

## RC #1

Review of GMD submission gmd-2023-55 "Representation of atmosphere induced heterogeneity in land-atmosphere interactions in E3SM-MMMFv2" by Le et al.

Recommendation: accept *after major revisions*

The manuscript reports analysis of simulations applying superparameterized GCM, the MMF, with three different couplings between atmospheric model and the land-surface model. Overall, I feel this manuscript reports analysis that is superficial and can be significantly expanded to make the comparison more meaningful. In addition – if I understand the model setup correctly – the setup does not allow a possibly significant aspect of the atmosphere-land surface interactions through the development of the subgrid-scale contrasts of surface characteristics. Finally, I am not sure if the GMD is the proper journal to report the simulations results. Below I discuss those points in more detail. The paper should be accepted only after those points are appropriately addressed. I may have more comments after the major issues are addressed and the manuscript is revised.

[Authors would like to thank the reviewer for his time and effort in providing us the valuable comments and suggestions. Our respond is colored blue below.](#)

Specific major comments:

1. I will leave it to the editor to decide if GMD is the appropriate journal for this submission. I personally feel JAMES would be more appropriate as the paper does not report any model development, just the impact of various possible couplings between the GCM's atmospheric and land-surface components.

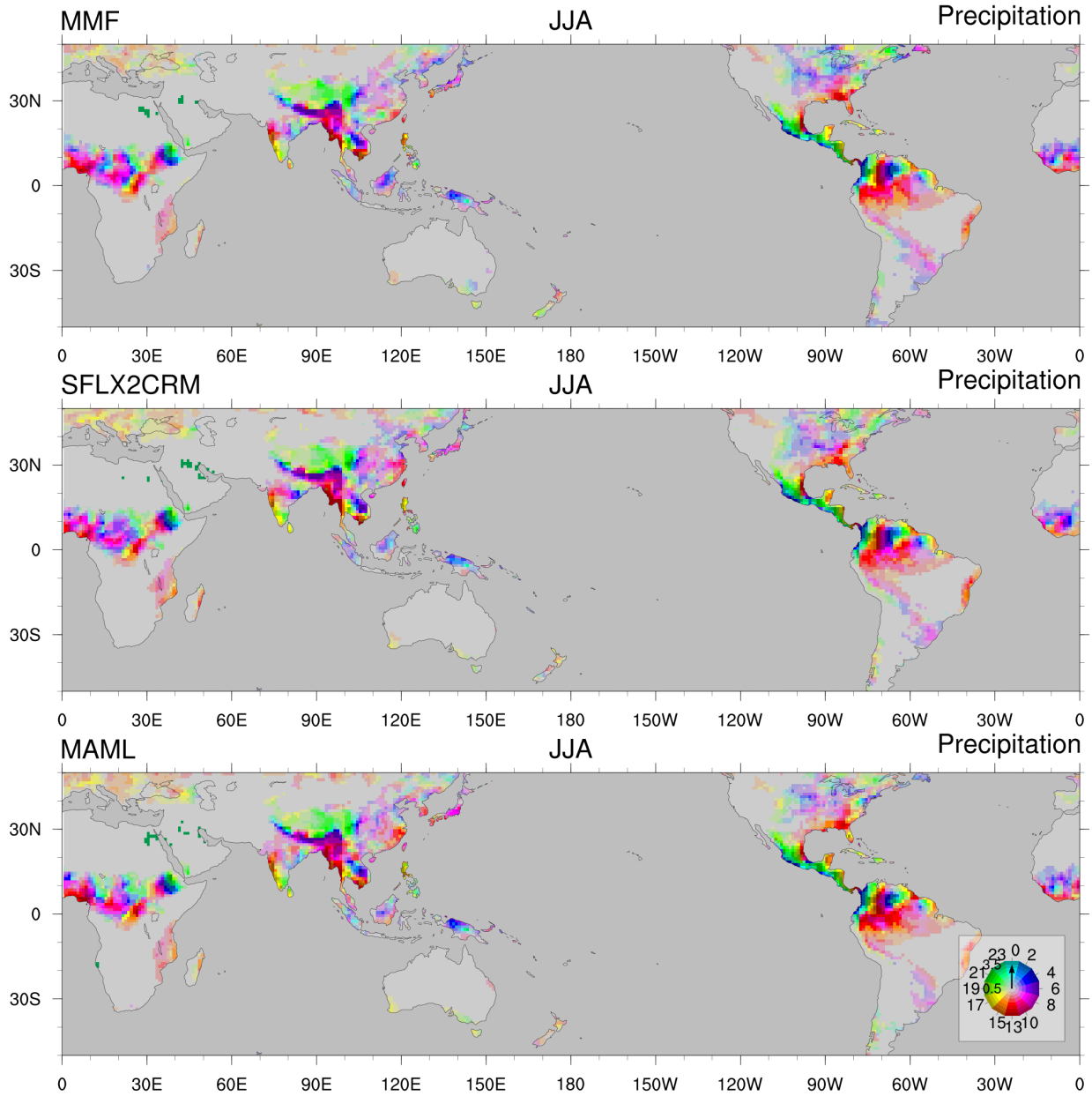
2. The analysis focusses on temporally and spatially averaged data. However, over land, the key is the diurnal cycle and how the surface characteristics impact it through surface heat fluxes. In essence, Bowen ratio of the surface heat flux impacts the surface buoyancy flux and this has a strong impact on daytime convection development. The paper that comes to mind (and references therein) is:

Thomas, L., N. Malap, W. W. Grabowski, K. Dani, and T. V. Prabha, 2018: Convective environment in pre-monsoon and monsoon conditions over the Indian subcontinent: the impact of surface forcing. *Atmos. Chem. Phys.*, 18, 7473-7488.

So the first suggestion is to look at the differences in model results of the diurnal cycle, not just at averaged heat fluxes between 6 to 18 local time. The analysis reported in Yang and Slingo (2001) can be repeated using model results with the focus on land.

Yang, G., and J. Slingo, 2001: The Diurnal Cycle in the Tropics, *Mon. Wea. Rev.*, 129, 784-801.

Authors thank you for the suggestion for further analysis. We looked into the diurnal cycle of precipitation; however, we did not see significant differences in the diurnal precipitation (both phase and amplitude) regardless how we couple land and atmosphere models. Figure below shows the Evans plot of the diurnal cycle of precipitation. Hue represents diurnal maximum timing and saturation represents diurnal amplitude. As shown in the figure below, there is slight difference in the diurnal maximum timing in Amazon and central Africa, but they are not significant. However, in our daytime mean analysis, we do find that the surface buoyancy is stronger in MAML case (Figure 10), which helps to clear up the lowest model level condensation (Figure 11). This is the most significant impact that we could find using three difference land-atmosphere coupling method.



The second suggestion is that one should look at the differences at various climatic zones, for instance, tropical versus subtropical versus summertime extra-tropics, and versus dry and rainy season in the tropics and subtropics. Analysis along those lines would make modeling results more convincing. I appreciate the fact that such information is to some extent provided by the global maps (e.g., Fig. 2), but selecting specific areas (e.g., US Midwest, Amazon, India, Europe, etc.) and showing local maps would bring some interesting differences.

What we find by inspecting each cases (MMF, SFLX2CRM, and MAML) from Figure 1 through 6 is that the most pronounced differences are located over the tropics where the surface net radiation is high, where there is less seasonal differences. We do find that in those areas, change in soil moisture, change in EF, change in near surface specific humidity and precipitation is positively correlated. We show this by presenting the scatter plot between those variables over in Amazon as an example and we added the figure 8 (shown below) in the text. Therefore, instead of focusing on specific regions separately, our analysis focuses on the tropical belt where the difference is relatively significant in comparison to other areas.

