Article details: 2022-0191
Title: The relationship between the level of (and changes in) perceived workplace support and mental health, wellbeing and burnout in healthcare professionals (HCP) during the COVID-19 pandemic: insight and mitigating strategies from the CoPE-HCP cohort study
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Reviewer 1: Lindsay Hedden
Institution: Faculty of Health Sciences, Simon Fraser University
General comments (author response in bold)

This paper reports on associations between self-reported measures of support from various sources and mental health and well-being variables in surveys at two time points conducted with health care workers and non-HCW academics during the COVID-19 pandemic.

The design and results provide an incremental advance on a number of cross-sectional studies that have demonstrated that perceived support and mental health outcomes are correlated in this context. The concept that is being tested here is an important one – that support, especially that provided by organizations and by individuals within organizations at a managerial level, is a useful intervention to mitigate the adverse impact of doing healthcare work during the pandemic. Rhetorically, this idea is an important counter-point to an over-emphasis on individual “resilience” which has been prevalent, especially in the early parts of the pandemic.

That said, this design of the current study is only modestly more robust than a cross-sectional survey and the reporting of the study should be clearer on the limitations of this design to support a causal hypothesis. In particular, these limitation require greater attention:

Self-report instruments measure perceived support, not objectively available support. A person with higher levels of distress, depression or burnout could be reasonably expected to perceive that a given “amount” of support is less effective (because their needs are greater) than the same amount of support provided to a person with less distress, who might find it fully adequate. Thus, measuring both support and mental health by self-report makes it hard or impossible to distinguish the direction of influence in an association of perceived support and psychological difficulty.
We thank the Reviewer for this valuable point. We agree that reverse causality is a potential limitation of this study design. We also agree that our measure of perceived support is inherently subjective. We have modified the analysis for ‘change in support’ (now detailed on page 7 line 30) which we believe is a more valid assessment of change in perceived support. Moreover, it must be noted that we do not frame our support items in the context of mental health, so we believe that participants will generally provide their assessment of support based on their observations in the workplace (irrespective of their mental health status).
We accept that individuals with higher depression symptoms (for example) may have more negative perceptions of the workplace, compared to individuals with lower depression symptoms. This confounder is unavoidable without the

implementation of a intervention design. As such, we have toned down our language regarding causality in this manuscript.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 12 line 21 - We have now included the potential issue of reverse causality in our limitations.
Page 7 line 30 - Details on our method for the amended cohort analysis.
Page 10 line 10 - Details on the results of our amended cohort analysis.

Because the above limit is the result of self-assessment, it is not overcome by including a second measurement point (i.e. this is different than the limits imposed by cross-sectional study design). Nonetheless, I note that the measure of change in support used in this survey is a retrospective assessment of change as measured at the second time-point, which is correlated with mental health measures also assessed at this time point – so it is essentially cross-sectional (as opposed to measuring support at time 1 and at time 2 and analytically determining change over time).
We provide a modified analysis which now determines the change in perceived level of workplace support over time by subtracting the baseline score (time 1) from the follow-up (time 2) score. Our findings are consistent with the previous analysis, but with some nuances which we highlight in the discussion. We thank the Reviewer for this valuable suggestion and we believe the manuscript is strengthened considerably.
As above:
Page 7 line 30 - Details on our method for the amended cohort analysis.
Page 10 line 10 - Details on the results of our amended cohort analysis.

The associations reported here are valuable in spite of these limits on interpretation, but the causal language that is used in the discussion ("demonstrate the impact of change in level of support on mental health outcomes") goes beyond the evidence of this study.
We thank the Reviewer for highlighting the issue of causal language in our discussion. We have toned down the causal language considering the limitations to this research design. Moreover, we present a modified manuscript title to reflect the evidence provided in our study.
Page 1 line 2 - Manuscript title is now amended to: "The relationship between the level of (and changes in) perceived workplace support and mental health, wellbeing and burnout in healthcare professionals (HCP) during the COVID-19 pandemic: insight and mitigating strategies from the CoPE-HCP cohort study".

