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Geoscientific Model Development Discussions

Interactive comment on "Land Surface Model influence on the simulated climatologies of temperature and precipitation extremes in the WRF v.3.9 model over North America" *by* Almudena García-García et al.

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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 extremely grateful for the thoughtful and constructive feedback of both reviewers. We really appreciate the quality of the revision, it has improved our new version of the manuscript.

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 2

Review of "Land Surface Model influence on the simulated climatologies of temperature and precipitation extremes in the WRF v.3.9 model over North America. By Garcia-Garcia et al. Submitted to GMD. Reviewed in June 2020.

This paper is focused on quantifying the uncertainty in the simulation of temperature and precipitation extremes that is associated with the choice of land-surface model (LSM) used in regional climate model (RCM) simulations. The authors performed 4, 34-year climate simulations using WRF driven with NARR boundary conditions. The only difference between each climate simulation was the choice of LSM (NOAH, NOAH-MP, CLM4, NOAH-MP-VG). They use a single land-atmosphere coupling metric to highlight regional differences in the way the land surface interacts with the atmosphere. They then calculate 16 different temperature and precipitation climate extremes to examine the role of the LSM. Finally they make an attempt to place their work in the context of other model ensembles by comparing climate extremes in their WRF ensemble with some NA-CORDEX models.

This paper is very well written, making it easy to follow. I also appreciate the quality of their figures and color tables. However, as this paper was submitted to a model development journal, I do not believe they include enough discussion of why differences in the LSMs result in differences in land-atmosphere coupling and climate extremes. I suggest this paper be accepted with major revisions.

As it was the case for the revision of referee 1, we thank the reviewer for the detail and the quality of this review. Some of these comments were also pointed out by referee 1.

We have addressed these points by performing some new calculations and simulations in addition to modify the text in the manuscript. We think this revision has improved the clarity and quality of our manuscript.

Major Comment:

1. More information and commentary/insights need to be provided regarding why the different LSM result in variations in land-atmosphere coupling and the VAC index. This could include maps of land cover type/fraction, how surface fluxes are calculated, how soil temperatures are calculated etc. The seasonal cycle of snow cover which will play a role in seasonal transitions to different regimes. Your study shows that the LSM does make a difference, but you need to do more to explain why the models are different (even if it is just hypotheses). This is especially true as you submitted this paper to a Model Development journal – and for this to be useful readers will want to know more about how the LSMs differ and how this could result in changes. Some of the details about this could be in supplemental, but a deeper discussion needs to be included in the paper itself as well.

We have modified sections 4.1, 4.2 and 4.3 to provide with a more comprehensive explanation about differences between LSMs in the representation of the VAC index and the climatology of extremes. Additionally, we have plotted the land use categories employed in the four simulations to study the role of the different LSM representations of land cover on the uncertainties in the representation of extreme indices among our four simulations. This comparison allowed us to identify some coincidences between vegetation and snow cover and areas with large uncertainty in the simulations of extremes. Thus, LSM differences in the representation of vegetation and snow cover are likely affecting the simulation of land-atmosphere interactions and consequently the simulation of extremes.

2. You do not sufficiently link differences in the simulation of land-atmosphere coupling are related to differences in temperature and precipitation extremes. In section 4.3

you do a small amount of work highlighting regions where the VAC index differs and differences occur in the extreme values – but there is no discussion of why/how land atmosphere coupling may affect the simulation of extremes. This could be included in the introduction, but also in more detail and specific to the LSMs used in this study in section 4.3.

LSM differences in the representation of land cover and soil conditions will lead to a different representation of energy fluxes at the land surface, affecting atmospheric processes. The different LSM representation of latent heat flux will affect the simulation of surface temperatures in the following way: a decrease in latent heat flux will likely mean an increase in the energy available for sensible heat flux, which is directly related to the air-ground temperature gradient. The increase in sensible heat flux yields an increase in this temperature gradient, likely leading to changes in air temperatures (Seneviratne et al., 2010). Meanwhile changes in latent heat flux originated from the different LSM components yield changes in the atmospheric water content, possibly leading to changes in the formation of clouds and precipitation (Seneviratne et al., 2010). We have included this discussion in the introduction and in section 4.3 in the new version of the manuscript (lines 35-44 and 318-330).

3. The motivation for including NA-CORDEX in this study is not sufficiently clear, and I'm not sure it adds value to the paper. I surmise from section 5.1 that you are trying to show or estimate how much of the uncertainty in temperature and precipitation extremes in multi-model ensembles may be associated with choice of LSM – but as you state there are so many differences in the NA-CORDEX simulations that it's impossible to say what role the LSM actually plays. You make the statement in a few places that the NA-CORDEX models have similar regions with large uncertainties in extremes – but I see more differences between the different model ensembles than similarities.

