My main issue with the original manuscript was that there was potential to better control for bias given the pre-existing data and the fact that part of it has already been analyzed. I find the countermeasures suggested by the authors in the revision to address these concerns.

Thus, I now only have two minor comments, which are only meant as suggestions for improvements.

* Paragraph that runs from p 2 to p 3 (starting with “The psychological aspects of risk perception…” and ending with “…compliance to infection control measures.”) is quite long and dense, and I struggle to pick up the main message. Could this be split in two, and perhaps a summarizing sentence added?
* Power analysis: I suggested looking into the possibility of running a power analysis. The authors reply that this requires making a lot of assumptions, which makes them skeptical of running it. I am sympathetic to this reasoning. Nevertheless, in their reply letter they also write that “*Based on the example given in that paper, we can conclude that for a cross lagged panel model with 4 measurement rounds, a sample of 1800 is sufficient to reliably detect “small” cross lagged effects of .10, at a power of .80, even with a very high degree of between-unit variance. We expect a panel sample of n ~ 2000 in our study, and we should thus expect to have a power of more than .80 to detect “small” cross lagged effects.*“. I found this quite informative, even if it is just a rough estimate. Perhaps it is self-evident that such a large sample gives enough power to detect even small effects, but I think it could be valuable to spell this out for readers. Note that this would not be a post-hoc power analysis, but a sensitivity power analysis giving a rough estimate of what kind of effects one would have for example 80% power to detect given the sample size and alpha (and other relevant parameters). This is not a big point, but at least something to consider.

I look forward to seeing the results and the final paper!

Sincerely,

Erik Løhre