As noted by the authors, the loss of about half of the cohort at the time of follow-up weakens any claims that the results that are found are representative.
We now present chi squared analysis comparing the characteristics of the baseline-only sample with participant who provided both baseline and follow-up data. The sample of those who participated at both baseline and follow-up consistent of higher proportions of self-identified white and female participants, which is not uncommon for cohort research. Importantly, it also demonstrates that the baseline-only and cohort participants do not differ on mental health outcomes.
Page 9 line 4 - Here we have inserted details of possible sample differences. Chi squared results are included in newly inserted Supplemental Table 1.
Page 12 line 14 - Here we also account for possible issues with representation in our limitations.

Qualitative analysis of free text comments could greatly enrich the results reported from standard instruments and so it is disappointing that the authors present a quantitative summary of the number of comments in each of several categories rather than a qualitative synthesis of the content with exemplar quotes.
Due to word limit restrictions, we are unfortunately unable to provide a qualitative synthesis (and detailed overview of themes) of the free-text items. However, we now present a more concise analysis of the qualitative data, limiting to responses of HCPs only and limiting the analysis to entries describing aspects regarding workplace support (and not other areas of support outside the workplace). In this updated analysis, we do not analyse themes based on the frequency of codes/themes but also account for the perceived significance to the participant when generating the themes.
Page 21 - Please see Table 3 for the amended qualitative analysis with exemplar quotes.
The discussion of various interventions to promote support within organizations is good, but the degree to which the evidence presented in this study supports such measures needs more careful discussion.
We agree with this statement and thank the Reviewer for raising this point. We have limited our conclusions to the design of the study and what is being measured, as mentioned above.

Reviewer 2: Robert Maunder
Institution: Psychiatry, Sinai Health System
General comments (author response in bold)

This study assesses the impact of “workplace support”, broadly defined, and mental health outcomes in the context of the health workforce during the COVID-19 pandemic. This is an area of considerable importance given the robust body of evidence pointing to high-levels of burnout and other mental health challenges within the health workforce, and the potential for increased rates of exodus from the workforce as a result. I appreciated the fact that the authors supplemented their quantitative analysis with a thematic analysis of open-ended questions to assess the nature of workplace support provided. I am not a qualitative expert, and while I enjoyed reading these elements of the paper, I’m not well positioned to provide commentary on methodological rigour or reliability.

Major Comments:
Despite multiple read-throughs I am still not 100% certain that I understand the methodology. Chiefly, it was difficult for me to parse as written whether this is a single sample surveyed at two time points with responses linked at the individual level, or a repeated cross-sectional analysis with no way to link individual responses between baseline and follow-up. Put another way, I am unclear about whether the follow-up results are linked back to the baseline data at the individual level. The fact that two figures are provided (supplemental figures 1a and 1b) with no obvious connection between them, and some of the analytic choices lead me to believe it is repeated cross section with no individual-level data linkage. However, the fact that changes over time in the mental health outcomes are provided, suggests that it must be linked? Assuming the latter is the case, I strongly suggest stating this clearly up front, and that the two supplemental figures be combined into one. Additionally, rather than referring to two samples, the authors should revise throughout to refer to one sample, with loss to follow-up.

We are grateful for this detailed response from the Reviewer. We have now clarified throughout the manuscript that the follow-up sample consists of only participants who also took part at baseline assessment too. The Reviewer is correct in that there is one sample, with loss to follow-up.
As per Reviewer suggestion, we have combined the supplementary tables into a singular table and supplementary figures 1a and 1b into a singular main text figure. We believe this will help avoid confusion.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 3 of supplementary document- Updated supplementary table provided here (Now as Supplemental Table 2).
Page 22 line 1- Updated singular flowchart figure provided here (Figure 1).
Page 7 line 14- Clarity is provided on what the cohort analysis consists of.
Page 7 line 30- Clarity is provided on what the cohort analysis consists of.
Page 8 line 18- Clarity is provided on what the cohort analysis consists of.

