PEER REVIEW HISTORY

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ARTICLE DETAILS

TITLE (PROVISIONAL)	Indicators of technostress, their association with burnout and the moderating role of support offers among nurses in German hospitals: a cross-sectional study
AUTHORS	Wirth, Tanja; Kräft, Jessica; Marquardt, Berit; Harth, Volker; Mache, Stefanie

VERSION 1 – REVIEW

REVIEWER	Shachak, Aviv University of Toronto, Health policy, Management and Evaluation
REVIEW RETURNED	10-Apr-2024

GENERAL COMMENTS	Thank you for the opportunity to review this manuscript. This study examined the association between indicators of technostress and nurses' burnout, and the moderating effect of employer support on the association. A cross sectional online survey of 303 nurses in Germany was employed using pre-validated scales and some free-text questions. There was no difference in technostress indicators between nurses with and without managerial roles. Two indicators of technostress were associated with burnout, and support from employers moderated the effect. Overall, the study is very comprehensive, well designed and executed. My comments below are mostly meant to help better present it and provide a more fulsome background for the study. Main comments: 1) Since burnout is one of the main variables in the study, it is essential to include more information about it in the background. Let the readers know why nurses burnout is a problem and tell them what is known about its implications and causes (similar to what you did for technostress in the "State of Research" section. As an example, you may see the introduction of our paper re physician burnout (this is NOT an expectation that you cite it): Bahr et al. Technostress as source of physician burnout: An exploration of the associations between technology usage and physician burnout. Int J Med Inform. 2023 Sep;177:105147. doi: 10.1016/j.ijmedinf.2023.105147. Epub 2023 Jul 8. PMID: 37517300. 2) Since support from employers is a major moderating factor, it would be good to tell the readers what is known about it. To my knowledge, it is often mentioned in the literature on implementation of health information technology (especially EHRs) but not so much in the context of mitigating burnout. As well, there are some theoretical frameworks of end-user support (including one that we developed). I'm not sure if they are relevant and there may be

	newer frameworks but you can check and compare to them in the discussion IF APPLICABLE. Munkvold R. End-user support usage. In: Gordon S (ed). Computing Information Technology: The Human Side. Hershey, PA: Idea Group, 2003, pp. 146–60. Shachak A, et al. Understanding end-user support for health information technology: a theoretical framework. Inform Prim Care. 2011;19(3):169-72. doi: 10.14236/jhi.v19i3.810. PMID: 22688226.. Minor comments: 3) Abstract: "validated scales" is too little and too vague. It would help if you specified the scales 4) In the methods, you mention 6 items of CBI/COPSOQ. However, each of these instruments contains more than 6 items. Can you please clarify which items you selected and why? Relatedly, it would be useful to include the full survey as an appendix (or supplementary material) 5) It would be useful to the reader to put the 5 indicators of technostress and their definitions in a table. It may also help with word count. 6) Could you please provide an interpretation for the burnout scale? For example, when you say "average burnout score of 49.86 ±19.90"- out of what? Is it that the higher the score the greater the burnout? 7) "hypothesis was confirmed" repeats several times in the manuscript. From a Philosophy of Science perspective, hypotheses cannot be 'confirmed'- only rejected or 'supported'

REVIEWER	Bockerman, Petri 09-May-2024
REVIEW RETURNED	

GENERAL COMMENTS	Comments 1. The sample size of the study is small (N=303 nurses). Does the empirical analysis that is conducted in the manuscript suffer from poor statistical power? There is (only) a short discussion of this (important) concern on page 7. (See also point 8 below.) 2. Cross-sectional data limits severely ability to make causal conclusions about the links between technostress, burnout, and support offers. My view is that causal studies are need for practical policy implications. 3. The data are not (nationally) representative of all German nurses. The sample had overrepresentation from certain federal states and from nurses with German as their mother tongue. Nurses with leadership positions are also disproportionately represented in the data. These concerns are crucial for the external validity of the results that are reported in the manuscript. (See also point 9 below.) 4. Due to the online distribution method of the survey that was conducted (page 7), it was (unfortunately) not possible to document a response rate in the manuscript. This limitation makes

	it really challenging to evaluate how representative the (final) study sample is of the intended target population. 5. Although validated scales are applied to capture/measure technostress and burnout, there might be limitations related to self-report bias. To address this concern, there are earlier studies that have used linked survey and register data. (See also point 7 below.) 6. My view is that the revised manuscript should pay more attention to the quantitative size of the estimated effects. The authors could state whether the quantitative size of the (main) estimates is plausible or not, based on theory and earlier empirical evidence. 7. An important concern is that employees/nurses are not randomly assigned into workplaces/job tasks, which lead to biased estimates. The issue should be noted in the revised manuscript. 8. The manuscript does not evaluate the (potential) heterogeneity in the estimated relationships. My view is that the relationships differ arguably significantly e.g., by nurse's age. Technostress is most likely a much more serious concern for the older nurses compared to young nurses who have grown up with various ICT applications from an early age. The small sample size ((N=303 employees) limits meaningful analysis of these links. 9. What is the external validity of the estimation results that are presented in the manuscript for other high-income countries/contexts than Germany?