We still think that the comparison of our simulations with the CORDEX simulations is interesting to present the hypothesis that the LSM component is contributing to the uncertainty in inter-model ensembles. The similarities in the areas with large uncer-

tainty in the simulation of extreme indices within our four WRF simulations and the CORDEX simulations suggest that the LSM can be an important source of uncertainty in inter-model ensembles. Therefore, some caution should be taken when selecting a multi-model ensemble to make sure you include a variety of LSM components so you account for the associated uncertainty. We have tried to emphasize this in the new version of the manuscript (lines 63-66, 89-91, 445-450, 466-475 and 525-531).

General Comments.

Need to define what they mean "early on". This paper only focuses on monthly timescales – so that limits the types of extremes that can be studied. All readers will come to this paper with a different assumption of what "extremes" mean. These are outlined in table 2 – but I think saying someplace you are looking at essentially annual maximum values calculated on the daily timestep. Even just a for example inclusion when you mention the climate indices used in the IPCC.

We have included a sentence to clarify that we use these extreme indices at climatological scales at the end of the first paragraph in the Introduction (lines 34). We have also defined all extreme indices in the methodology using more general words to make sure the reader understands what we are representing (lines 200-206).

Line 22: The word "interpretation" is not appropriate in this context (here it would mean "explanation" but models don't explain the climate they represent or simulate the climate. I would say "simulation" or "representation".

Agreed and changed (line 26).

Line 28: add "the" before IPCC.

Done (line 32).

Line 31: instead of "affect and are affected by" you could use "are coupled to" and be more clear. Also no comma needed after phenomena.

Changed (line 35).

Paragraph on lines 53-67: At the moment this reads as a "non-sequitur" in the introduction the discussion of LSMs in reanalysis products needs to be linked to the work done in this work (which does not include analysis of reanalysis products). One option would be to include an explicit statement for why this should be discussed in the introduction. Something along the lines of "examination of the variations in land-atmosphere coupling based on the choice and complexity of the LSM will have implications for weather forecasting and the production of reanalysis products". (or whatever reason you include this information here, if my assumption was incorrect).

Agreed. We have worked on the connection of this paragraph with the introduction (lines 66-69).

Line 68: I suggest adding "coupling feedbacks" – not all coupling leads to feedbacks per-say.

Agreed and changed (line 83).

Lines 113-115: Please provide a justification for why a single year of spin up was used. Is this sufficient for deep soil moisture to spin up? Did you do any testing to see if soil moisture etc. was spun up after one year? What level of soil moisture is important for your study and is that actually spun-up in this time frame?

We used the one year spin-up because it is the spin up duration used in previous WRF climate simulations to reach the equilibrium of air and soil variables, such as those in Wang and Kotamarthi, (2015), Katragkou et al., (2015) and Barlage et al., (2015). We have included this justification on the text (lines 135-140). To address the reviewer query, we also performed an additional simulation with the CLM4 LSM starting on June 1st 1979. The comparison of the WRF solution of monthly latent heat flux and surface air temperature from 1980 to 1981 show very small differences between both simulations (Figures 1 and 2 in this document). Thus, the effect of initial conditions on

our results is small.

Line 130-134: This relates to my previous comment that a discussion of what type (e.g. temporal scale) of extreme events this paper is focusing on. You have chosen a LA coupling metric that works on monthly time scales. What type of variability and coupling will you capture using monthly data. Presumably you can calculate the VAC regimes using daily data rather than monthly data – which might include some shorter frequency variations that are lost in the monthly data. Why use a monthly metric when all of your extremes are based on daily maximums/percentiles etc. I'm not saying this was an incorrect choice, it just needs to be explained.

We used the VAC metric at monthly scales with the 30th and 70th percentiles as in Sippel et al., 2013, where the authors demonstrated that this monthly metric is useful for the analysis of daily extreme events at climatology scales by a statistical analysis and the comparison of the VAC metric with another correlation metric. We have included this justification on the text (lines 162-164). Additionally, since we are interested in the climatology of extreme events estimating the mean of all extreme indices for the analysis period, we do not think the monthly VAC metric is losing relevant information for our study.

Page 5 – the equations for VAC. I suggest adding "Atmo. Control Coupling" or "Atmo. Control interactions" or something like that – the use of the word control was a little confusing as it could also relate to a "control run".

Agreed and changed (Equation 1, page 7).