If it is a single sample with two linked points of follow-up, the fact that over half of the respondents did not complete the second survey is a huge concern, creating potential for significant selection bias arising from loss to follow-up. The authors do point to this in the limitations sections but fail to comment on how this may impact interpretation of the results. Nor is there any statistical attempt to correct for this aside from pointing out the ways that the follow-up sample differ (or don't) from baseline in terms of demographic characteristics.
We agree with the Reviewer that selection bias is a potential issue here. We would like to point out that the participants volunteering at baseline were not asked to participate in the full 4-month study, but rather they were asked if they would consent to receiving follow-up survey. As such, retaining just under half of respondents is not excessive. We present results from a chi square analysis in the manuscript which demonstrates that, on all mental health measures, they did not differ significantly from the baseline sample. We do highlight in the discussion section that the follow-up participants consist of those generally self-identifying as female and white ethnicity, therefore the findings are generalisable to HCPs with these characteristics. It is not uncommon for studies which rely on self-selection to consist generally of female and white ethnic participant, so we do not perceive this to be a critical issue.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 9 line 4 - We describe similarities and differences between baseline-only and those who responded to both surveys (cohort) on baseline demographic characteristics and mental health. We also insert Chi square result for possible differences in profile in our supplementary document (Supplemental Table 1.)
Page 12 line 18 - statement added regarding mental health and lack of selection bias ("no significant differences were observed for mental health between baseline-only and follow-up (cohort) participants").

Furthermore, assuming it is a individually linked data, I'm confused by the methodological decision to ask respondents to comment directly on the change in management support rather than simply including the change in raw management support scores between the two time points as the primary independent variable in the regression model assessing change in mental health outcomes.
We thank the Reviewer for this suggestion. Our revised manuscript now presents this updated analysis and we believe the manuscript is now much improved.

Key amendments made (indicated by yellow highlighter in manuscript):
Page 7 line 30 - Details on our method for the amended cohort analysis.
Page 10 line 10 - Details on the results of our amended cohort analysis.
Page 11 line 19 – The nuanced, yet consistent, findings of this amended cohort analysis are discussed here:
"However, whilst we observe a trend between change in perceived level of support and insomnia and burnout scores over time, these associations were non-significant. This highlights the relevance of improvements in perceived workplace support to distinct mental health issues, and we speculate that other workplace factors which are not accounted for in this analysis (e.g. long working hours) are more likely to impact on burnout and insomnia."

If the data are not linked at the individual level, can the authors please clarify whether the respondents to the follow-up survey are a subset of the original sample? I.e. were respondents required to submit a baseline survey in order to participate in the follow-up? Also, I would suggest the inclusion of a tabular comparison of those two sample as their table 1, rather than the comparison between HCP and non-HCP.
This is addressed following calculating the change in score between baseline and follow-up because only those with both phases of data will be eligible for this analysis. We agree with the suggestions regarding tabular comparison of the two samples.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 18 line 1- Table 1 is now amended as recommended by the Reviewer.

An additional limitation is the lack of inclusion of data on professions/roles. The nature and burden of mental health issues as well as the availability of workplace and managerial supports are likely to vary dramatically by profession. I'm less familiar with the context in the UK, but in Canada, most physicians wouldn't have "managers" or "supervisors" who would be providing the supports listed in Table 2.
We thank the Reviewer for this constructive feedback. Regarding the first point on UK and Canada differences, we have revised our thematic analysis to themes relating to workplace support (and not specifically managerial support) in the broader sense. We believe that our themes describe the qualities of the workplace which comprise effective 'support' regardless of their role or where the professional is posted.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 20 - Please see Table 3 for the amended qualitative analysis with exemplar quotes.

This is further muddied by the inclusion of the non-HCP workers, about whom we are provided no work-related data.
We have now streamlined the entire manuscript and report quantitative and qualitative findings from HCPs only.
Page 8 line 18 - Clarity provided regarding who the sample are included in the analysis.
Page 8 line 21 – For further clarity, we make additional amendment by describing the regions based for the non-UK HCPs in the analyses.

