

Peer Review Report

Review Report on Mental health status, risk and protective factors for healthcare staff prior to the first major COVID-19 outbreak in Western Australia

Original Article, Int J Public Health

Reviewer: Roberto Mediavilla

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EVALUATION

Q 1 Please summarize the main findings of the study.

The authors used a sample of Australian HCWs assessed in early 2022 to describe the mental health problems (anxiety, depression, and PTSD symptoms) and explore their association with risk and protective factors. They found that workplace-related stress was associated with worse mental health outcomes and that social support was associated with less anxiety and depression symptoms (n ~ 400).

Q 2 Please highlight the limitations and strengths.

Main strengths: Australian context, good instruments

Main limitations: lack of causal models, results not clearly presented

Q 3 Please provide your detailed review report to the authors. The editors prefer to receive your review structured in major and minor comments. Please consider in your review the methods (statistical methods valid and correctly applied (e.g. sample size, choice of test), is the study replicable based on the method description?), results, data interpretation and references. If there are any objective errors, or if the conclusions are not supported, you should detail your concerns.

I think this study can make a contribution to the current state of the art. My major concern is that the authors use statistical models that do not seem to be based in prior causal knowledge. For instance, the authors first say that women reported more mental health problems than doctors (line 192, Table 2). A Table 1 the characteristics of the participants is missing, but I would tend to think that such differences may be due to women being more likely to have mental health problems, and nurses being more likely to be women, compared to doctors. My point here is: what does such bivariate association add to our current understanding? Are authors only trying to identify risk groups and do not care about potential causal associations between exposures and outcomes? If so, I think they should have a larger sample size and probably they could have used a different analytic approach (e.g., machine learning). Since authors are interested in exploring different associations between exposures (both risk and protective factors) and outcomes (anxiety, depression, and PTSD), and considering the quantity and quality of papers on the topic, I suggest that the authors build causal models for each research question and adjust for potential sources of confounding. They could check the work by Hernán and Robins on causal inference, and use DAGs, to simplify the process. In relation to this, I am not sure about the step-wise approach. The authors run a model which basically includes all predictors in different steps, and then make interpretation of individual predictors separately. As a consequence, they get unexpected findings, such as the lack of association between gender and mental health problems. Again, I think that estimates would be more interpretable if authors used specific causal models for each association (e.g., if the authors want to explore the effect of exposure to COVID-19 patients, they should at least adjust for age, gender, and type of job, so that the estimate is not biased).

In general, I would also suggest that the authors emphasize how their results contribute to the current state-of-the-art. I am not familiar with the Australian reality, but I think it is very unique and it can reveal many things that are important for current and future pandemics. Hence I suggest that the authors put their results more in context with what we already know from other Australian studies (they could expand on the first paragraph of the Discussion). In relation to this, I think the authors should make clear in the Discussion

whether the levels of exposure to stressors, especially pandemic-specific exposures such as infection concerns or working hours, are different from other studies conducted in countries with different infection rates. Such context, in addition to causal models, may help telling a very interesting story, namely that there are “just-right” levels of exposure to stressors that do not necessarily worsen mental health.

The study is limited by non-probabilistic sampling, which increases the risk of selection bias. In the Methods section, the authors say that “all HCWs” from different hospitals were invited to participate. Is that true? Could then they give an estimate of response or collaboration rates? That would be very useful.

Additional issues:

- Lines 96-97. The authors should provide the variables' values (e.g., doctors, nurses, etc.)
- Lines 101-112. Workplace factors. The authors created an aggregated sum score which has good internal consistency. Because they rely on it very much, I think they should (a) provide the specific items on the Appendix and (b) report an additional index of validity or reliability.
- Lines 131, 140, 145. How did the authors determine the cut-offs? Are these valid thresholds for Australian population?
- Line 158. The authors mention collinearity. Did they manage to avoid it in the regression models? If they did, that is probably because they used aggregated scores which may not be reliable or valid. They should comment on this on the limitation section.
- Tables are not very informative. A Table 1 showing the characteristics of the participants is needed. The authors could include four columns: all, probably depressed, probably anxious, and probable PTSD. I cannot see in the tables how many people are probably depressed in the sample, for instance. I also suggest that they report both aggregated scores and dichotomous scores. I do not find ordinal scores (mild, moderate, severe) very informative for this purpose.
- Table 3. If the authors re-analyse everything from a causal framework, they should build a new Table 3, where they could report adjusted estimates of each relevant exposure (rows) on each mental health outcome (columns). I would not do step-wise models. If the authors wish to report R2 and other model fit estimates, they should include it as a footnote.
- I would avoid reporting p-values. I would keep Tables 1 and 2 fully descriptive, and report crude and adjusted estimates in Table 3, along with 95% CIs.

PLEASE COMMENT

Q 4 Is the title appropriate, concise, attractive?

I would rephrase: "Mental health outcomes and risk and protective factors among healthcare workers prior to the first major COVID-19 outbreak in Western Australia"

Q 5 Are the keywords appropriate?

anxiety is missing

Q 6 Is the English language of sufficient quality?

Yes

Q 7 Is the quality of the figures and tables satisfactory?

No.

Q 8 Does the reference list cover the relevant literature adequately and in an unbiased manner?

Probably, yes (authors should provide more context from Australian reality)

QUALITY ASSESSMENT

Q 9 Originality

Q 10 Rigor

Q 11 Significance to the field

Q 12 Interest to a general audience

Q 13 Quality of the writing

Q 14 Overall scientific quality of the study

REVISION LEVEL

Q 15 Please make a recommendation based on your comments:

Major revisions.