Line 140: "transitional areas" is not clear. Is this a transition from one regime to another? Why is it a transitional area rather than just a "moisture" limited region where soil moisture plays a larger role. This is the language used in coupling papers such as (Dirmeyer, 2011) or Koster et al, 2009).

Right, the "transition areas" term can be confusing. We have changed this term by

"water limited areas" (line 165).

Line 143: While the jargon "vegetation activity" may be used with the VAC coupling index – it is a term that is not commonly used, and no meaning to me when reading the paper. Please define what you mean by "vegetation activity" before using the jargon.

We have replaced all vegetation activity terms by vegetation photosynthetic activity to be more accurate (lines 168, 170 and 173).

Lines 140-145: There is a lot of uncommon jargon (see points above) and I think this section should be revised to make sure people less familiar with using the VAC to estimate coupling can follow what the different regimes are and why they are that way.

We have revised this section, and we think it is clear now (lines 161-174).

Line 160-170: I think this section is critical to include in your paper. Many people who study climate extremes and climate impacts will ask why not just "bias correct" the data. You list very good reasons for this – and I agree you need to look at the absolute value of these terms to really see the differences the LSMs are causing. However – the way this is worded is confusing. I would suggest removing the concept of "bias correction" from this paragraph (lines 161-64) and just discuss the reasons to use absolute and statistical percentile data. Then following that discussion, add in that bias correction is often employed bout would break physical relationships etc. Just a suggestion to improve readability and flow.

Great suggestion, we have changed the flow of the paragraph in the new version of the manuscript (lines 189-200).

Line 175: While I think it is great you include results from NA-CORDEX – I think the context of why you do this analysis needs to be better justified early on (e.g. motivate in the intro better and then remind the reader why you are doing this in the methods).

We have improved the context for the use of the CORDEX simulations in the introduction and in the methodology as suggested (lines 63-66, 89-91, and 211-215).

Line 176: When using existing model simulations, you need to check their data use policy and make sure you appropriately cite the data. There is a DOI that must be included in your paper for NA-CORDEX (see: https://na-cordex.org/) Mearns, L.O., et al., 2017: The NA-CORDEX dataset, version 1.0. NCAR Climate Data Gateway, Boulder CO, accessed [date], https://doi.org/10.5065/D6SJ1JCH

We have included this reference in the main text (see line 214).

Paragraph starting on line 175: You have not included enough information about the NA-CORDEX simulations used in your study for the reader to understand the results shown in the paper. Please include the LSMs used and some information about their differences (https://na-cordex.org/rcm-characteristics.html). For example WRF does use the NOAH model – how different is this from your WRF runs. Some of the models (WRF) use nudging and the others don't, this could cause differences. Also there appears to be more 50km NA-CORDEX simulations with ERA-I boundary conditions (https://na-cordex.org/simulation-matrix.html), why have you only chosen these three?

The NA-CORDEX information about RCM differences was summarized in table S1, now included in the main text as Table 3 in the new version of the manuscript. As the reviewer indicates there is a WRF simulation employing the NOAH LSM. However, this simulation used nudging to match the boundary conditions from the ERA-Interim reanalysis. The comparison of our WRF-NOAH simulation and the one included in the CORDEX ensemble (second column in Figures S11-13 and third column in Figures S17-S19 in the supplementary information) shows similarities in the spatial pattern of the extreme indices with some differences in the index values, like it is shown for the other 2 CORDEX simulations. The spatial similarities suggest that the topography, land cover and the latitudinal gradient may be driving the spatial features of these results. The specific differences between our WRF-NOAH simulation and that included in the CORDEX project are probably driven by the boundary conditions because in addition to be different, the CORDEX WRF simulation used nudging techniques to match the employed reanalysis product. We have included this discussion in the new version of

the manuscript (lines 476-484).

Furthermore, the NA-CORDEX project includes evaluation simulation for three extra RCMs (the RCM4, the HIRHAM5 and the REGCM4 RCMs). However, all these simulations start in 1989 and end in 2009 or 2011. The use of these simulations would reduce our analysis period significantly, so we decided to use the three simulations providing data for the analysis period of our simulations. We have included this justification on the text (lines 218-219).

Section title for 4.1 – You do not do an "evaluation" of the WRF simulations as there are no observations to evaluate the quality of the of the WRF coupling – I think a better word would be "examination" or "comparison"

We have gone through the text and replaced the "evaluation" word by a more appropriate term in the cases where it can be confusing (e.g. lines 5, 26, 31, 85, 93, 147 and 223).

Figures 1+2: Are all possible cases captured in the 4 VAC categories? Should the sum of all VAC categories equal 100

Figure 3: This may be a draft quality issue but it is difficult to read the numbers under the labelbars.

