PEER REVIEW HISTORY

ARTICLE DETAILS

TITLE (PROVISIONAL)	Burnout and Coping Strategies among Nurses in Malaysia: A National-level Cross-sectional Study
AUTHORS	Binti Zakaria, Nursyahda; Binti Zakaria, Nor Haniza; Bin Abdul Rassip, Muhammad Nur Amir; Lee, Kun Yun

VERSION 1 – REVIEW

REVIEWER	Vinnikov, Denis Kyrgyz State Medical Academy
REVIEW RETURNED	01-Jun-2022

GENERAL COMMENTS	This is a summary of a nationwide study of burnout and coping strategies in a large sample of nurses in Malaysia, both from hospital and outpatient facilities. With pertinent sample size underpinning and adjusted logistic regression analysis, this study showed that of all included variables, only working more night shifts, less sleep and some traumatic occupational event in the past were the strongest predictors of burnout, but not education, age or years in service as studies elsewhere inconsistently showed before. Albeit I believe that the manuscript should reach the stage of publication as it provides useful information for decision makers in healthcare on how to combat burnout, I have a few comments on its methodology and presentation. Dara presentation seems to be too detailed causing poor table readability. Please see my comment for specific tables below. English in this manuscript of fairly good quality, but more effort is needed in proofreading to make it look better for a native speaker. For example, I would point out excessive use of 'the' article. I would strongly advise the authors to ask a native speaker take a deeper look in the style, punctuation and syntax. Burnout predictors selected for this analysis represent a wide range of expected predictors conventionally mentioned in similar studies. However, a deeper look in the literature will show that smoking, alcohol use and even personal health-related quality of life as a surrogate of personal health cab also affect burnout (please see https://link.springer.com/article/10.1186/s13690-019-0345-1 as an example). I regret smoking was not included in the questionnaire. Please discuss this as a limitation and refer to other studies where these were analyzed. Other comments also include the following: Abstract The prevalence and risk factors of what? "Addressing modifiable stressors identified in this study at individual, institutional, 28 and systemic levels will be beneficial to reduce the prevalence of burnout among nurses" – this statement is too strong and has not been tested in this study. It is speculative.

	Materials and methods Although Statistics is described in quite clear way, I feel it would benefit from rewording. The authors may wish to clearly state whet the primary outcomes of interest were, what were the distributions and were there any non-parametric methods used in the univariate comparisons. Income stratification needs explanation. A reader from outside Malaysia would not understand what those abbreviations are. Age and years of service will indeed be collinear; therefore, the authors need to perform multicollinearity tests and then exclude collinear variables from the analysis. Such pairs which come to my mind are age and years in service; night shifts and overall number of shifts and so on. Results Table 3 is indeed overinflated and the authors should think how to more it reader-friendly. What if N estimated population is deleted? Table 4 – needs footnote. What are these? Beta-coefficients? Tables must be self-explanatory. On page 15, I would not devote the entire paragraph to such detailed description of univariate analyses, since adjusted analysis will yield more accurate effects later on in Table 6. Discussion. Paragraph 1 – the last sentence should not be placed here. On page 21 I would not refer to univariate analyses showing that 'double shifts significantly increased the prevalence of overall burnout and its three domains' because this effect was blocked in the adjusted analysis, and that is the entire rationale to adjust for confounders (please see Directed Acyclic Graphs for clarification). As discussed above, please mention and discuss how smoking and alcohol use and even physical activity may affect burnout, and missing these predictors along with health-related quality of life may be a limitation. Moreover, supervisor-employee conflict and work-home conflict were reported as significant predictors of burnout in other occupational groups. Because these were not analysis in the current study, this must be mentioned as a limitation. Moreover, I would advise to add a small paragraph on these burnout predictors (reported in other studies) referring to studies themselves.

REVIEWER	Putra, Kuswantoro Rusca Universitas Brawijaya
REVIEW RETURNED	14-Jun-2022

GENERAL COMMENTS	I found the paper to be overall well written. The study adds new knowledge about burnout and coping strategies among nurses. I still have some comments and suggestions to improve the paper. I have very little confidence in some paper's parts and came away with too many questions to be able to recommend this paper without major revision. I have several significant concerns about some paper´s parts that should be addressed prior to publication: Introduction The title "Burnout and Coping Strategies among Nurses………". This study aims to determine the prevalence of burnout syndrome and its relationship with sociodemographic and professional characteristics as well as examine the coping strategies used by nurses in dealing with stressful conditions in the workplace. The research results are expected by researchers to be able to guide

	the planning and implementation of preventive measures, especially following the immeasurable workload and workload brought on by the COVID-19 pandemic. But the background has not explained the phenomenon and the problem why do these variables need to be investigated? Is there a discrepancy between the title and the research objectives? Therefore, the correction of introduction is needed. Material and Methods This study uses two main groups of nurses from primary care and hospital settings, why use these two research sites with different characteristics? Shouldn't there be a need for different tests on burnout prevalence and coping mechanisms? Results The research data displayed is too much so that it is confusing for the reader. I think it would be better to describe results based on aims Discussion The interpretation reasonably reflects the results. many repetitions of statistical figures from research results in the discussion. Implications for practice, education, and further research are missing. Conclusion The quality of the introduction would be enhanced by describing the final summary of study results.