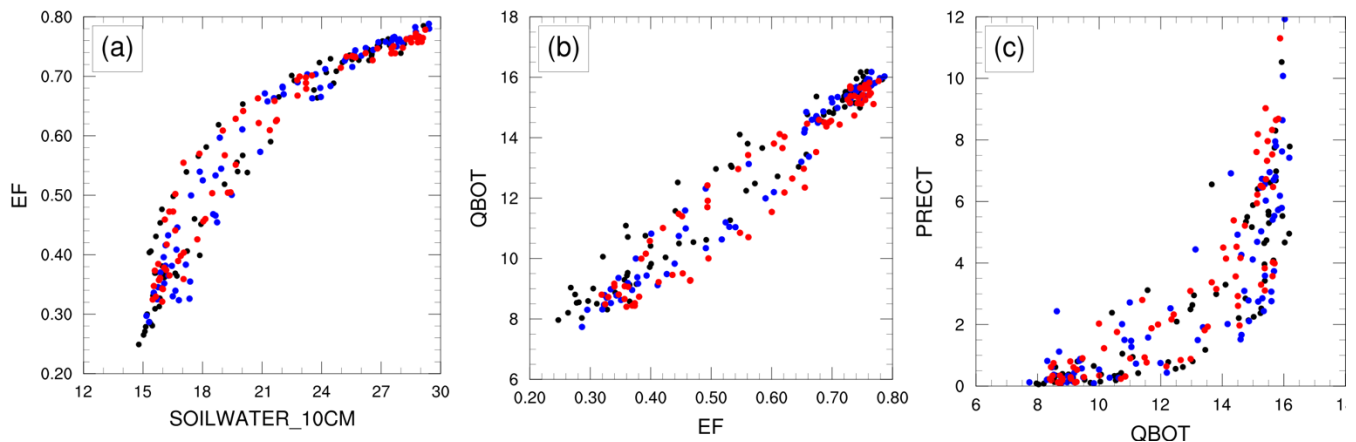


Figure 8. Scatter plots of 5-day averages of (a) near surface soil moisture versus evaporative fraction, (b) evaporative fraction versus near surface specific humidity, (c) near surface specific humidity versus precipitation averaged over Amazon. MMF, SFLX2CRM, and MAML are denoted by black, blue and red dots, respectively.

3. As far as I understand the model setup, land surface models are not allowed to develop small-scale variability in the MAML setup. I think this is a significant drawback of the methodology not discussed in the paper. To what extent such variability is important for the problem the paper investigates? For instance, if rain falls in a fraction of the CRM domain (e.g., because of convection), then the land surface is modified beneath those CRM columns. As a result, CRM can develop circulations that can affect subsequent evolution of small-scale flow and subsequent convection development. This is not possible in the current setup, correct? That said, the wind that plays important role in atmosphere - land surface interactions seems to be taken from the GCM (per l. 71). Again, this is unrealistic.

In SFLX2CRM case, yes there is no intra CRM heterogeneity in the land surface model. Entire CRM **domain** is coupled to a single land model, therefore even if there is precipitation in one part of the domain, the land model won't feel this heterogeneity in the spatial distribution of precipitation as an input to drive land surface processes. However, in MAML, we couple each CRM **grid** to an independent copy of land model.

For instance, in our set up, each CRM domain is configured with 32 grid points. Entire CRM domain is coupled to a 32 independent copy of land models, that is, one land model per CRM grid. Therefore, if only a portion of CRM grid develops precipitation, only the land model beneath those specific CRM grid will retrieve those precipitation as an input to drive land surface processes. In MAML, atmosphere induced heterogeneity in land-atmosphere coupling inside CRM domain is represented. We add this information in section 2.2 and also in conclusion.

In the MMF, we use 2D CRM, therefore meridional wind component can be unrealistically strong. As a land model input, we need wind speed to compute surface fluxes. We were concerned that using the wind speed computed directly from the 2D CRM wind fields would yield unrealistic surface fluxes. As an alternative we used GCM wind speed near surface to pass onto the land model. We did not test the implication of using GCM wind to compute surface heat fluxes. We added this information in L129-131 and is given below:

L129-131: Using EAM wind to drive land surface processes and grouping neighboring CRM columns for radiation calculation decreases heterogeneity induced by atmosphere in MAML. We did not test how these modifications would affect the resulting climate.

There is also another issue that requires a little more detailed explanation of the coupling between GCM and CRM models. I assume CRMs are aligned in a specific way and predict small-scale fluctuations of only one horizontal wind component (i.e., along the 2D domain). But the total wind can be taken from the GCM to create local (i.e., CRM's) 2D surface winds. Such winds should be used in the land model to calculate surface fluxes.

In E3SM-MMF, there is large scale forcing from GCM to CRM but no momentum feedback. Therefore, CRM wind is effectively nudged toward GCM wind rather than coupled. Therefore, we had to use GCM winds.

4. This comment is related to 2 and 3 above. For the diurnal cycle over land, it has been shown in high-resolution simulations that the interactive surface fluxes (in contrast to the prescribed horizontally-homogeneous surface fluxes) have a noticeable impact on moist convection diurnal cycle (e.g., Kurowski et al. 2018). It is unclear to me if that aspect can be considered in the way models are coupled in the current setup. This might be important for the land-atmosphere coupling. Finally, the CRM does not include CRM-scale topography. This is an important drawback of the atmosphere-land coupling in MMF. Perhaps this should be appropriately stressed in the manuscript.