We have modified this Figure to enlarge the color scales and improve the quality of the figure.

Lines 230-233: This information should be in the figure caption.

We have included these lines in the caption of the figure.

Discussion around 345: The WRF NA-CORDEX simulation is a different setup than the model simulations you performed, however it uses the NOAH LSM. Many readers could be curious about how the WRF NA-CORDEX experiment compares with the experiments in this study. Also a discussion of how the NA-CORDEX WRF simulation

is different from your WRF simulations would be useful.

The comparison of these simulations (second column in Figures S11-12 and third column in Figures S17-S19 in the supplementary information) show similarities in the spatial pattern of the extremes with some differences in the index values, like it is shown for the other 2 CORDEX simulations. The spatial similarities suggest that the topography, land cover and the latitudinal gradient may be driving the spatial features of these results. The specific differences between our WRF-NOAH simulation and that included in the CORDEX project are probably related to the different boundary conditions because the CORDEX WRF simulation uses nudging techniques to match the employed reanalysis product. We have included this discussion in the new version of the manuscript (lines 476-484).

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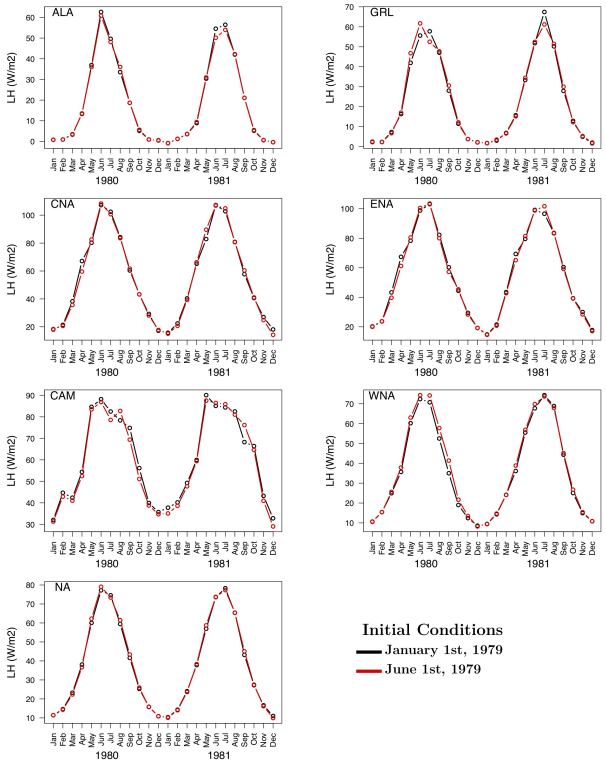

Fig. 1. Monthly time series of latent heat flux averaged over North America (NA) and the subdomains included in the manuscript. The black line corresponds with the WRF-CLM4 simulation employed for our analysi

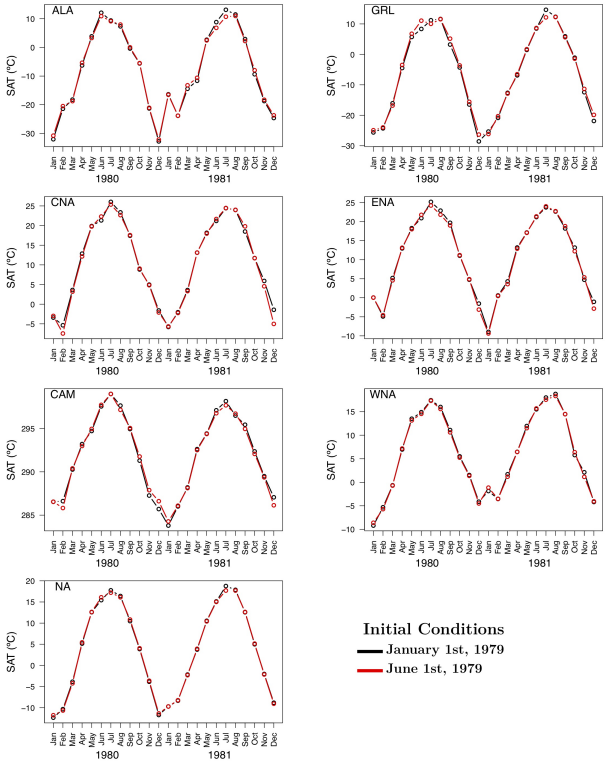

Fig. 2. Monthly time series of surface air temperature averaged over North America (NA) and the subdomains included in the manuscript. The black line corresponds with the WRF-CLM4 simulation employed for our