Author’s response to reviews

Title: A Cross-Sectional Study Exploring the Relationship Between Burnout, Absenteeism, and Job Performance among American Nurses

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Author’s response to reviews:

Tovah Honor Aronin, Ph.D.
BioMed Central, USA
Editor
BMC Nursing

Dear Dr. Aronin,

Thank you for your provisional acceptance of our manuscript.

Thank you for your review of our manuscript. We appreciate yours and the reviewers’ comments and we have responded to the suggestions as detailed in the response to the reviewers.

We believe the revisions have strengthened our manuscript and resubmit it for your review. If you need any additional information please do not hesitate to contact me.

Thank you very much for your consideration.

Sincerely yours,

Pamela O. Johnson, MS, RN, NEA-BC, Mayo Clinic Chief Nursing Officer
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Reviewer reports:
Karen Dominguez-Cancino, MPH, BSN, RN (Reviewer 1): I appreciate that the researcher team had considered all the observations made. The manuscript is more clear, mostly in the variable definition and management. And you decreased the numbers of references that past the 5 years of publication to 33.8%, which is better and also they explain why is so high in the manuscript.

Response: We thank the reviewer for this comment.

However, I suggest they analyse as a team the following observations:
- Page 6 line 119, you have to give the values of validity and reliability as you did with PRIME MD instrument. This is applicable to all instrument used.

Response: We provide the sensitivity and specificity of the PRIME MD as responses on the measure can be compared to meeting DSM V criteria for depression as determined by a mental health provider (the gold standard). In a small sample of workers in the Netherlands the sensitivity and specificity of the Maslach Burnout Inventory for detecting “work-related neurasthenia” (according to the ICD-10 criteria), as determined by a psychiatrist, was 70% and 64%, respectively. The sensitivity and specificity of the Maslach Burnout Inventory for detecting “burnout” (according to the ICD-11 criteria), as determined by a psychiatrist or other mental health professional, is unknown. This is in part due to the fact that the ICD-11 definition comes several decades after the development of the MBI as the instrument best aligned with the construct of burnout as defined by Maslach, Jackson, and Leiter. Hence, we are unable to provide sensitivity and specificity of the MBI for detecting burnout. Factor structure and other evidence of validity have, however, been extensively published and the reader is directed to relevant references.

With respect to the HPQ, previous validation studies in US workers have demonstrated significant associations between HPQ scores and payroll records of absenteeism and job performance assessments by supervisors and other records (receiver operating characteristic curves of 0.58-0.72 in US workers). The HPQ has been used widely in samples of workers, although not specifically in nurses. Additional information can be found at: https://www.hcp.med.harvard.edu/hpq/index.php. We have added information about validity of the HPQ to page 8, line 153-158.

- About the missing data, is not correct to run analysis with missing data, or you imputed or you delete the person of the data base.

Response: Responders who were missing data for variables of interest (burnout, satisfaction, etc.) were excluded from those particular analyses. Data were not imputed for this analysis.
- Why they did bivariate analysis between burnout and work performance and not between burnout and absenteeism? they have to explain.

Response: The bivariate analysis between burnout and absenteeism is presented in line 211-212 and shown in Table 2.

- If the objective was to explore the relationship between burnout with absenteeism and work performance, the results has to be redacted in that line. The team shows the relationship between absenteeism and fatigue when they literally explain that fatigue and depression were confusion variables described in the literature, so the results can’t be directed in that way. They have to be explicit that in the adjusted model with absenteeism as a predictor, they didn't find a statistical significant association adjusted by fatigue, age, sex and work hours per week. This last variables were the only real adjustment variables because they stay when you run forward and backwards stepping analysis for the logistic regression, not the all set of variables, independently that they had to force some variables in the model (which was good to explain). If the model held other variables when you made this analysis, they have to include these one’s in the table 3. You can add then that the variables fatigue and work hour per week were statistical significant in the model, as you expected to be, because these variables are adjusted variable that don't answer your real question. The same thing has to be done with the Poor Work Performance result.

Response: We have clarified the text of the multivariable results to report adjustment in Table 3 for only the listed variables from the model selection process. We believe this addresses the reviewer’s concern, but if we have misunderstood we would welcome additional clarification.

