

PEER REVIEW HISTORY

BMJ Open publishes all reviews undertaken for accepted manuscripts. Reviewers are asked to complete a checklist review form (<http://bmjopen.bmj.com/site/about/resources/checklist.pdf>) and are provided with free text boxes to elaborate on their assessment. These free text comments are reproduced below.

ARTICLE DETAILS

TITLE (PROVISIONAL)	Job Burnout and Turnover Intention among Chinese Primary Healthcare Staff: The Mediating Effect of Satisfaction
AUTHORS	Ran, Li; Chen, Xuyu; Peng, Shuzhen; Zheng, Feng; Tan, Xiaodong; Duan, Ruihua

VERSION 1 – REVIEW

REVIEWER	Professor R. L. Brown University of Wisconsin - Madison USA
REVIEW RETURNED	28-Jan-2020

GENERAL COMMENTS	<ol style="list-style-type: none">1. Should use the term structural equation modeling instead of path analysis, since SEM typically refers to the use of latent variables and path analysis does not.2. Page 6. The authors discuss that job burnout is categorized as three dimensions, emotional exhaustion, depersonalization, and reduced personalized accomplishment, but seemed to have only modeled a single domain in their model?3. Page 6. The authors should probably indicate that job satisfaction serves as a “mediator” instead of saying “pathway.”4. Page 7. Design and sample section. The authors should indicate that this was a cross-sectional design, since that is a critical piece of design information regarding mediational analyses.5. Page 7. It is unclear regarding sample size. The authors indicate they used random recruitment from the N=1279 healthcare workers? If so, what was the denominator (population)? Why was a random recruitment used? A 100% response rate is very impressive, does this mean no healthcare worker refused or did not complete a survey?6. Page 8. I am baffled regarding “Cronbach coefficient.” I am not familiar with this internal consistency measures and cannot seem to find any statistically related publication defining it. While at first I thought it was a simple misspelling of Cronbach’s alpha coefficient for internal consistency, but I have notice this coefficient published in a number journals. I do think the authors need to provide some detail regarding this coefficient. Also, the authors mentioned this coefficient, but then do not seem to report it?7. Page 9. What was the level of missing data in the items and how was this dealt with?
-------------------------	---

	<p>8. Page 9. The authors indicate in their stages of analysis (1) EFA/CFA, (2) correlational/regression, and (3) path analysis. Technically the authors are using latent structural equation modeling and not path analysis, since they are modeling latent variables of burnout, satisfaction and turnover.</p> <p>9. Page 9. The paper probably needs a table 1 (description of the respondents).</p> <p>10. Page 10. I am not sure I understand why the authors included this multiple regression analysis? Also, if covariates of age, education, and income were important confounders, why were they not brought into the mediation model?</p> <p>11. Page 10. What estimator did the authors use for EFA/CFA and SEM? Was it maximum likelihood or a weighted least squares estimator, or ADF, Bayesian?</p> <p>12. Page 11. Did the authors use a resampling procedure (e.g., bootstrap) for the indirect effect standard errors and CI as suggested by MacKinnon, et al, (2004)? Not that familiar with AMOS, but I think they offered bootstrapping?</p> <p>13. Table 3, I would include confidence intervals instead of p-values.</p> <p>14. Page 14. Conclusion: The authors may wish to refer to partial mediation of job satisfaction, instead of indicating direct effect "or" via mediating, not as clear.</p> <p>MacKinnon, David & Lockwood, Chondra & Williams, J. (2004). Confidence Limits for the Indirect Effect: Distribution of the Product and Resampling Methods. <i>Multivariate Behavioral Research</i>. 39. 99-128. 10.1207/s15327906mbr3901_4.</p>
--	---

REVIEWER	Luis Albendin Andalusian Health Service. La Chana clinical management unit. Spain
REVIEW RETURNED	10-Mar-2020

GENERAL COMMENTS	Congratulate the authors for their magnificent work.
-------------------------	--

REVIEWER	Shannon Ruzycski Cumming School of Medicine, University of Calgary, Canada
REVIEW RETURNED	27-Apr-2020

