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Geoscientific Model Development Discussions

Interactive comment on "Modeling land surface processes over a mountainous rainforest in Costa Rica using CLM4.5 and CLM5" *by* Jaeyoung Song et al.

Anonymous Referee #1

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Song and colleagues present comparisons between observations and two versions of the Community Land Model in Costa Rica.

I totally agree that we need more model evaluations in the wet tropics, which is a pivotal region in the evolution of the future carbon cycle.

But for this we have to firstly compare apples with apples and we secondly need not only model evaluations but also show ways how to improve the models. For example, I think that the comparison between observed PAR and modelled PAR is a very inaccurate comparison because the PAR sensors where shaded by a nearby emergent tree while the model calculated PAR from incoming global radiation above the emergent

tree. One could have also added radiation reahing the ground, which is part of the two-stream approximation, to Figure 3e to compare with the 10 m observations. An example for the second point would be that slope and aspect could have been implemented pretty easily in CLM by simply changing the zenith angle. This does not mean a full implementation of slope and aspect in the whole land surface model as an offline and online model running on the whole globe but it would have shown a way to improve the model.

In this respect, the eddy measurements were surely far from optimal. One should then be also quite cautious in their interpretation. I was really quite worried by the repeated claim that the quantum efficiency of photosystem II should be much lower. This claim comes from simple comparison of uncertain GPP estimates with APAR values, which come either from the net radiation sensor above the canopy or from the shaded PAR sensors, which are up to 70% different (not specified in the manuscript). It should be at least surprising that the estimated GPP does not show any saturation. Instead of the quantum efficiency, also APAR could be wrong. The analysis via an apparent quantum yield neglects. for example, also sun and shaded leaves. An apparent quantum yield could be lower than the quantum efficiency because of a wrong partitioning of sun and shaded leaves, a decrease of nitrogen within the canopy that is non-exponential, wrong leaf temperatures, etc. Nothing like this is discussed in the manuscript.

For me the interesting part starts at Figure 7. I think that one can learn most about leaf wetness and model temperatures from the current data set. And it looks like that the single leaf temperature for sunlit and shaded leaves might be the main culprit of the model deficiencies. Wrong leaf temperatures lead also to erroneous canopy evaporation and hence wrong leaf wetness. The single vegetation temperature is not enough discussed in the manuscript. The literature about scaling (e.g. Wang and Leuning 1998, de Pury and Farquhar 1999) is neglected. Soil temperature, G, soil evaporation all depend on the short-wave and the long-wave radiation reaching the ground. The former could be compared to PAR at 10 m, which would give a hint if it is the radiation scheme that needs updating or the calculation of canopy and/or canopy air temperature.

In summary, I would recommend to refocus the manuscript to the temperatures and leaf wetness. If you provide ideas how to improve the model, the manuscript might fit to GMD. At the moment, the manuscript matches rather the scope of Biogeosciences. The latter would also offer the possibility to highlight more the unique observations. They are much more criticizable in the context of a model comparison.

Some specific remarks that I noted during reading the manuscript:

- The introduction reads like a defense why we need model-data comparisons in the wet tropics. This is more than obvious to me.
- I could not access the PhD thesis Song (2019), while I would have been interested to know how he determined LAI.
- I could not find the figure that show that the "predominant winds flow parallel to the valley (e.g., N-S) and not perpendicular to the mountain slope." (line 132f). Why e.g.?
- Line 170ff has already opinions about model formulations in the method section.
- A 100 year spinup? This is much too long for energy and water and not enough for carbon.
- There is often the mentioning of "oversimplification". Is a process that is not implemented in a model an oversimplification?
- Line 362-375 is gibberish. I did not understand the sentences.

- Section 3.5: I think that the formulation "the simulated temperature might be overly sensitive to incoming solar radiation" is unphysical to say the least. Be more specific, more process-related.
- Line 425: "This study demonstrates the possibility of reducing predictive uncertainty by adapting the model to mimic such slope effect ..." The study did not show this. It only demonstrated that one can improve comparison by reducing the quantum efficiency. This is not mimicking a slope effect.
- Line 508ff mentions a good point and this should be elaborated. How could this be improved? Should there be different wetness fractions for sunlit and for shaded leaves? How would you implement this? If light changes. i.e. the fraction of sunlit and shaded leaves change as well, what would you do with the excess (or missing) water that come from purely changing the fractions without any evaporation or percolation yet? What other models would be less "physically simplified"? The Gash model? The Rutter model?

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