VERSION 1 – AUTHOR RESPONSE

Reviewer: 1
Dr. Aviv Shachak, University of Toronto
Comments to the Author:
Thank you for the opportunity to review this manuscript. This study examined the association between indicators of technostress and nurses' burnout, and the moderating effect of employer support on the association. A cross sectional online survey of 303 nurses in Germany was employed using pre-validated scales and some free-text questions. There was no difference in technostress indicators between nurses with and without managerial roles. Two indicators of technostress were associated with burnout, and support from employers moderated the effect. Overall, the study is very comprehensive, well designed and executed. My comments below are mostly meant to help better present it and provide a more fulsome background for the study.

Main comments:
1) Since burnout is one of the main variables in the study, it is essential to include more information about it in the background. Let the readers know why nurses burnout is a problem and tell them what is known about its implications and causes (similar to what you did for technostress in the "State of Research" section. As an example, you may see the introduction of our paper re physician burnout (this is NOT an expectation that you cite it):
Bahr et al. Technostress as source of physician burnout: An exploration of the associations between technology usage and physician burnout. Int J Med Inform. 2023 Sep;177:105147. doi: 10.1016/j.ijmedinf.2023.105147. Epub 2023 Jul 8. PMID: 37517300.
Author's response: Following your suggestion, we have included a description of the prevalence, consequences and causes of burnout among nurses in the "State of Research" on p. 5.

2) Since support from employers is a major moderating factor, it would be good to tell the readers

what is known about it. To my knowledge, it is often mentioned in the literature on implementation of health information technology (especially EHRs) but not so much in the context of mitigating burnout. As well, there are some theoretical frameworks of end-user support (including one that we developed). I'm not sure if they are relevant and there may be newer frameworks but you can check and compare to them in the discussion IF APPLICABLE.
Munkvold R. End-user support usage. In: Gordon S (ed). Computing Information Technology: The Human
Side. Hershey, PA: Idea Group, 2003, pp. 146–60.
Shachak A, et al. Understanding end-user support for health information technology: a theoretical framework. Inform Prim Care. 2011;19(3):169-72. doi: 10.14236/jhi.v19i3.810. PMID: 22688226.
Author's response: Thank you for your recommendations on possible frameworks of end-user support. We have added a section on support offers in our theoretical background (p. 4), where we use a definition from the framework and expand it with the relevant health context.

Minor comments:
3) Abstract: "validated scales" is too little and too vague. It would help if you specified the scales
Author's response: We have revised the abstract accordingly and added the specific scales.

4) In the methods, you mention 6 items of CBI/COPSOQ. However, each of these instruments contains more than 6 items. Can you please clarify which items you selected and why? Relatedly, it would be useful to include the full survey as an appendix (or supplementary material)
Author's response: Perhaps we have expressed this in a misleading way. We used the subscale on personal burnout of the CBI. A German version of this personal burnout subscale is included in the COPSOQ and we have used this German version. Both the English original and the German version consist of six items. We did not exclude any items. We have slightly revised the relevant passage (p. 7). Additionally, we have included an overview of the scales and items of the questionnaire as supplemental information (Supplementary Table A).

5) It would be useful to the reader to put the 5 indicators of technostress and their definitions in a table. It may also help with word count.
Author's response: Thank you for this suggestion. We understand that this could contribute to a better overview for the reader. However, there is no room for further tables in the manuscript according to the journal. Therefore, such a table would have to be included in the supplement. However, we would like to describe this important information centrally in the manuscript and would therefore like to leave it in the text.

6) Could you please provide an interpretation for the burnout scale? For example, when you say "average burnout score of 49.86 ±19.90"- out of what? Is it that the higher the score the greater the burnout?
Author's response: Thank you for the comment. We have added information on the interpretation of the scale value in the methods section (p. 7).

7) "hypothesis was confirmed" repeats several times in the manuscript. From a Philosophy of Science perspective, hypotheses cannot be 'confirmed'- only rejected or 'supported'
Author's response: We have replaced "confirmed" with "supported" in the relevant passages (pp. 10, 11 and 13).

Reviewer: 2
Dr. Petri Bockerman, Labour Institute for Economic Research, Helsinki
Comments to the Author:
Comments
1. The sample size of the study is small (N=303 nurses). Does the empirical analysis that is

conducted in the manuscript suffer from poor statistical power? There is (only) a short discussion of this (important) concern on page 7. (See also point 8 below.)
Author's response: We carried out a sample size calculation before conducting the study and recruiting participants, which resulted in a required sample size of N = 273. You can find the description on p. 6 (section "Study design and data collection"). We have achieved this required sample size. We are now making this point more clearly in the discussion on strengths and limitations (p. 14). For the empirical analyses such as the multiple regression analysis with six predictors, the sample size can be considered sufficient, so that low statistical power cannot be assumed (e.g. Schneider et al. 2010, doi:10.3238/arztebl.2010.0776).

