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 small-scale 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:

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.

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.

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?

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?

2d. L19: “Careful” is not a good word here. Please remove as it is not needed.

2d. L28: “processes … controls”?

2e. L35: “…to 25 generalize”?

2f. L37: “landatmosphere”?

2g. L74: EAM is not defined.

2h. L80: “resolution” should be “grid length” or “grid spacing”.

2i. L81: “number of vertical model level is”?

2j. L84 and L87: “centered year 2000”? “To avoid this caveat”?

2k. L89: “ELM” is not defined.

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.

2m. L.123: Replace “…are prescribed with…” with “feature”. Also “nx” is not defined.

2n. L. 140: “period”?

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.

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.

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?

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.

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.

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?

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.

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