GENERAL COMMENTS	<p>Major: The authors should indicate if this study was approved by the institution's ethical review board. The authors should indicate if participants were reimbursed for participation, how participants were recruited (e-mail, mailing list, in-person?). Since the study asks sensitive information, such as intention to leave one's job, the authors should state whether the study data was anonymous.</p> <p>The authors should clarify if "all 1,269 healthcare workers" were invited to participate or if there was a "random" sampling of this population, as the statement on page 7, lines 27-29 uses both terms and is contradictory.</p>
-------------------------	---

	<p>The full survey used in this study should be included as an Appendix. The authors should state the total number of items in the survey and whether the survey was completed by paper or electronically. The authors should describe what the maximum and minimum scores are for each subscale, and include the meaningful difference in score, if known. The authors could refer to https://academic.oup.com/intqhc/article/15/3/261/1856193 for more information.</p> <p>How was the turnover intention scale developed? Is this a previously used tool, or did the authors develop this for their study? If developed for this study, the authors should indicate if and how it was pilot tested, how survey items were developed, and report validity testing for this section on its own.</p> <p>The authors should present the characteristics of the healthcare workers, which are described on page 8, lines 31-42, in a Table. The authors should also report the number and percentage of healthcare workers that are nurses, physicians, and other types.</p> <p>If this study included different types of healthcare workers (physicians, nurses, and others) then the results should be stratified by these groups to determine if rates of burnout and turnover intention are different between these groups. The correlations should also be tested in these different groups, because the relationship between turnover intention and burnout may be expected to be different between these different professionals.</p> <p>The description of "Path Analysis" [page 11, lines 19-45] relies very heavily on numbers and statistics. It is very difficult to understand what these results mean beyond what the models have shown numerically. This section would benefit from being rewritten in plainer language, with the numbers to support the written conclusions.</p> <p>The authors should report the prevalence of turnover intention and burnout in their study population, so that readers can determine if their rates are higher or lower than in other settings.</p> <p>The Discussion section should begin with a summary of the results, rather than a restatement of the problem that led to the study.</p> <p>The authors' statement that "The turnover rate would be reduced through enhancing healthcare staff's satisfaction... raising the income level, providing more advanced-learning opportunities... etc" [page 13, lines 9-12] is not supported by their results as presented. The authors could instead highlight how these areas were prevalent in their results and propose these as solutions. Alternately, the authors should present evidence from their study or other literature that support these statements.</p> <p>Table 1 should be rewritten to include the actual item statements. As it stands, the reader has no idea what each item is and cannot understand the table.</p> <p>Table 2 has too many abbreviations. Each Row heading should be written out in full.</p>
--	--

	<p>Figure 2 is not clear - it is not clear what each number and letter represents, and how the reader should interpret this Figure.</p> <p>Minor: The authors' description of the local problem (shortage of healthcare workers) may benefit by comparing to other countries. For example, the number of registered nurses (3 million [page 5, line 31]) by itself does not convince me that there is a shortage. Can the authors compare this to other countries? Is there a recommendation for the number of nurses for China? This would be helpful for most of the data presented in this section. What is the 'staff allocation standard'? [page 5, line 28].</p> <p>What is the turnover rate in other healthcare settings? Is 5.8% and 8-10% high compared to other countries? The authors should cite evidence that high turnover is contributing to the shortage of healthcare workers or otherwise rewrite the sentence "A grossly inadequate amount of healthcare staff has become a social problem cannot be neglected, which is mainly caused by a growing turnover rate" to reflect that this is a hypothesis.</p> <p>The 100% response rate [page 7, lines 29] is a result and should be presented in the Results section.</p> <p>On page 8, lines 32-42, the authors should use the same number of significant digits throughout. The US dollars should be rounded to two decimal places.</p>
--	---

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1

1. Should use the term structural equation modeling instead of path analysis, since SEM typically refers to the use of latent variables and path analysis does not.

Response: Thank you for your kindly reminding. We have revised it in the manuscript.