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Dr. Denis Vinnikov, Kyrgyz State Medical Academy, Al-Farabi Kazakh National University
Comments to the Author:
This is a summary of a nationwide study of burnout and coping strategies in a large sample of nurses in Malaysia, both from hospital and outpatient facilities. With pertinent sample size underpinning and adjusted logistic regression analysis, this study showed that of all included variables, only working more night shifts, less sleep and some traumatic occupational event in the past were the strongest predictors of burnout, but not education, age or years in service as studies elsewhere inconsistently showed before. Albeit I believe that the manuscript should reach the stage of publication as it provides useful information for decision makers in healthcare on how to combat burnout, I have a few comments on its methodology and presentation. Dara presentation seems to be too detailed causing poor table readability. Please see my comment for specific tables below.

Thank you for your time and valuable opinions. We have improved our manuscript based on your comments.

English in this manuscript of fairly good quality, but more effort is needed in proofreading to make it look better for a native speaker. For example, I would point out excessive use of 'the' article. I would strongly advise the authors to ask a native speaker take a deeper look in the style, punctuation and syntax.

We thank the reviewer for pointing this out. We have taken efforts to rephrase some of the sentences. The revised paper has also been edited by a proofreader.

Burnout predictors selected for this analysis represent a wide range of expected predictors conventionally mentioned in similar studies. However, a deeper look in the literature will show that smoking, alcohol use and even personal health-related quality of life as a surrogate of personal health cab also affect burnout (please see https://link.springer.com/article/10.1186/s13690-019-0345-1 as an example). I regret smoking was not included in the questionnaire. Please discuss this as a limitation and refer to other studies where these were analyzed.

We agree with the viewpoint of the reviewer. In fact, smoking and alcohol use were part of our questionnaire. However, the prevalence obtained was very low (<0.1%), likely attributed to the fact that our study population is predominantly female Malay nurses of Islam religion. We have explained this in the revised manuscript. Please see P17 L33 – P18 L5.

Other comments also include the following:
Abstract
The prevalence and risk factors of what?
"Addressing modifiable stressors identified in this study at individual, institutional, 28 and systemic levels will be beneficial to reduce the prevalence of burnout among nurses" – this statement is too strong and has not been tested in this study. It is speculative.

Thank you for your opinion. We have rephrased the sentence to "By addressing modifiable stressors of burnout at individual and institutional levels, it can be potentially beneficial to reduce the prevalence of burnout among nurses". (P2 L25-26)

Materials and methods
Although Statistics is described in quite clear way, I feel it would benefit from rewording. The authors may wish to clearly state whet the primary outcomes of interest were, what were the distributions and were there any non-parametric methods used in the univariate comparisons.

The primary outcomes of interest included the prevalence of overall burnout and its three domains (EE, DP, and PA). We have added it in the Method Section (P6 L36-37).

With the large sample size and the complex sampling design, normality distribution can be assumed based on the Central Limit Theorem.

Income stratification needs explanation. A reader from outside Malaysia would not understand what those abbreviations are.

Thank you for highlighting this. Definitions were part of the footnote of Table 2 in the original manuscript. However, we have included the definitions of income classifications under the Method section in the revision (P6 L12-16).

Age and years of service will indeed be collinear; therefore, the authors need to perform multicollinearity tests and then exclude collinear variables from the analysis. Such pairs which come to my mind are age and years in service; night shifts and overall number of shifts and so on.

Our apologies for not outlining the details in the original manuscript. We have added the findings of the multicollinearity tests. (P6 L11-14)

Results
Table 3 is indeed overinflated and the authors should think how to more it reader-friendly. What if N estimated population is deleted?

We agree with your view. We have taken the steps to update Table 3 (now renamed Table 2) to improve the readability. (P10)

Please note that Tables 2 and 3 in the original manuscript have been combined to reduce the number of Tables in our revised manuscript.

Table 4 – needs footnote. What are these? Beta-coefficients? Tables must be self-explanatory.

Footnotes have been added to Table 4 (now renamed Table 3). (P12 L6)

On page 15, I would not devote the entire paragraph to such detailed description of univariate analyses, since adjusted analysis will yield more accurate effects later on in Table 6.