Kurowski, M. J., K. Suselj, W. W. Grabowski, and J. Teixeira, 2018: Shallow-to-deep transition of continental moist convection: cold pools, surface fluxes, and mesoscale organization. *J. Atmos. Sci.*, 75, 4071-4090.

Yes I am aware of previous CRM and LES studies that looked at the heterogeneous surface fluxes that can induced secondary mesoscale circulations and indeed impact on the water cycle over land. However, in the current set up of MAML, even if each CRM grid is coupled to its own land surface model, land surface characteristics across the multiple land surface models within a CRM domain remains the same. Therefore, the set up is missing the heterogeneity induced from the heterogeneity in the land surface characteristics including topography. In MAML framework, where each land model instance can read in its own initial land condition file, theoretically, we can prescribe difference land surface types to each land model instance within a given CRM domain. However, we are using 2D CRM, therefore translating the observed land surface heterogeneity into the 2D space is a non trivial matter and out-of-scope of this study. We modified the existing sentence to stress that the CRM does not include CRM-scale topography in Line 127-129.

Line 127-129: These nx copies of ELM are prescribed with the same land surface characteristics. Also, CRM does not include CRM-scale topography. Therefore, this method does not fully represent a significant level of surface heterogeneity.

## RC #2

Representing land-atmosphere interactions is crucial for weather and climate models. Lee et al., manuscript implements three different approaches to represent land-atmosphere interactions at different spatial scales within the multi-scale modeling framework (MMF) of DOE E3SM model. It also shows the difference in 5-year climatology between the simulations with these three approaches. This manuscript provides valuable guidance on the model development in sub-grid land-atmosphere coupling and helps understand the soil-moisture, PBL, and precipitation interaction mechanisms. My comments are shown below.

### Major comments

1. The current figures should be presented in different orders. Sec. 3.1 covers Figure 2-6, and Sec. 3.2 starts with Figure 2 again. Readers have to go back and forth to look up the correct figures. Thus, the figures for climatology from MMF simulations should be grouped together and placed first, followed by the difference between MMF and SFLX2CRM, and MAML simulations.

We appreciate your suggestion. Many times, in the text, we try to point out the regional changes are interconnected among different variables to qualitatively represent land-atmosphere coupling. Even if we group global climatology and differences together, readers should still have to go back and forth to follow the text. We apologize for this inconvenience, but we would like to leave the order as it is.

2. I agree that MAML makes minor changes in 5-year climatology simulations on a global basis as shown in Table 1. However, the spatial figures do show some non-negligible regional changes. For instance, the changes in CRES and precipitation over Amazon, North America, East Asia, Central Africa and etc. in Figure 6 (e) and (f) show noticeable differences (up to ~20% estimated from the naked eye). Therefore, in addition to the global mean change, the authors should emphasize the regional changes in the text, at least in the Abstract and Conclusion parts.

We thank you for the suggestion. We added a couple sentences throughout the text to highlight the regional differences. Those changes are given below

L12-13: 5-year global simulations are run using these 3 coupling strategies and we find some regional differences but overall small change whether a land model is coupled to CRM or a global atmosphere.

L191-192: For example, precipitation and cloud radiative effect at surface shows noticeable differences in SFLXCRM and MAML over Amazon, North America, East Asia, Central Africa regions.

We also provide additional regional differences based on the vegetation types throughout the text. Since the largest difference between model configurations exists in the tropics where the net surface radiation is the largest, we do focus on those region as a whole

3. The authors attribute the differences between the MAML and MMF simulations only to the change in heterogeneity in land-atmosphere coupling over the CRM domain. This is not the whole story. The difference is also due to the different ways of coupling between MMF and SFLX2CRM. And based on Figures 2-6, the differences between MMF and SFLX2CRM (MMF-SFLX2CRM) are larger than that due to the change in heterogeneity in land-atmosphere coupling over the CRM domain (MAML - SFLX2CRM). So the authors should discuss more on the MMF-SFLX2CRM difference. What are their differences in terms of the land-atmosphere coupling in the model? How does that translate to the difference in climatology as shown in Figure 2-6? The current manuscript fails to give a clear answer.

Global mean maps shows some areas in SFLX2CRM with larger difference than the areas in MAML, but the global means of SFLX2CRM is very similar to the means of MMF as shown in Table 1. Also, We discuss the SFLX2CRM-MMF difference focusing on the buoyancy, TKE and cloud liquid water difference in Section 3.4. In section 3.4, we discuss that the buoyancy and TKE changes with interactive land surfaces, in our case both SFLX2CRM and MAML. But the atmosphere induced heterogeneity in MAML drives stronger PBL turbulence. We also modified a sentence in L300-301 in a text to stress our point.

