Hydrol. Earth Syst. Sci. Discuss.,
https://doi.org/10.5194/hess-2020-199-RC1, 2020

Hydrology and Earth System Sciences
Discussions
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Interactive comment on "Simulation Analysis of Local Land Atmosphere Coupling in Rainy Season over a Typical Underlying Surface in the Tibetan Plateau" *by* Genhou Sun et al.

Anonymous Referee #1

Received and published: 20 July 2020

This study investigates the local land-atmosphere coupling over a site in the Tibetan Plateau using the WRF model in a nested domain configuration. The experimental design uses several different LSM and PBL scheme combinations for a 36-hour simulation on a case study day in which many observations exist. The Tibetan Plateau is an area of great interest for water resources priorities, so this type of investigation is quite relevant to a broad community. The results show that the model coupling is sensitivity to the LSM and PBL scheme combination, as well as to the initial soil moisture. These differences ultimately cause changes to convective cloud development.

The paper is generally well-written and the scope is manageable and interesting. That

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Discussion paper

being said, there are a few areas in which the manuscript should be revised before publication. In particular, the spatial soil moisture map derived from LAI seems to have unreasonably high values of volumetric soil moisture. In addition, although most of the results are discussed appropriately, there are a couple of statements in the results that are not supported by the figures or analyses as they are currently presented. These issues are discussed in more detail below in the major comments.

Major comments:

1) Lines 121-130: The spatial soil moisture map derived from LAI and shown in Figure 2b has a lot of very high values (>∼0.6). This is worrisome for two reasons. The first is that volumetric soil moisture is not usually that high, so I'm skeptical of how realistic this map is. What are the observed values of soil moisture at the site of interest on this date? What are the min and max soil moisture over the course of the year for this site? Although it is only for one site, this type of analysis could give insight into what is a reasonable max soil moisture for this area. The second potential issue with soil moisture values this high is that they will not even really be used in the Noah LSM. If the authors are using the standard soil texture classifications and soil parameter tables, each soil class has a maximum soil moisture. The maximum soil moisture value varies for each class but even the highest max soil moisture value is less than 0.5. Therefore, all of these high values will essentially be reset to the max SMC for the texture class anyway, making the map more dependent on the soil texture map than on the derived soil moisture from LAI.

2) Section 2.2: Please provide more information on the experimental design. The analysis is 8:00-17:00 local time on August 7, 2011 and the total simulation length is 36-hours, but when is the simulation initialized? Please specify the exact date and time. This is important for understanding the divergence in the starting point in the mixing diagrams between the Noah runs and the CLM runs. The Noah run is wetter and warmer than the CLM run, but they start with the same initial soil moisture and are being forced by the same atmospheric data, right? The amount of time and the time of

day that has passed between the initialization and the figures shown are necessary to understand these differences.

3) Figure 8 does not appear to support the description given of the figure. For example, lines 248-249 say that the soil moisture pattern corresponds with the LEsfc and Hsfc. Except for a couple of spots in the higher elevations in the southwest part of the domain, I don't see how these patterns match up. Perhaps the scale on the figure is not doing the pattern justice? If so, please revise. Otherwise, perhaps the LEsfc corresponds better with the vegetation pattern?

Minor comments:

1) Line 35 describes the Tibetan Plateau as being the Asian Water Tower (i.e., 'also known as'). Line 41, 'TP's role in Asian Water Tower' implies that they are two different things and the TP may affect the Asian Water Tower. Please correct and clarify.

2) Line 98 refers to Fig. 1c, but there is not a Fig. 1c.

3) Lines 129-130: This statement about how the soil moisture was extended to the lower levels is unclear. Was the derived top layer soil moisture used for the entire depth down to 40 cm (so it is uniform vertically)? Please clarify.

4) Line 142: Please specify the exact start date/time that the run was initialized.

5) Lines 148-149: Unless a modification was made to the Noah LSM in this study, the Noah LSM uses a static vegetation dataset. The Noah LSM is the land model for the GFS model. The way the statement is written, it sounds like the Noah LSM is using output from GFS for vegetation, which is not correct. Please revise.

6) Section 3.1: At 8am, the CLM simulations are starting cooler and drier than the Noah simulations. Is this because of the initial conditions at the start of the coupled runs? Is it because of vegetation or other differences in the way CLM and Noah calculate fluxes? This is an important point and should be explored further.

7) Section 3.1: In most of this section, the paper states the statistics and which runs have more or less flux, but there is little explanation of physical explanation behind the statistics. Please consider adding more physical explanation as it would be more interesting to reader if these statistics were translated into what is physically happening in the PBL to cause these differences.

8) Line 263: Does this approach only exclude the water points or also the gridcells nearby water? Why not dismiss the grids where the SM is 1.0 or the land cover is water instead?

9) Lines 266-267: The larger spread in EF seems to imply that there is more surface heterogeneity in Noah than CLM, right? What is the dominant factor causing this? The initial soil moisture is the same for both Noah and CLM, right? Does that mean that it's the treatment of vegetation and/or soil parameters? This should be explored more because it seems to be an important factor of these differences between LSMs.

10) Line 267-268: "...the simulation using BouLac produces closest result to the observation, which agrees with the results in this study." What variables/metrics are being used to determine that Noah-BouLac is the closest to observations for this study? Based on Figure 6, the Noah-BouLac is not the closest to observations. Please clarify and explain.

11) Figures 6 and 7 show the same runs, but the color scheme is different. Please keep a consistent color scheme between these two figures so that it makes it easier on the reader to follow. Also, if possible, please consider reducing the number of colors shown here. You could do that by assigning one color to each PBL scheme (for example orange to YSU) and then using an open icon (i.e., unfilled, just the outline) for CLM and filled icon for the Noah for Figure 6. For Figure 7, you could use one color for each PBL scheme again, but dashed line for CLM and solid for Noah.

Technical corrections:

1) Line 24: conductive should be conducive

2) Line 64: in-sit should be in-situ

3) Line 163: remove ‘at the daytime’

4) Line 163: ‘furthered this study’ -> furthered this ‘method’ or ‘technique’ might be more appropriate than study.

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