This observation is fair. We have modified the relevant sections. (P12 L8-12)

Discussion.
Paragraph 1 – the last sentence should not be placed here.

We appreciate your opinion but we would like to retain the last sentence to explain the potential differences between the prevalence of burnout in different study settings (population selected, tools used). (P16 L16-18)

On page 21 I would not refer to univariate analyses showing that 'double shifts significantly increased the prevalence of overall burnout and its three domains' because this effect was blocked in the adjusted analysis, and that is the entire rationale to adjust for confounders (please see Directed Acyclic Graphs for clarification).

We have amended the sentences to better reflect the results of the multivariate analysis. (P18 L8-10)

As discussed above, please mention and discuss how smoking and alcohol use and even physical activity may affect burnout, and missing these predictors along with health-related quality of life may be a limitation.

We have acknowledged this issue in the revision. Please see P17 L33 – P18 L5.

Moreover, supervisor-employee conflict and work-home conflict were reported as significant predictors of burnout in other occupational groups. Because these were not analysis in the current study, this must be mentioned as a limitation. Moreover, I would advise to add a small paragraph on these burnout predictors (reported in other studies) referring to studies themselves.

We appreciate the reviewer's insightful suggestion. Sadly, interpersonal and organisational level predictors were beyond the scope of our paper. Nevertheless, we recognize this limitation should be mentioned in the paper, so we have edited the last section of the Discussion. (P20 L3-7)

Reviewer: 2
Dr. Kuswantoro Rusca Putra, Universitas Brawijaya
Comments to the Author:
I found the paper to be overall well written. The study adds new knowledge about burnout and coping strategies among nurses. I still have some comments and suggestions to improve the paper. I have very little confidence in some paper's parts and came away with too many questions to be able to recommend this paper without major revision.
I have several significant concerns about some paper´s parts that should be addressed prior to publication:

Thank you for taking the time to review our manuscript. We appreciate your kind feedback and made the necessary amendments to improve our manuscript.

Introduction
The title "Burnout and Coping Strategies among Nurses………". This study aims to determine the prevalence of burnout syndrome and its relationship with sociodemographic and professional characteristics as well as examine the coping strategies used by nurses in dealing with stressful conditions in the workplace. The research results are expected by researchers to be able to guide the

planning and implementation of preventive measures, especially following the immeasurable workload and workload brought on by the COVID-19 pandemic. But the background has not explained the phenomenon and the problem why do these variables need to be investigated? Is there a discrepancy between the title and the research objectives?

Therefore, the correction of introduction is needed.

We acknowledge the lack of mention of other aspects of relevant study variables besides burnout in the original manuscript. We have amended the Introduction section to provide more information about coping strategies and the rationale behind the selection of variables in our study. Please refer to all the amendments under 'Track Change' in the Introduction. (P4-5)

Material and Methods

This study uses two main groups of nurses from primary care and hospital settings, why use these two research sites with different characteristics? Shouldn't there be a need for different tests on burnout prevalence and coping mechanisms?

As our study participants of interest are nurses working in the public healthcare system, we included all nurses in the hospital and primary care clinics. All these nurses are trained and graduated from public-funded nursing colleges. They also have the flexibility to be transferred between the two settings. More importantly, it is our aim to estimate the national prevalence of burnout, thus we feel both groups of nurses should be included in the analysis. We would also like to add that no significant differences were observed between the baseline characteristics of the two groups of nurses.

Results

The research data displayed is too much so that it is confusing for the reader. I think it would be better to describe results based on aims

We agree with your view. We have taken the steps to update the tables to improve their readability. Table 2 which originally reported the overall prevalence has been incorporated into Table 3 (now renamed Table 2). (P10-11)

The results section has also been updated to focus on the prevalence of burnout, correlation with coping strategies, and the predictors of burnout, all of which were the outcomes of interest based on our study aims.

Discussion

The interpretation reasonably reflects the results. many repetitions of statistical figures from research results in the discussion. Implications for practice, education, and further research are missing.

Thank you for your observation. We have amended the last section of the Discussion to address this issue. Implications for practice, education, and future research have been highlighted in P19 L19-31.

Conclusion
The quality of the introduction would be enhanced by describing the final summary of study results.

We have taken the necessary steps to enhance the conclusion. (P20)

Our sincere gratitude to both reviewers again for their time and comments. We hope the Editor will find our revised manuscript to be worthy of publication in BMJ Open. Thank you.

VERSION 2 – REVIEW

REVIEWER	Vinnikov, Denis Kyrgyz State Medical Academy
REVIEW RETURNED	18-Aug-2022

GENERAL COMMENTS	I believe that the effort made to improve the manuscript is sufficient. The manuscript has significantly improved with this revision.