Comment on nhess-2021-373
Anonymous Referee #2

Referee comment on "How well are hazards associated with derechos reproduced in regional climate simulations?" by Tristan J. Shepherd et al., Nat. Hazards Earth Syst. Sci. Discuss., https://doi.org/10.5194/nhess-2021-373-RC2, 2022

Review of nhess-2021-373: How well are hazards associated with derechos reproduced in regional climate simulations?

The authors used WRF as a convective-permitting regional climate model to produce 11 simulations of a severe derecho that affected the northeastern U.S. in 2012. The derecho had a major impact in terms of property damage and power outages over a large, populated region. This derecho was poorly forecast, yet we need to understand how climate change will impact such extreme mesoconvective systems. The authors examined the role of microphysical parametrization, nudging, and two different reanalysis products on 3km simulations of several days leading up to and including the derecho. The authors compared model output to surface and radar observations of precipitation, wind gust, hail, as well as variables describing the convective environment, such as vertical velocities and cold pool formation. The explanation of the methods of model assessment was particularly thoughtful.

Overall, the manuscript is well constructed with clear objectives, detailed methodology, and significant findings. I recommend that the manuscript be accepted following minor revisions.

Major comments

- While most of the simulations poorly represented the derecho, this is not surprising given that this event was not well predicted. While I would not ask the authors to address in this manuscript, it would be intriguing to duplicate this work for a significant mesoconvective system that was well predicted.
While the use of the different microphysical parameterizations in the model was well designed, it is unclear what is learned by the comparison of ERA5 and ERA-Interim. It is not clear to me that you can generalize that ERA-Interim is inherently better at producing boundary conditions for such simulations (l. 526-527), or whether the small differences in the pre-storm temperature and moisture fields in ERA-Interim (l. 413-415) fortuitously produced more realistic simulations.

I share the concern with the first reviewer that the abstract was not sufficiently specific, but I find the proposed new abstract in the authors’ response to be a significant improvement that addresses this concern.

I share the first reviewer’s concern about the need for additional context on the convective environment for this storm in the background. The additions proposed by the authors appear to address this concern.

Minor

44: Is “atmospheric phenomena” another way of saying “weather”? Or is it intended to capture more.
44: focuses
51: “function model configuration” – appears a word is missing
54: Is “advected” the right word? Perhaps “propagated”?
56: run-on sentence
88-89: it might be helpful to explain “scale-aware convective parameterizations”. How are they “scale-aware”?
93: “degree/manner in what the model parameterization interact” is unclear. What is this trying to say?
306: Perhaps I’m missing something obvious, but why would s(w) be used as intensity for vertical motion rather than just w?
322-323: This sentence (Rank correlation coefficients...”) isn’t clear. How does the rank correlation show which model property most greatly influences skill? The correlations show how well the model and observations agree, but the word “influences” suggests that you can determine a causal mechanism.
501: The authors use the pseudo-global warming framework as a justification but don’t provide references (perhaps I missed them) how this framework has been used to examine mesoconvective systems nor provide examples of how such a framework might be used.