policies.”
In the discussion section, we also added some further discussion. Please check the manuscript for more details.

* Appendix
- In Appendix Table 1, identify in the table footnote what A1, A2, ... etc refer to among the factors?

Thanks a lot for your kind comment and detailed suggestion!
We identified what A1, A2 ... etc. refer to among the factors in the Appendix Table 1 footnote.
“A1 to A7：seven demographic factors. B1 to B22：twenty-two job characteristic factors. C1 to C18：eighteen Job satisfaction factors.”

**VERSION 2 – REVIEW**

| **REVIEWER** | Chiara Pomare |
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| | Australian Institute of Health Innovation, Macquarie University, Australia |
| **REVIEW RETURNED** | 26-Jul-2020 |

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| **GENERAL COMMENTS** | Thank you for addressing past comments. The manuscript is much improved. Well done on a great paper.<br><br>A few minor points to be addressed:<br>- Please rephrase point 3 of the 'strengths and limitations of this study'. As it stands, the sentence is unclear.<br>- Spacing issue pg 3, line 17<br>- "PHWs working inside include doctors, nurses, public health workers and administrative staff, most of them have to play multiple roles" - please explain what you mean by 'have to play multiple roles'<br>- A justification is warranted as to why a sample of 1000 and response rate of 80% was regarded as acceptable? |

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| **REVIEWER** | Ericson Gutierrez<br>Instituto Nacional de Salud, Perú |
| **REVIEW RETURNED** | 12-Jul-2020 |

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| **GENERAL COMMENTS** | I have no further comments |

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| **REVIEWER** | Mark F Harris<br>University of New South Wales<br>Australia |
| **REVIEW RETURNED** | 11-Jul-2020 |

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| **GENERAL COMMENTS** | The authors have addressed all the points in my previous review. However there are still many grammatical errors. |

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| **REVIEWER** | Abdelrahman Ibrahim Abushouk<br>Beth Israel Deaconess Medical Center - Harvard Medical School |
| **REVIEW RETURNED** | 11-Jul-2020 |

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| **GENERAL COMMENTS** | The authors have adequately addressed my former recommendations. |