2. Cross-sectional data limits severely ability to make causal conclusions about the links between technostress, burnout, and support offers. My view is that causal studies are need for practical policy implications.
Author's response: Thank you for this comment. We are aware of the limitations of the cross-sectional data with regard to causal relationships and are discussing them on p. 14 (section "Strengths and limitations"). Unfortunately, it was not possible for us to collect longitudinal data for various reasons. We did not intend to derive any policy implications from this study. To make this clearer in the manuscript, we have made some corresponding additions (see section "Implications for practice" on p. 14 and section "Implications for future research" on p. 15).

3. The data are not (nationally) representative of all German nurses. The sample had overrepresentation from certain federal states and from nurses with German as their mother tongue. Nurses with leadership positions are also disproportionately represented in the data. These concerns are crucial for the external validity of the results that are reported in the manuscript. (See also point 9 below.)
Author's response: It was not our intention to obtain a representative sample for all German nurses. But we agree with you and are aware of this limitation. We have now specified this limiting factor in more detail on p. 14 (section "Strengths and limitations").

4. Due to the online distribution method of the survey that was conducted (page 7), it was (unfortunately) not possible to document a response rate in the manuscript. This limitation makes it really challenging to evaluate how representative the (final) study sample is of the intended target population.
Author's response: We also agree with you on this point. Calculating the response rate is very often a difficulty with online surveys that are widely distributed. We are aware that this is a limitation and therefore name it accordingly on p. 14 (section "Strengths and limitations").

5. Although validated scales are applied to capture/measure technostress and burnout, there might be limitations related to self-report bias. To address this concern, there are earlier studies that have used linked survey and register data. (See also point 7 below.)
Author's response: In Germany, it is very difficult to obtain and use registry data due to restrictive access to health-related data and data protection regulations, so it would not have been practical for this study. But, of course, we understand your concerns regarding self-reports of participants and have now included this aspect in our discussion on strengths and limitations (see p. 14).

6. My view is that the revised manuscript should pay more attention to the quantitative size of the estimated effects. The authors could state whether the quantitative size of the (main) estimates is plausible or not, based on theory and earlier empirical evidence.
Author's response: Thank you for your suggestion. We have revised the discussion and included a comparison of the quantitative size of our standardised regression coefficients with those found in other studies (see p. 14).

7. An important concern is that employees/nurses are not randomly assigned into workplaces/job tasks, which lead to biased estimates. The issue should be noted in the revised manuscript.
Author's response: It is due to the study design that randomly assignment of nurses to job tasks was not applicable and it rather represents a general limitation of the design of an observational study. Conducting an interventional/ experimental study was not an appropriate option with regard to our research questions and hypotheses. Additionally, such a design would have been associated with other limitations such as artificial situations and probably also restricted generalisability.

8. The manuscript does not evaluate the (potential) heterogeneity in the estimated relationships. My view is that the relationships differ arguably significantly e.g., by nurse's age. Technostress is most likely a much more serious concern for the older nurses compared to young nurses who have grown up with various ICT applications from an early age. The small sample size ((N=303 employees) limits meaningful analysis of these links.
Author's response: We cannot confirm this assumption. Systematic reviews found no linear trends between age and technostress among employees (Berg-Beckhoff et al. 2017, doi:10.1080/10773525.2018.1436015; Rohwer et al. 2022, doi:10.3390/ijerph19063625). Some studies reported that older employees experienced less technostress (Gimpel et al. 2018, https://www.boeckler.de/de/faust-detail.htm?sync_id=HBS-007024, Ragu-Nathan et al. 2008, doi:10.1287/isre.1070.0165). Previous studies among health care professionals also showed inconsistent results regarding age and technostress (Golz et al. 2021, doi:10.2196/31408; Tell et al. 2023, doi: 10.3390/healthcare11141988). Nevertheless, we considered the variables sex and age as control variables in the multiple linear regression model. However, there was no substantial change in standardised beta coefficients of the predictors with these control variables in the model. You can find the related description on p. 8.

9. What is the external validity of the estimation results that are presented in the manuscript for other high-income countries/contexts than Germany?
Author's response: As described in response to your point 3, the external validity is limited due to the non-representative sample. In the discussion, however, we emphasise the comparison with studies from Germany and other high-income countries (pp. 13 and 14). Furthermore, we have specified this limiting factor on p. 14 (section "Strengths and limitations").

VERSION 2 – REVIEW

REVIEWER	Shachak, Aviv University of Toronto, Health policy, Management and Evaluation
REVIEW RETURNED	04-Jun-2024

GENERAL COMMENTS	Thank you for addressing all of my comments. The revised manuscript is improved and I have no additional concerns.

REVIEWER	Bockerman, Petri Labour Institute for Economic Research, Helsinki
REVIEW RETURNED	03-Jun-2024

GENERAL COMMENTS	I am happy with the revised paper / responses.