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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.

Anonymous Referee #2

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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.

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.

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.

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.

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.

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".

Line 28: add "the" before IPCC.

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.

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).

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

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?

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.

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".

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).

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.

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.

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.

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).

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-COREX (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

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?

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”

Figures 1+2: Are all possible cases captured in the 4 VAC categories? Should the sum of all VAC categories equal 100%? This would be useful.

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

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

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.

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