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

Overall, I am disappointed with the revision. The revised manuscript shows some minimal changes when compared to the initial submission. Maybe this is because there is not that much to show in terms of the difference between various model configurations. However, I do not think so.

Please think about the bigger picture: The key conclusion of this investigation can be summarize as follows: coupling copies of the land model to all columns of the embedded small-scale models (i.e., many land models per a GCM column) rather than using GCM fields to drive just one land model in a GCM column gives very little difference when averaged over long time (years). Does that imply that small-scale land-atmosphere coupling is irrelevant for climate? Or maybe it shows limitations of the superparameterization approach? Or maybe some differences (that I expect are there) smooth out when averaged over time? My vote is “no” for the first question, and “yes” for the question two and three. The simulations discussed in the paper should be capable to provide answers to those questions as well.

Right now, the paper shows that the results averaged over 5 years show very little difference (e.g., table1). However, I suggested in my first review that the authors looked at shorter-time scale processes, such as diurnal cycle or the impact of interactive surface fluxes on convection development over tropical or warm-season midlatitude continents. In their responses, the authors dismiss my suggestions. They state that there is little difference in the diurnal cycle of precipitation. However, is there any impact of the small-scale land-surface model on convective development? For instance, surface characteristics should show smallscale differences (e.g., availability of the surface water) when the land model is coupled to CRM columns, correct? That should affect convection development over the next day, correct? Perhaps such differences smooth out when long-term statistics is gathered, but this remains to be shown. Moreover, the authors’ response to my suggestion to look at the role of interactive surface fluxes seems awkward. I do not suggest to use a different land-surface type is each CRM column. However, a small-scale precipitation pattern should develop gradients of the surface characteristics (soil moisture in particular) even if the same land surface type is used across all CRM model columns.

In summary, I still maintain that the analysis presented in the paper is superficial. One way to make it more interesting would be to contrast short time scale processes (as briefly discussed above) and long-time averages. In addition, looking at global maps is rather uninteresting way to point out differences. Can the authors be a little more creative? For instance, select various land-surface types in a given climatic zone, and select some characteristics for each land-surface type. Something along Fig. 7, for instance.

Despite my criticism, I do not want to delay publication of this manuscript. So my recommendation is to accept after minor revisions. I have several specific minor comments that the authors should address before the manuscript is accepted. Many of those comments apply to the initial submission as well. I did not report them as I thought the major issues needed to be addressed first.

Specific comments:

We appreciate the comments and suggestions given by the reviewer. Please see our response in blue below.

1. This comment was addressed by neither the authors nor the editor: "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."

I have not received any correspondence from the editor either. So I assume the paper is still assumed to be considered as a GMD submission. I maintain the submission is more appropriate for JAMES.

We appreciate the reviewer's suggestion that this manuscript would be more suitable for JAMES. However, this paper was intended to introduce 3 different methods in land-atmosphere coupling within E3SM-MMF and provide initial comparison of the impact of those methods on long-term climate. Our result suggests that the way land-atmosphere is coupled in E3SM-MMF leads to rather insignificant impact on long-term climate even though significant more computation resources are required to enable interactive land-atmosphere interaction at CRM grid level. Therefore, we find GMD is an appropriate journal to publish our manuscript.

2. There are numerous small editorial problems. Some of them were in the original submission, perhaps some are new. Below is a list (Lx means the comment applies to line x in the manuscript):

2a. The land surface model is not described/discussed. Please add.
Thank you for the suggestion. We added information about the land surface model (ELM) in line 82-89 and is given as below:
L82-89: E3SM Land Model (ELM) inherits many of its functionalities from its source model, the Community Land Model version 4.5 (CLM4.5; Oleson et al., 2013). ELM simulates hydrological and thermal operations in vegetation, snow, and soil for a variety of land cover types including bare soils, vegetated surfaces, lakes, glaciers, and urban areas. Leaf area index is determined utilizing satellite data and photosynthesis without any constraints from leaf nutrients. Since branching off from CLM4.5, ELM has undergone various improvements (Golaz et al. 2019). The impact of aerosol and black carbon on snow was added. The evaporation was reduced over pervious road under dry condition. The equation for stomatal conductance was revised to avoid inaccurate representation of negative internal leaf CO_2 concentrations. Also, the nighttime albedo over land was updated to 1.

