Response to Reviewers Document for GMD-2020-86 by Almudena García-García, Francisco José Cuesta-Valero, Hugo Beltrami, Fidel González-Rouco, Elena García-Bustamante and Joel Finnis

We are grateful for the thoughtful and constructive feedback of both reviewers.

This Response to the Reviewers document provides a complete description of the changes that have been made in response to each individual reviewer comment. Reviewer comments are shown in plain text. Author responses are shown in blue text. All line numbers in the author responses refer to locations in the revised manuscript with changes marked.

Referee #1

In their paper "Land Surface Model influence on the simulated climatologies of temperature and precipitation extremes in the WRF v.3.9 model over North America", the authors present an analysis of 4 WRF simulations run over North America over the same period, with each simulation using a different land surface model. Their study is clear and interesting, and serves not only to highlight the importance of land-atmosphere coupling on temperature and precipitation extremes, but also to demonstrate that while topography and the atmosphere itself do play a first order role in controlling extremes over land, the choice of land surface model itself can be a source of substantial spread.

The authors have done an excellent job addressing the concerns raised during the last round of revision, and the manuscript and figures have improved in their readability and clarity. I have only minor comments, noted below.

Line 290 / Figure 4: I feel the need to still push back at calling a region that covers the north-eastern corner of Canada and does not cover Greenland "Greenland". While I understand that the authors want to be consistent with the domains of a referenced study, they should at least acknowledge that the domain does not include Greenland and they're only calling it that for ease of comparison to Giorgi and Francisco (whose domain did actually include Greenland, unlike this study's domain).

We have included a couple of lines clarifying this in the new version of the manuscript (See lines 287-289).

Line 245: I appreciate the inclusion of more discussion on why each VAC category is occurring for each model (e.g. the low latent heat fluxes in CLM4 during winter). It would be even MORE helpful if the authors could identify what aspects of each LSM were resulting in the observed fluxes that control the VAC category, though they do allude to variations in representations of plants and snow.

Differences between LSM components in the description of land cover affect the simulation of soil properties, such as albedo, evaporative resistance, and surface roughness. These soil properties play a key role in the computation of the energy and water fluxes at the land surface, and therefore in the simulation of near-surface conditions. We have included this explanation in lines 400-403.

Methodological comment: I still think it would be really useful to see the difference in spread in results that occurs from using multiple ensemble members of a single LSM-WRF setup (e.g. multiple NOAH-WRF simulations) vs the spread across 4 single instantiations of 4 different LSM-WRF simulations. I

understand that that is computationally expensive, and don't think it is required for the publication of this study, but if the authors choose to further pursue the questions they raised in this study, explicitly quantifying the spread in an ensemble of single LSM-WRF runs and comparing that to the spread in WRF runs across multiple LSMs would (a) be interesting and (b) if the single LSM-WRF spread proves to be small, make the results presented in this study more robust.

Thanks for the suggestion, it is indeed an interesting future line of investigation. However, considering the computational resources required for that ensemble, it maybe more efficient to perform those simulations for a shorter period of time (e.g. 5 or 10 years). We could compare that ensemble with the spread among the LSM configurations for that period of time and use the remaining computational resources to modify atmospheric parameterizations. Although we discussed the effect of atmospheric parameterizations on the simulations using references from the literature, its direct comparison with our simulations is also an interesting research line.

Minor typos:

Line 85: "examinate" should be "examine"

Done (line 85).

Line 173/174: "increases" and "decreases" should be "increasing" and "decreasing"

Corrected (lines 172).

Referee #2

I believe the authors have sufficiently responded to my reviews. The changes they have made based on both reviewers have improved the paper, and beyond a few technical corrections the paper is ready for publication.

Line 25: confirm "that" all of these...

Done (line 25).

Line 34: of is typed two times, one needs to be removed.

Thanks for catching that. It has been corrected (line 34).

Line 49: I believe it should be snow cover, not snow covers.

Agreed and changed (line 48).

Paragraph staring on line 67: As ERA5 and MERRA2 have been published for a while now, a brief description of how their land surface components are treated should be included.

We have included information about the ERA5 product and the MERRA2 product as requested (lines 71-75 and 81-82).

Line 225: Would be better worded as: "agreeing in seasonality and broadly in the regional classification of energy and water limited areas (e.g. areas with high probability of episodes where atmospheric forcing or soil conditions control land-atmosphere interactions).

Changed (line 222-224).

I appreciate the inclusion of the discussion of how land-use differences and snow cover could lead to differences LA coupling in the models. I think, however, you need to be more careful with your language about cause/effect. e.g. on line 381 you say "representation of vegetation cover are causing". To really know if this was the cause i think sensitivity tests where the land cover type is changed in each model would need to be done. You could change the language to "likely play a role" or "influence" in these cases, but directly stating "cause" is beyond the scope of this analysis.

We have gone through the manuscript addressing the reviewer concern (e.g. line 381, 398, and 556).

Line 458: I am confused by the confusion of "ocean parameterizations" here. In these RCM simulations the ocean is just a boundary condition provided by the GCM. The way the boundary layer responds to the ocean and fluxes are calculated will be different, but those are not really ocean parameterizations. I suggest removing "ocean parameterizations" here and add it to "treatment of boundary conditions, including sea surface temperatures over the ocean"

Agreed. We have modified these lines accordingly (see line 457).