2. Page 6. The authors discuss that job burnout is categorized as three dimensions, emotional exhaustion, depersonalization, and reduced personalized accomplishment, but seemed to have only modeled a single domain in their model?

Response: Thank you for the question. We explained as follows: (1) Here we evaluated the job burnout using an adjusted scale instead of a standardized scale. So, the number of the items in each dimension decreases. (2) As can be seen in our modified model (Figure 2), there are 4 items involved in job burnout, only covering two aspects — emotional exhaustion and reduced personalized accomplishment, and there is 1 item left in personalized accomplishment after adjusting. (3) In the correlation analysis, there is a strong correlation ($r > 0.7$) between the items in each dimension, that means it is not proper to conduct the SEM by dimensions.

3. Page 6. The authors should probably indicate that job satisfaction serves as a “mediator” instead of saying “pathway.”

Response: Thanks for your suggestion. We have revised it.

4. Page 7. Design and sample section. The authors should indicate that this was a cross-sectional design, since that is a critical piece of design information regarding mediational analyses.

Response: Thank you so much. We have added it to the Design and Sample section.

5. Page 7. It is unclear regarding sample size. The authors indicate they used random recruitment from the N=1279 healthcare workers? If so, what was the denominator (population)? Why was a random recruitment used? A 100% response rate is very impressive, does this mean no healthcare worker refused or did not complete a survey?

Response: (1): sample size: the sample was estimated with the average detection rate of burnout in China (55%) applying the equation: $n = \left[\frac{Z_{\alpha/2}}{\delta} \right]^2 \times p \times (1-p) / \delta^2$, where α is 0.05 and δ is 0.08. Then the sample size was amplified according to the inefficiency of 10%, and at least 540 questionnaires were needed. We added it to the revised manuscript.

(2) response rate: We are sorry for an ambiguous expression. We sent out 1300 questionnaires and every participant responded and filled in it. However, due to uncompleted answers or suspected unreal answers were found in 21 questionnaires, that did not meet the inclusion criteria, so the final sample size was 1279. We revised it in the manuscript.

6. Page 8. I am baffled regarding "Cronbach coefficient." I am not familiar with this internal consistency measure and cannot seem to find any statistically related publication defining it. While at first I thought it was a simple misspelling of Cronbach's alpha coefficient for internal consistency, but I have noticed this coefficient published in a number of journals. I do think the authors need to provide some detail regarding this coefficient. Also, the authors mentioned this coefficient, but then do not seem to report it?

Response: We are sorry for a spelling error. What we mean is Cronbach's alpha coefficient. Because the scales used to evaluate burnout, satisfaction, and turnover intention were adjusted in this study, the validity and reliability should be checked. The Cronbach's alpha coefficient is a method to test internal reliability, and if the value greater than 0.70 is considered satisfactory. Cronbach's alpha coefficients of the three scales were reported in section 3.2.1.

7. Page 9. What was the level of missing data in the items and how was this dealt with?

Response: Sorry for leaving out such important information. As mentioned above, a total of 1300 questionnaires were sent out, and 21 questionnaires with uncompleted answers or suspected unreal answers were deleted directly. We have complemented the content in the manuscript.

8. Page 9. The authors indicate in their stages of analysis (1) EFA/CFA, (2) correlational/regression, and (3) path analysis. Technically the authors are using latent structural equation modeling and not path analysis, since they are modeling latent variables of burnout, satisfaction and turnover.

Response: Thank you so much for your advice. We have modified the improper expression in the full text.

9. Page 9. The paper probably needs a table 1 (description of the respondents).

Response: Thank you so much. We have added a new table (Table 1) to describe the characteristics of healthcare workers.

10. Page 10. I am not sure I understand why the authors included this multiple regression analysis? Also, if covariates of age, education, and income were important confounders, why were they not brought into the mediation model?

Response: Thank you. After careful thought, we decided to delete regression analysis. But we have added age, education, and other variables in the SEM.

11. Page 10. What estimator did the authors use for EFA/CFA and SEM? Was it maximum likelihood or a weighted least squares estimator, or ADF, Bayesian?