2b. Throughout the text: I am not sure the term "instance" is the best way to describe application of the land model. Maybe "copy" would be better?

For MAML, we modified the ensemble simulation capabilities of E3SM. From there, we inherited the term “instances”. However, I see that “copy” would be more suitable. So, I converted all ‘instances’ to ‘copies’ in the text.

2c. L9: “…coupling multiple land instances to each column..”. I think this incorrect. Only one copy of a land model (or scheme) is applied to each CRM column, correct?
Thank you for catching this. We revised the sentence in line 9 and is given below:
3) coupling a single copy of land model to each column of the CRM grid (MAML).

2d. L19: “Careful” is not a good word here. Please remove as it is not needed.
Thank you for the suggestion, We removed it in L19

2d. L28: “processes … controls”?
Thank you for catching the typo. We changes “controls” to “control” in L28

2e. L35: “…to 25 generalize”?
We removed ‘25’ in L37

2f. L37: “landatmosphere”?
In the word document version that I have has ‘-‘ between land and atmosphere. This could be an error occurred during conversion of docx into pdf.

2g. L74: EAM is not defined.
Thanks for catching it. We defined EAM, and is given below as:
E3SM Atmospheric Model (EAM) in L80.

2h. L80: “resolution” should be “grid length” or “grid spacing”.
Thank you for the suggestion, we replaced ‘resolution’ with ‘grid spacing’ in L93.

2i. L81: “number of vertical model level is”?
Thank you for catching the typo. We changed ‘level’ in the sentence to ‘levels’ in L94.

2j. L84 and L87: “centered year 2000”? “To avoid this caveat”?
We changed ‘centered year 2000’ to ‘centered at year 2000’ in L96, also changed ‘to avoid this caveat’ to ‘to bypass this difficulty’ in L100.

2k. L89: “ELM” is not defined.
ELM is defined as ‘E3SM Land model’ in L82.

2l. L115/116: The sentence “This method…”. First, I think all coupling methods prescribe surface buoyance flux, either indirectly (like the MMF method) or directly, like the two other. In the first two methods, the surface buoyancy forcing is horizontally uniform. In the third method, it can be horizontally heterogeneous. If this is incorrect, then there is something in the methodology that I do not understand. Also “turbulences” is not a word.
Your understanding of the method section is correct. We modified the sentence in L132-133 and is given below as:
This method prescribes surface buoyancy forcing that is horizontally homogeneous.

2m. L.123: Replace "…are prescribed with…" with "feature". Also "nx" is not defined.
Thanks for the suggestion. We replaced 'are prescribed with' with 'feature' in L144. We also added definition of nx – number of horizontal grids – in line 137.
2n. L. 140: "period"?
Thank you for catching the typo. We changed 'periods' to 'period' in L161.

2o. Daytime is defined in the manuscript as the average between 6 and 18 local hour. This is not appropriate for wintertime extratropics where the daytime is much shorter. Perhaps making averages over periods with positive incoming solar radiation would make more sense. If this is too much trouble, just commenting on that would be sufficient.
Thanks for the suggestion. We commented about this in Line 168-169 and is given below:
However, one should note that the day length in extra-tropics is shorter in winter time.

2p. L. 146/147: I dot understand what is meant by "…magnitudes of fluxes increase…". The maximum increase? The range (night time versus daytime) increase? Please explain.
It means daytime mean fluxes have stronger magnitude than the annual mean fluxes. We rewrote the sentence in L167-168 and is given below:
L167-168: In comparison to the 5-year climatology, the magnitudes of daytime mean fluxes are higher than the amount of annual mean fluxes,

2q. L173: "…terrain effect and local-scale land-atmosphere interaction process…". Explain what you mean by that statement. What is "terrain effect"? Are "local-scale land-atmosphere interactions" insignificant in other geographical locations?
I meant the large-scale circulation from terrain by 'terrain effect'. I revised the sentence to reflect that in L200-201. Local-scale land-atmosphere interactions are present in other geographical locations but can sometimes be muted by large-scale land-atmosphere interaction. I was emphasizing that both large-scale and local-scale land-atmosphere interaction is important factor in moisture convection in Amazon.