- You also have to explain why you forced the other variables age and sex, the most of the time we say that they keep in the model because are traditional confounders.

Response: We have changed the text in line 180-181 and the footnote to Table 3 to indicate that age and sex were kept in the model because they are traditional confounders, as suggested by the reviewer.

- In the table 3, the fatigue has to be present with the same name

Response: We have made this correction

- You have to correct the following references: 8, 39, 40, 59. They don't have the correct format.

Response: We have corrected the references.

Nina Granel Gimenez, PhD, MsC, RN (Reviewer 2): After reviewing this version and evaluating again, I believe that the points raised in my previous review were adequately addressed. The manuscript is now substantially improved.

Response: We thank the reviewer for this comment.
Lori A. Loan, PhD, RN (Reviewer 3): Thank you for the opportunity to review this manuscript. It is well-written in a level appropriate to this journal's readership. The topic is timely, and the subject is important.

The paper title and purpose are appropriate and congruent, and well portrayed in the text. Content is of high quality and depicted in a scholarly manner.

Response: We thank the reviewer for these comments.

The literature review seems to end in 2016 (line 70) and misses several relevant reports of studies relating nurse burnout and outcomes published within the past 3 years.

Response: We have updated our literature search and added several recent articles to the citations on line 65 and 67. We were unable to find additional articles that explored the relationship between nurse burnout and absenteeism or ratings of work performance, but would be pleased to cite any such literature we may have overlooked.

The paper and findings are strengthened by use of standardized survey instruments however validity of the HPQ when used for evaluating nursing work performance and the study's delineation of cutoff scores determining low performance do not appear to be evidence based for nursing populations. Justification with the inclusion of references supporting these decisions must be incorporated.

Response: The HPQ has not been validated specifically in nursing populations. Justification for the cut-offs can be found in references 37, 38, and 42 with supporting statements located on lines 147-150.

At the end of the abstract (line 52) and in the body of the manuscript in lines 68, 252 and 298, authors use the term "productivity" as if it is an outcome measured in the study, however the study was not designed to measure or imply productivity. The manuscript provides no definition for this term (which usually refers to measure of the efficiency of a person in converting inputs to useful outputs) and the conceptual relationship between study measures and productivity are not described. The phrase "impact of nurse burnout on productivity, such as absenteeism and work performance, which could add strain the adequacy of the workforce in an area of nurse work force shortage" is not supported by reference 25. Referring to study outcomes as including or implying productivity should be removed.

Response: We have removed the word ‘productivity’ throughout the manuscript. See line 52, 67-69, 245, and 291.

In the analysis write up I did not see mention of checking for multicollinearity among variables. Were depression and burnout highly correlated? If yes, how was this handled in your analysis? This should be discussed in the analysis section.

Response: There were no collinearity “flags” in the statistical analyses. In response to the reviewer’s question, we ran collinearity diagnostics on the independent predictors and found
relatively small ViFs (variance inflation factors) ranging from 1.01 (gender) to 1.43 (age). We suspect that the large sample size provided separation of moderately correlated variables such as depression and burnout, allowing analysis without limiting collinearity.

Rather than list the study’s sample as a strength, it should be included in the weaknesses. Regardless of the sample size, the sample does not well represent the U.S. nurses. For example, approximately 25% of respondents worked in inpatient settings when evidence supports that approximately 60% of nurses work in inpatient settings. The sample characteristics for BSN saturation and education are also not close to government reports of U.S. nurse population statistics.

Response: We appreciate the reviewer’s note about differences in practice setting – of note, practice setting was not found to be associated with the outcomes of interest in this study, and did not confound the reported results. In our view the sample of &gt;800 nurses from diverse practice settings is a strength. Our responders were fairly typical of US nurses with respect to age, sex, highest academic degree related to nursing, and work hours and the prevalence of burnout in this cohort was similar to that found in previous studies of nurses. However, due to the reviewer’s concern, we have removed the statement about the sample’s strength from line 279-280.

Consideration of the previous comments and associated improvements will expand ability of the paper to provide helpful insights about nurse burnout and expand on baseline evidence supporting future research.

Response: We thank the reviewer for this comment.