Response: Thank you for the question. The parameters in model were estimated by maximum likelihood estimate method. We described it in the Statistical Analysis section.

12. Page 11. Did the authors use a resampling procedure (e.g., bootstrap) for the indirect effect

standard errors and CI as suggested by MacKinnon, et al, (2004)? Not that familiar with AMOS, but I think they offered bootstrapping?

Response: Thank you for the question. The mediation effect was tested by using the bootstrap method. We have added it to the revised manuscript.

13. Table 3, I would include confidence intervals instead of p-values.

Response: Thank you so much. The relevant content of regression analysis has been deleted, but we further talked about the difference between different occupational population and studied the influencing of age, education, income, and other variables in the revised manuscript.

14. Page 14. Conclusion: The authors may wish to refer to partial mediation of job satisfaction, instead of indicating direct effect "or" via mediating, not as clear.

Response: Thank you. The conclusion has been rewritten in the revised manuscript.

Thanks again for your valuable advice.

Reviewer: 2

Congratulate the authors for their magnificent work.

Response: Thank you so much for your approval.

Reviewer: 3

Major:

The authors should indicate if this study was approved by the institution's ethical review board. The authors should indicate if participants were reimbursed for participation, how participants were recruited (e-mail, mailing list, in-person?). Since the study asks sensitive information, such as intention to leave one's job, the authors should state whether the study data was anonymous.

Response: Thank you so much for your reminding. We are sorry for leaving out such an important information. Our study was approved by the Research Ethics Boards of Wuhan University (No.2018YF0080). All participants were recruited face-to-face, and the study data was anonymous. We have added it to the manuscript.

The authors should clarify if "all 1,269 healthcare workers" were invited to participate or if there was a "random" sampling of this population, as the statement on page 7, lines 27-29 uses both terms and is contradictory.

Response: We are so sorry for a vague expression. We have revised it.

The full survey used in this study should be included as an Appendix. The authors should state the total number of items in the survey and whether the survey was completed by paper or electronically. The authors should describe what the maximum and minimum scores are for each subscale, and include the meaningful difference in score, if known. The authors could refer to <https://academic.oup.com/intqhc/article/15/3/261/1856193> for more information.

Response: Thanks for your suggestion. We have revised the manuscript according your advice, and uploaded the questionnaire as an appendix.

How was the turnover intention scale developed? Is this a previously used tool, or did the authors develop this for their study? If developed for this study, the authors should indicate if and how it was pilot tested, how survey items were developed, and report validity testing for this section on its own.

Response: Sorry for leaving out the information. The turnover intention questionnaire here was adjusted with reference to turnover intention scale explored by Griffeth. We added it to the manuscript.

The authors should present the characteristics of the healthcare workers, which are described on page 8, lines 31-42, in a Table. The authors should also report the number and percentage of healthcare workers that are nurses, physicians, and other types.

Response: Thanks for your kindly reminding. We added a table to show the characteristics of HCWs.

If this study included different types of healthcare workers (physicians, nurses, and others) then the results should be stratified by these groups to determine if rates of burnout and turnover intention are different between these groups. The correlations should also be tested in these different groups, because the relationship between turnover intention and burnout may be expected to be different between these different professionals.

Response: Thank you for your advice. The objects of this study are mainly physicians and nurses. The correlation analysis was not conducted by different groups because there was no difference between them. But to test the stability of SEM, we added a multiple-group analysis between the physicians and nurses in the revised manuscript.

The description of "Path Analysis" [page 11, lines 19-45] relies very heavily on numbers and statistics. It is very difficult to understand what these results mean beyond what the models have shown numerically. This section would benefit from being rewritten in plainer language, with the numbers to support the written conclusions.

Response: Thank you for your suggestion. We have simplified the content and tightened the language.

The authors should report the prevalence of turnover intention and burnout in their study population, so that readers can determine if their rates are higher or lower than in other settings.

Response: The prevalence of burnout and turnover intention was added in Table 1 and described in the revised manuscript. Thanks for your suggestion.

The Discussion section should begin with a summary of the results, rather than a restatement of the problem that led to the study.