L300-301: We find that global means of SFLX2CRM is similar to that of MMF. However, MAML tends to produce drier and warmer surface weather.

### **Minor comments**

Line 71. and Line 76-77. The authors state “use the wind velocities from EAM in all three methods” and “grouped 2 neighboring CRM columns”. Does this mean that the



heterogeneity of wind and radiation is reduced in the model? How would that affect the conclusions in the manuscript?

Yes, heterogeneity of wind and radiation was reduced in the MAML due to the fact that EAM wind was used to force the land model and we grouped 2 neighboring CRM columns for radiation computation. We decided to use EAM wind because the possibility of too strong meridional wind component of 2-D CRM. We had no other option but to use EAM wind. Also, grouping 4 neighboring CRM columns for radiation was tested against no grouping case, we found negligible differences in the result climate. We didn't test how these choices would affect the land-atmospheric interaction processes in the model. We added below sentences in the text to include that information.

L129-131: Using EAM wind to drive land surface processes and grouping neighboring CRM columns for radiation calculation decreases heterogeneity induced by atmosphere in MAML. We did not test how these modifications would affect the resulting climate.

Line 95-97. Not clear what the differences are between MMF and SFLX2CRM in terms of implementation. The standard MMF method does not allow the development of surface buoyancy in the PBL?

In the standard E3SM-MMF, buoyancy is not directly passed to the CRM. Instead, buoyancy is passed only to EAM. Temperature/moisture profiles in EAM changes according to the given buoyancy. Then CRM feels the buoyancy indirectly by the large scale forcing from EAM. On the other hand, in SFLX2CRM, we pass buoyancy directly to CRM as a bottom boundary condition. So in SFLX2CRM, PBL development is directly driven by the surface buoyancy. This information is found in L111-112 and L118-120.

Line 128. Not exactly. The strength of sensible heating is also related to solar radiation. For instance, the sensible heating flux is low over the Antarctic with no vegetation cover.

I meant to say the bare soil area in the tropical, sub-tropical regions such as deserts produces stronger sensible heat flux. I modified the sentence as given below:

L159-160: On the other hand, regions in the tropics and the sub-tropics with no vegetation cover (1) produce stronger sensible heating as shown in fig.2a.

Line 144. Change "signification" to "significant".

Thank you for catching this typo. We changed 'signification' to 'significant' in Line 170 in the revised manuscript.

Table 1. The last column should be "MAML"?

Thank you for catching this typo. We changed 'MMF' to 'MAML' in the last column of Table 1.

Line 160. "Fig. 3" is meant to be "Fig.2"?

Thank you for catching this typo. We changed 'Fig 3' to 'Fig 2' in Line 187 in the revised manuscript.

Line 172. How did the authors get the number of 3.8 W/m<sup>2</sup>?

It's computed based on the average direct vegetation evaporation on broadleaf evergreen tropical trees. It's the difference between MMF and MAML case.

Figure 7. Can the authors add legends to show what the scatter dots with different colors stand for? As well as to clarify what different pft numbers are corresponding to?

Thanks for the suggestion. We added color dot legends in the figure panel, and added vegetation type instead of pft in the figure.

Line 187. It is not straightforward to see "how the lower-atmosphere condition is influenced by different coupling methods" from Figure 5. Can the authors elaborate on that?

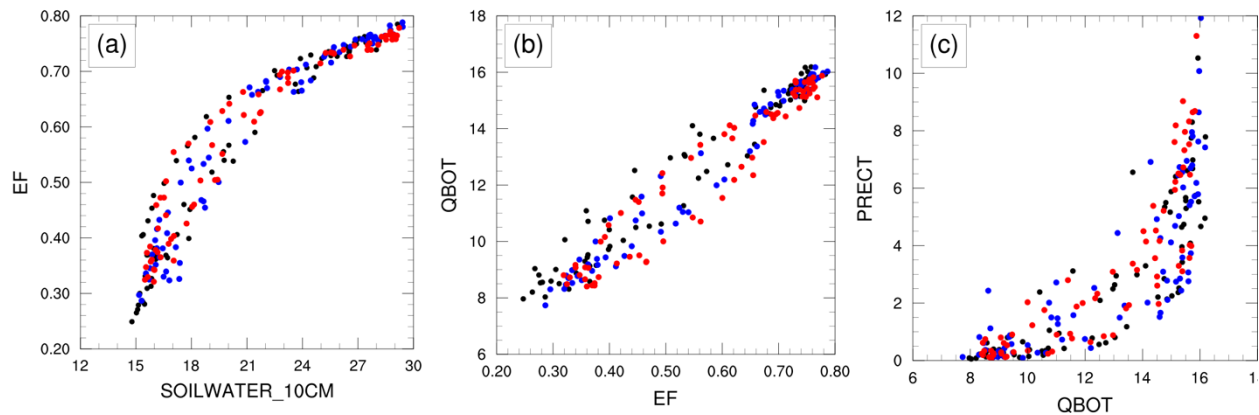
We edited the sentence as given below:

L230-232: Fig. 5 shows how the lower-atmosphere meteorological condition is influenced by underneath surface heat fluxes and soil moisture. Regions with higher (lower) evaporation aligns with regions with humid (dry) PBL.

Line 188-192. These are qualitative descriptions of the soil moisture-PBL-precipitation interactions. Can the authors quantify the interactions to show how different coupling strategies affect these interactions?

To show the correlation between each segment of soil moisture – PBL – precipitation interactions, we added a new figure shown below. This scatter plot shows that there is positive correlation exists between EF and SOILWATER\_10CM, SOILWATER\_10CM and

QBOT, and QBOT and Precipitation. We added the figure shown below into the manuscript and added additional sentences in Line 233-239, which is also given below.



L233-239: Figure 8 shows scatter plot for each segment of soil moisture-PBL-precipitation interactions. These scatter plot presents that positive correlation exists between soil moisture and evaporative fraction (EF), EF and near surface humidity, and near surface humidity and precipitation. There is no noticeable differences between each cases.

Line 197-198. "strong spatial heterogeneity in land surface process is found where the MAML differs from the MMF the most". Can the authors explain more on how did they reach this conclusion?

We modified the sentence, and the new sentence is given below:

We use the standard deviation as a measure of CRM scale spatial heterogeneity (Fig. 8). Strong spatial heterogeneity in land surface processes is found where the MAML differs from the MMF the most by visually inspecting Fig. 2, 4-6.

Line 202. The standard deviation alone cannot reflect the difference well in spatial heterogeneity between different variables in the lower atmosphere and the land states. Instead, the standard deviation normalized by the land grid mean is a better way for the comparison between different variables.

Yes, In some cases, we agree that normalized standard deviation may be a better way for the comparison between different variables. But here, sensible heat flux, latent heat flux, and precipitation has regions where the land grid mean is close to zero, thus, gives us very strong normalized deviation regardless that the non-normalized standard deviation is very small or not. We could see large normalized standard deviation because the land grid mean value is very small, which is against what we are trying to show. Therefore, we decided to leave the figures as is. Thank you for your suggestion.

Line 208. How is the buoyancy flux diagnosed?

Buoyancy flux is diagnosed inside the code as

$$BFLX = \frac{SHF}{c_p} + 0.61T_s \frac{LHF}{L_v}$$

Where SHF (LHF) is sensible (latent) heat flux,  $c_p$  is specific heat of air,  $T_s$  is near surface temperature, and  $L_v$  is latent heat of vaporization. For MAML case, BFLX is computed for each land model instance. We added above information in L259-263.

Figure 10. Why do the MMF and SFLX2CRM simulations show almost the same TKE profiles, but quite different liquid cloud contents?

It is the difference between whether surface buoyancy flux is supplied as a bottom boundary condition to stir turbulences in PBL or not. In MMF, CRM does not receive the surface heat flux information in the code, therefore computes TKE inside CRM based on the vertical gradient of thermodynamic profiles. Whereas in SFLX2CRM, surface heat flux is directly coupled to CRM so surface heat fluxes are used to compute the TKE at the bottom most interface level. You can see that TKE profile is slightly higher in SFLX2CRM near the bottom. The reason why TKE profile is similar between MMF and SFLXCRM above the surface level can be explained that TKE is computed based on the thermodynamic profile and both cases have similar thermodynamic profiles.

We added below information in the text:

L282-285: In MMF, CRM does not receive surface heat fluxes from ELM, therefore computes TKE based on the thermodynamic profile. Since MMF and SFLX2CRM has similar thermodynamic profile, both produces similar TKE profile. However, near the surface, SFLX2CRM has slightly higher TKE due to the non-zero surface buoyancy flux. This is the reason why there is a large difference in the liquid cloud water profile when the TKE is very similar.