PCI-RR Review of “Relationship between perceived risk and compliance to infection control measures during the first year of a pandemic” by Sebastian Bjørkheim, Sigurd Hystad, and Bjørn Sætrevik

The authors propose a study of the relationship between risk perception and compliance with infection control measures during the first year of the COVID-19 pandemic. The study uses a large representative sample of the Norwegian population, with measurements at four time points, between March and November 2020. This represents a great opportunity to investigate how the relationship between risk and compliance develops over time. I find the proposal timely, interesting, and generally well planned, but I have some points which I think could be improved, mostly concerning methodology and analysis.

**1A. The scientific validity of the research question(s).**

I find the research question to be interesting and scientifically justifiable. The introduction is well-written and gives a clear rationale for the proposed study, and the study connects well to previous research on similar topics.

**1B. The logic, rationale, and plausibility of the proposed hypotheses, as applicable.**

There are three hypotheses. H1 proposes a positive association between risk and compliance at each time point. H2 proposes risk at previous time point will be positively associated with compliance at the subsequent time point. H3 proposes that compliance at previous time point will predict risk at subsequent time point. All three hypotheses seem logical and plausible.

However, even though the introduction gives a nice overview of the topic and the need for the proposed study, I feel like there is room to expand a little bit concerning the hypotheses and the potential outcomes of the analyses. For instance, in the study design template in Table 3, the authors discuss some interesting interpretations of different outcomes. Especially for hypothesis 2 and 3, I think these could be discussed in the introduction. Communicating for example the idea that high risk could lead to establishing habits that persist over time and/or could lead to fatigue would help readers think about the importance of the research and about potential implications, beyond what is already included.

**1C. The soundness and feasibility of the methodology and analysis pipeline (including statistical power analysis or alternative sampling plans where applicable).**

As acknowledged by the authors, the fact that the data has already been collected and that the authors have already inspected some key measures and tested the association between risk and compliance at T1, makes the proposal a “Level 1” submission. Even though the key research questions have not yet been analyzed, I believe the authors should think more about how they can further reduce the risk of bias.

First, under section 2.3, the authors explain that they have previously described the association between risk and compliance at T1 in a couple of publications, so that they have in a sense already tested H1a. Even though the association they tested “also included several other items not included in the present analysis”, it seems important to (briefly) describe what this previous analysis showed. From a quick look at the publications, it seems like the association between perceived risk and compliance was not straightforward at T1. This should be described so that readers are made aware of what the authors already know about the relationships that will be investigated in the current proposal.

Second, one suggestion to reduce bias for Level 1 submissions is to have “an extremely conservative statistical threshold”, but the authors suggest in Table 3 to use the conventional .05 level. I think it would be good to consider a stricter statistical threshold. Given the large sample size, the authors would probably have power to detect relatively small effects even if the alpha level is set at a stricter level. If the authors decide against this, they should at least explain why they believe the conventional threshold is sufficiently strict enough.

On a related note, the authors do not report a power analysis. It would be interesting to know something about which effect sizes one could reliably detect with e.g. 90 or 95% power. Although I suspect this is complex for the proposed model, it seems that there are packages that could be helpful: <https://www.tandfonline.com/doi/full/10.1080/10705511.2022.2122467>

If it turns out that it is too complex to perform meaningful power (sensitivity) analysis, it would still be helpful to include a section discussing the topic in relation to the sample size and potential effect sizes.

Third, another suggestion to reduce bias is to recruit a blinded analyst. I am not sure if this is necessary here, but I include the point to illustrate that the authors should emphasize the issue of potential bias to a greater degree. Expanding on section 2.3 and explaining which countermeasures they have taken, or why such countermeasures are not necessary, would be helpful.

Analysis pipeline

The analysis plan is clearly, but quite briefly described. In light of the issues discussed above, I think it would be good to provide syntax for the proposed analysis. This would help to minimize concerns about analytical flexibility.

I also have a question. I do not know much about the kind of model proposed here, but regarding for instance H2, I wondered whether the proposed positive association between e.g. T1 risk and T2 compliance controls for perceived risk at T2? This might be a terribly naïve question, but I think some more details about the analysis plan could help those who are not well versed in cross-lagged panel models to get a better picture of what is going on.

**1D. Whether the clarity and degree of methodological detail is sufficient to closely replicate the proposed study procedures and analysis pipeline and to prevent undisclosed flexibility in the procedures and analyses.**

As mentioned, code/syntax for the proposed analysis would be beneficial. Additionally, it is not clear whether it will be possible for others to access the data. It would be best if the data could be shared openly, but I suspect there might be privacy concerns here preventing this possibility. If so, it is important to state this clearly, and to discuss measures that would ensure maximum transparency and reproducibility.

**1E. Whether the authors have considered sufficient outcome-neutral conditions (e.g. absence of floor or ceiling effects; positive controls; other quality checks) for ensuring that the obtained results are able to test the stated hypotheses or answer the stated research question(s).**

I don’t see this point as applicable here.

**Other comments**

I found Figure 1 illuminating. The fluctuations in the infection rate also reveals something about the “objective” risk at different times in the pandemic (with some important caveats, e.g., the high uncertainty especially at T1 may make it difficult to give an objective risk measure). I wonder whether it would be interesting to control for “objective” risk levels, by using infection rates or something similar. Or perhaps one could compare effects of perceived risk on compliance with effects of objective risk? I’m not sure if this is at all feasible, but raise this question as something the authors can consider.

Minor comments:

Abstract: “during the first year COVID-19 pandemic in Norway” 🡪 “during the first year of the COVID-19 pandemic in Norway”

p. 3 “They also found that psychological factors to be” 🡪 “They also found psychological factors to be”

p. 5 “The current study may help to fill the knowledge gap” 🡪 I think you can allow yourselves to be less modest here.

**Conclusion**

I think the topic is very interesting and worthwhile to pursue, and that the current proposal is solid. My main comment is that I see potential for improvements when it comes to controlling for potential bias. I hope the authors find my comments helpful.

Sincerely,

Erik Løhre