Response: Thanks for your advice. We have rewritten the discussion.

The authors' statement that "The turnover rate would be reduced through enhancing healthcare staff's satisfaction... raising the income level, providing more advanced-learning opportunities... etc" [page 13, lines 9-12] is not supported by their results as presented. The authors could instead highlight how these areas were prevalent in their results and propose these as solutions. Alternately, the authors should present evidence from their study or other literature that support these statements.

Response: Thanks for your advice. We have rewritten the discussion.

Table 1 should be rewritten to include the actual item statements. As it stands, the reader has no idea what each item is and cannot understand the table.

Response: Thanks so much. We have revised it.

Table 2 has too many abbreviations. Each Row heading should be written out in full.

Response: Thanks so much. We have revised it.

Figure 2 is not clear - it is not clear what each number and letter represents, and how the reader should interpret this Figure.

Response: Thank you. We have tightened our language and explained the main results in Figure 2.

Minor:

The authors' description of the local problem (shortage of healthcare workers) may benefit by comparing to other countries. For example, the number of registered nurses (3 million [page 5, line

31]) by itself does not convince me that there is a shortage. Can the authors compare this to other countries? Is there a recommendation for the number of nurses for China? This would be helpful for most of the data presented in this section. What is the 'staff allocation standard'? [page 5, line 28].
 Response: Thank you so much. We have rewritten this section.

What is the turnover rate in other healthcare settings? Is 5.8% and 8-10% high compared to other countries? The authors should cite evidence that high turnover is contributing to the shortage of healthcare workers or otherwise rewrite the sentence "A grossly inadequate amount of healthcare staff has become a social problem cannot be neglected, which is mainly caused by a growing turnover rate" to reflect that this is a hypothesis.
 Response: Thank you so much. We have rewritten this section.

The 100% response rate [page 7, lines 29] is a result and should be presented in the Results section.
 Response: Thank you so much. We have revised it in the manuscript.

On page 8, lines 32-42, the authors should use the same number of significant digits throughout. The US dollars should be rounded to two decimal places.
 Response: Thank you. We have modified it in the manuscript.

Thanks again for your valuable advice.

VERSION 2 – REVIEW

REVIEWER	Roger Brown University of Wisconsin
REVIEW RETURNED	01-Jun-2020

GENERAL COMMENTS	<p>The authors have done a good job responding to many of my comments, and should be commended for their efforts. I do have just a few more comments.</p> <p>Page 9, lines 28, 37, 44. The chi-square degrees of freedom ratios seems large to me relative to the other fit measures. Is this the ratio or just the chi-square value?</p> <p>Page 10, line 12. This is somewhat correct, but may mislead a reader, since these are standardized estimates. Typical interpretation would be something like, If burnout decreases by one standard deviation, then we expect that satisfaction will increase by 0.406 standard deviations, and this of course, depends on the type of standardization used.</p> <p>Page 11, line 21-24 and Page 12, line 38. I would have liked to seen a little more discussion about what this mediational effect means. Also, while statistically significant indirect effect was found, the mediational effect relative to the total effect was not very substantial, what does that mean?</p>
-------------------------	---

REVIEWER	Shannon Ruzycki Cumming School of Medicine, University of Calgary, Calgary, AB, Canada
REVIEW RETURNED	02-Jun-2020

GENERAL COMMENTS	Thank you for your work in revising this manuscript. You have addressed many of my initial points. In particular, the methods and
-------------------------	---

