Comment on egusphere-2022-9
Anonymous Referee #1

Referee comment on "Technical Note: Quantifying Hazard Probability and Risk from Ensemble Projections of Downscaled Climate Variables" by James P. Kossin et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-9-RC1, 2022

Summary:

This technical note presents a method to estimate changes in risk due to a changing climate while characterizing sources of uncertainty. It is a useful effort in that resource managers need to make decisions on the response to increased climate-driven risks, yet there is often a divide between the advances in climate science and the delivery of information targeted at stakeholder needs. While as a technical note it is not expected to be a lengthy manuscript, the paper misses important context which weakens its contribution. Detailed comments follow.

Comments:

- Line 34, “Often this information is provided based on projections of the most-likely value of some hazard variable at some particular time” should be augmented by noting that many current studies remove time from the equation and present projected impacts for a certain level of warming or accumulated CO2 emitted (which removes the difficulty in considering the different emissions pathways independently).
- Lines 40-44, The discussion of natural variability versus model spread reminded me of the foundational work by Hawkins & Sutton (2009; 2011), who also fit a polynomial to projections to separate external from internal variability, and then added variability due to model spread to their analysis. I was surprised to see no reference to their work (let alone the many papers that have built on that over the last decade), given that this is quite similar to the approach presented here. There have also been dozens of papers presenting changes in return periods of extreme events (extreme precipitation, peak flows, extreme sea levels, ...), presenting the results in many different ways. This paper needs a much better review of past work in this area so what may be new can be discerned.
Lines 45-51, the NASA-NEX CMIP5 downscaled data was used from this study. It would be helpful to remind readers that there are many options for downscaling climate model output. In particular for this data study, some caveats should be noted. For example, the daily BCSD method has been shown to underestimate extreme precipitation events, with the monthly version outperforming it on some important metrics (Gutmann et al., WRR, 2014). While the paper is presenting a method that could be used with any downscaled data, that would be important information to provide.

Following on the last comment, additional uncertainties should also be mentioned, if not included in the analysis. The choice of downscaling method can have a significant effect on the results. For example, using downscaled data from https://gdo-dcp.ucar.gov/downscaled_cmip_projections or https://cal-adapt.org/ could help frame some of these uncertainties.

Another source of uncertainties is the choice of impact model. As one example, the rudimentary AECOM model is used, but applying any other model would certainly give different results. Has the AECOM model been described and validated in another peer-reviewed publication? If it has not been presented for peer review somewhere, its use here does not seem supported unless it is presented and validated. The only reference provided is an untraceable professional report. There are many other simple hydrologic models available, many of which have been used in climate change studies. A brief review of some of them would be helpful, with an explanation for why the AECOM was selected for this demonstration.

A minor comment: line 74 “has little physical relevance” might be more accurately phrased “has no temporal correspondence with observations”.