3. Since the paper use several acronyms (some not defined as pointed out above), I suggest to include an acronym table to help the reader.
We added a Table A1 in Appendix section and is given below:
Line 74: This paper uses several acronyms and Table A1 is added in Appendix to help readers.

Table A1. Table of acronyms

Acronym	Meaning
BFLX	Surface buoyancy flux
CRES	Net cloud radiative effect at surface
CRM	Cloud resolving model
EAM	E3SM Atmosphere Model
EF	Evaporative Fraction
ELM	E3SM Land Model

E3SM	Energy Exascale Earth System Model
GCM	Global climate model
LHFLX	Latent heat flux
MMF	Multi-scale modeling framework
PBL	Planetary boundary layer
PFT	Plant functional type
Q2M	Specific humidity at 2-meter height
RH	Relative humidity
SAM	System for atmospheric modeling
SHFLX	Sensible heat flux
SST	Sea-surface temperature
TKE	Turbulence kinetic energy
TSA	Temperature at 2-meter height
TSOI_10CM	Soil temperature in the upper 10 cm

4. Calculation of the surface fluxes in CRM, lines 85 to 87. I do not understand this logic. To me, the correct way to couple GCM and CRM winds for the surface flux calculation is to combine horizontal wind from the 2D CRM with the second GCM wind component. For instance, is the CRM is aligned along the zonal direction, the wind used in the surface flux formula at each CRM column should be taken as sqrt[(u(x)**2 + V**2)], where u is the surface horizontal wind in the CRM, and V is the meridional wind from the GCM model at the location of the embedded CRM. If this is not correct, then please explain how this is done in the code and why it is not done in the way I explain above.
Unfortunately, we can't combine wind components from GCM and CRM to get the wind speed for the land model. It is because doing so violates the conservation of mass. Also, the friction is applied to GCM winds, so we need the input winds to the surface components to match.

5. Fig. 10. I do not see any black point in the figure. Maybe use three panels or artificially separate clouds of points. I think the difference between red points and all others attest to the role of local circulations that develop because of the horizontal variability of surface characteristics in MAML approach, correct?
As you understood, the purpose of this figure is to show that the red dots (MAML case) is different from the other 2 cases (SFLX2CRM, and default). In Figure 10, the distribution of black points is very similar to that of blue points, that, unfortunately, made the distinction between black and blues points hard. However, we added the regression lines with linear

regression equations on the figure and we think that serves the purpose to compare MAML (red dots) with two other cases, which are very similar. So, we will leave the figure as it is, but we appreciate your suggestion.

6. Fig. 11. Left panel: would it make more sense to average TKE only over locations with strong surface buoyancy flux (for convective situations) or strong surface shear of the mean wind (for shear-driven boundary layer)? The plot shows results over just a few levels in the BL and I am not sure what to think about the significance of that plot. Right panel: I think it shows that circulations that develop in the MAML setup help to remove the "stratofogulus" (e.g., https://doi.org/10.1029/2020JD032619) that often develops in climate model simulations when boundary layer circulations are inefficient in transporting water vapor from near the surface to higher levels.

I wanted to show the lowest level because it shows the largest difference between MAML and other two cases. The meaning of this left panel is that MAML has stronger TKE near surface, therefore more efficient in transporting water vapor from near the surface to higher levels. For the same reason, we think sampling TKE per surface buoyancy or near surface shear is unnecessary.

We added a sentence to denote the presence of 'stratofogulus' in E3SM-MMF and is given below:

L292-294: Gettelman et al. (2020) reported that CAM5 also develops clouds in the lowest-model level layer ("stratofogulus") because boundary layer circulation is inefficient in transporting water vapor from near surface to higher levels.

== END ==