	<p>results are much improved. The introduction is much more clear and focused.</p> <p>Major: How did the authors determine who had a mental illness for exclusion from their survey (Methods) - I do not see this question in their survey.</p> <p>The authors should clarify how the survey was delivered in addition to stating that participants were recruited face-to-face in the Methods. In the Results, the authors state that "1,300 electronic questionnaires were sent out" even though the participants were recruited face-to-face. The authors should outline this clearly in the Methods - how were the participants recruited (where, and by whom?) and how the participants then completed the surveys.</p> <p>It would be helpful for readers who are unfamiliar with path analysis if the authors could define the significance of the standardized path coefficient in the Results. For example, the authors could state "higher standardized path coefficients suggest stronger correlations, with values over 0.200 considered very correlated".</p> <p>The Discussion should begin with a paragraph outlining the results, answering the question posed in the Introduction. The first paragraph of the Discussion currently contextualizes the result and compares to other literature.</p> <p>The Discussion may be clearer if the authors state which results are from their study and which results are from other literature. For example, instead of "It was additionally found that satisfaction..." the authors could state "Our result also found that..." or "Another study also found that (with citation)" as it is not always clear which result is from the authors' work compared to literature cited in the preceding sentence.</p> <p>Minor: What is the "staff allocation standard" referenced in the Introduction? I am not familiar with this terminology. Is this the goal number of physicians per 1,000 population?</p> <p>The authors should define what each of the occupational titles (junior, etc) are in the Methods section - is this based on years of experience? Is this the same for physicians, nurses, etc?</p>
--	--

VERSION 2 – AUTHOR RESPONSE

Reviewer: 1

(1) Page 9, lines 28, 37, 44. The chi-square degrees of freedom ratios seems large to me relative to the other fit measures. Is this the ratio or just the chi-square value?

Response: Thank you for your question. The chi-square degrees of freedom ratios are sensitive to the sample size, and we have a sample size over 1000 in this study, so the value of χ^2/df would be large. Considering this situation, we optimized the model and χ^2/df decreased.

(2) Page 10, line 12. This is somewhat correct, but may mislead a reader, since these are standardized estimates. Typical interpretation would be something like, If burnout decreases by one standard deviation, then we expect that satisfaction will increase by 0.406 standard deviations, and this of course, depends on the type of standardization used.

Response: Sincerely thanks for your advice. We have changed the expression in the revised manuscript.

(3) Page 11, line 21-24 and Page 12, line 38. I would have liked to see a little more discussion about what this mediational effect means. Also, while statistically significant indirect effect was found, the mediational effect relative to the total effect was not very substantial, what does that mean?

Response: Thank you so much. We have revised the manuscript according to your suggestions.

We are appreciated for your valuable advice. Thank you so much.

Reviewer: 3

(1) How did the authors determine who had a mental illness for exclusion from their survey (Methods) - I do not see this question in their survey.

Response: Thanks for your question. This exclusion criterion is set for ensuring the authenticity of the investigation results. This survey included 29 primary health care institutions in Huangpi District, and we contacted the people in charge before it began, therefore, we have a basic understanding of the physical and mental state of the respondents. Those who had a mental illness were excluded, and the question about mental illness did not list in the questionnaire.

(2) The authors should clarify how the survey was delivered in addition to stating that participants were recruited face-to-face in the Methods. In the Results, the authors state that "1,300 electronic questionnaires were sent out" even though the participants were recruited face-to-face. The authors should outline this clearly in the Methods - how were the participants recruited (where, and by whom?) and how the participants then completed the surveys.

Response: Thank you for your suggestion. The participants were recruited from 29 primary health care institutions in Huangpi District in central China by our research group. Participants fulfilled electronic questionnaires with a mobile application or they orally answered questions and the results were synchronously typed in. We have filled in the details. Thank you so much.

(3) It would be helpful for readers who are unfamiliar with path analysis if the authors could define the significance of the standardized path coefficient in the Results. For example, the authors could state "higher standardized path coefficients suggest stronger correlations, with values over 0.200 considered very correlated".

Response: Thank you so much. We have revised the manuscript according to your suggestions.

(4) The Discussion should begin with a paragraph outlining the results, answering the question posed in the Introduction. The first paragraph of the Discussion currently contextualizes the result and compares to other literature.

Response: Your advice is very helpful in improving the manuscript. Thank you. We have revised the first paragraph in the Discussion section.

(5) The Discussion may be clearer if the authors state which results are from their study and which results are from other literature. For example, instead of "It was additionally found that satisfaction..." the authors could state "Our result also found that..." or "Another study also found that (with citation)" as it is not always clear which result is from the authors' work compared to literature cited in the preceding sentence.

Response: Thanks for your kindly reminding. We have revised it according to your suggestions.