The lack of inclusion of profession in the adjusted results is a significant limitation. And not triangulating thematic analysis by profession is similarly problematic. It seems the

authors may have this data as they note in the second paragraph of the results that the non-HCP sample was mostly university/academic staff.
We agree that the association between perceived support and mental health outcomes may vary by profession. Our amended logistic regression and linear regression analyses are now adjusted for professional role (medical doctor [reference group] vs. healthcare assistants, nurses, and AHPs), based on the growing evidence that medical doctors are at significantly lower risk (odds) of mental health impact during COVID-19 pandemic.
Regarding triangulating the thematic analysis by profession, we believe that this is beyond the scope of the present manuscript. Our thematic analysis aims to report trends/themes across the whole data, and does not seek to quantify codes/themes by stratified roles. We also hold the view that a thematic analysis does not seek to compare between categories, in which case a quantitative analysis of survey data would be more appropriate.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 7 line 28- Including adjustment of professional roles in the models.
Page 19 and Page 23 - Tables of updated analyses provided here (additional adjustment for professional role).

Finally, there is so much content here. It is a lot to get through at 600+ words over *CMAJ*'s limit plus pages and pages of supplemental materials. I strongly suggest considering how this could be streamlined. One option might be to narrow the focus to HCP only, eliminating the data from non-HCP, as there isn't much made of the comparison between the two as written.
We thank the Reviewer for this suggestion. We have removed non-HCPs from both quantitative and qualitative analysis and believe that the manuscript is much improved. We have also greatly reduced the number of supplementary materials and main text tables and figures. Whilst we considerably streamlined the manuscript, our word count is reduced from ~3135 words to just 3115 due to further additions to the manuscript (at request of Reviewers and Editors).

Minor comments:
Please provide a short explanation of the inclusion/exclusion criteria in the methods section rather than the supplement.
Inclusion criteria is now provided on page 6 line 11.

In the first paragraph of the discussion, the authors note that their findings indicate "the important causal associations between workplace supports and mental health of professionals". I do not agree that a single cohort (or repeated cross sectional) study can demonstrate causation. I recommend removing this statement.
We agree with the Reviewer for this recommendation. We have removed language alluding to causation throughout the manuscript, or conclusions which go beyond the evidence presented.

The work was conducted in the UK; however, respondents come from a variety of jurisdictions. Given that this is a Canadian journal, could the authors revise the background and discussion to include Canadian publications/perspectives on health workforce mental health and workplace supports?
We have conducted brief literature search for relevant Canadian publications. In addition to the quantitative studies Cyr et al. (2021) and Havaei et al. (2021)

already cited in the introduction, we have now further cited 2 qualitative studies of healthcare workers in Canada.
Unfortunately, due to word limit restrictions we are unable to elaborate on the findings and discuss how these are relevant to Canadian health systems specifically.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 5 line 21- We now cite the following Canadian articles here (Page 16 line 4 for reference list):
Berkhout, S. G., Sheehan, K. A., & Abbey, S. E. (2021). Individual-and Institutional-level Concerns of Health Care Workers in Canada During the COVID-19 Pandemic: A Qualitative Analysis. JAMA Network Open, 4(7), e2118425-e2118425. doi:10.1001/jamanetworkopen.2021.18425
Buckley, L., Berta, W., Cleverley, K., & Widger, K. (2022). Exploring Pediatric Nurses' Perspectives on Their Work Environment, Work Attitudes, and Experience of Burnout: What Really Matters?. Frontiers in Pediatrics, 10. https://doi.org/10.3389/fped.2022.851001

Models are listed as controlling for pre-existing mental health conditions and physical health conditions. Please provide question wording. Is this a yes/no variable, a list, or…? This also should be in the methods section rather than the supplement.
Now on page 6 line 21 – We now provide brief description of these items, and we also state that it is regarded as yes/no binary item in statistical analysis on page 7 line 27. We now omit the supplementary methods from our submission because we believe it is superfluous.

Can the authors provide rationale for collapsing their outcome from a 10 point scale to "unsupported", "neither supported nor unsupported", "supported". Also please provide the question wording, as what is written in the methods makes the question sound as though was dichotomous and it clearly was not: "… asking if participants felt that they received adequate support directly from their supervisors…"
The reason for collapsing the primary predictor variable 'perceived support' to a three-variable was to help readers interpret the results. We have run similar logistic regression models (not reported in manuscript) at baseline and follow-up where we treat perceived support as a continuous predictor variable: the results are consistent with our logistic regression outputs reported in the manuscript.
Key amendments made (indicated by yellow highlighter in manuscript):
Page 6 line 22 – We have now added the exact statement for the item measure perceived level of workplace support for clarity.