Minor:

(6) What is the "staff allocation standard" referenced in the Introduction? I am not familiar with this terminology. Is this the goal number of physicians per 1,000 population?

Response: Yes, you are right. The staff allocation standard means the goal number, which is a measurement criteria of health resources allocation in China. To make this easier to follow, we have changed the "staff allocation standard" to "goal number" in the revised manuscript. Thank you.

(7) The authors should define what each of the occupational titles (junior, etc) are in the Methods section - is this based on years of experience? Is this the same for physicians, nurses, etc?

Response: Thank you for your suggestion. The occupational titles are basically depended on the years of professional technical qualification and working experiences, but it also depended on the assessment of work performance and scientific research ability in China. Generally, the junior title can be applied when acquiring a medical practitioner qualification, while intermediate title can be applied after working 5 years for bachelor degrees and 2 years for master's degrees. Different medical institutions will formulate corresponding assessment standards on this basis, so, it is complex to define the occupational title in this article. We are appreciated for your advice, but it is a little difficult to do so.

Thanks again for your valuable advice.

VERSION 3 – REVIEW

REVIEWER	Dr. Roger L. Brown University of Wisconsin-Madison, USA
REVIEW RETURNED	31-Jul-2020

GENERAL COMMENTS	The authors have responded to my concerns.
-------------------------	--

REVIEWER	Shannon Ruzycki University of Calgary, Canada
REVIEW RETURNED	05-Aug-2020

GENERAL COMMENTS	Major: Regarding the exclusion criteria of people with a mental illness: I read the authors' reply to me previous comment (How did the authors determine who had a mental illness for exclusion from their survey (Methods) - I do not see this question in their survey.). The authors state that the participant's employers provided medical information on their employees to the study team and then participants were excluded if they had mental illness. The answer is unsatisfactory and does not explain this process. The process for excluding these patients should be clear from the methods section. I also think that this method for excluding participants is unusual and may introduce bias into the results.
-------------------------	---

	<p>The rewriting of the Discussion is much clearer. I appreciate the contribution of this result to the literature.</p> <p>Minor: The authors list four latent measures of job burnout in the Discussion but there appear to be only three (page 10, lines 23-26) listed.</p>
--	---

VERSION 3 – AUTHOR RESPONSE

Reviewer: 1

1.The authors have responded to my concerns.

Response: Sincerely thanks for your approval. We do appreciate for your help.

Reviewer: 3

1.Regarding the exclusion criteria of people with a mental illness: I read the authors' reply to me previous comment (How did the authors determine who had a mental illness for exclusion from their survey (Methods) - I do not see this question in their survey.). The authors state that the participant's employers provided medical information on their employees to the study team and then participants were excluded if they had mental illness. The answer is unsatisfactory and does not explain this process. The process for excluding these patients should be clear from the methods section. I also think that this method for excluding participants is unusual and may introduce bias into the results.

Response: I am sorry that my statement might be vague or ambiguous. In fact, medical staff were interviewed face to face to evaluate their mental symptoms before landing the job, and they had regular psychiatric test with scales. In this study, the criterion is set for ensuring the authenticity, but no participant was excluded due to it. So, we decide to delete this criterion to avoid misunderstanding. We are thankful to your question.

2.The rewriting of the Discussion is much clearer. I appreciate the contribution of this result to the literature.

Response: Thanks for your approval. We sincerely appreciate for your thoughtful help.

3.The authors list four latent measures of job burnout in the Discussion but there appear to be only three (page 10, lines 23-26) listed.

Response: Thanks for your question. We found that the latent measures including no personal accomplishment, indifference, and anxious and fretful were strongly associated with job burnout. So, the original sentence went like this: " For job burnout and its four latent measures, "no personal accomplishment", "indifference", "anxious and fretful" show a strong correlation with burnout except for "a heavy and work"."

VERSION 4 – REVIEW

REVIEWER	Shannon Ruzycki University of Calgary, Canada
REVIEW RETURNED	08-Sep-2020
GENERAL COMMENTS	Thank you for these changes.