

Geosci. Model Dev. Discuss., referee comment RC1
<https://doi.org/10.5194/gmd-2020-436-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.



Comment on gmd-2020-436

Anonymous Referee #1

Referee comment on "CM2Mc-LPJmL v1.0: Biophysical coupling of a process-based dynamic vegetation model with managed land to a general circulation model" by Markus Drüke et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2020-436-RC1>, 2021

Dear authors,

after reading your paper, I had generally good impressions, but I also found the methods section confusing.

The overall structure of the paper is fine. I really like the choice of experiments for the model evaluation, and it provides a clear overview of the strengths and limitations of the model. I do think this model has potential to help advance our understanding of climate-land surface-human activity interactions (high scientific significance), and therefore think that this work has good scientific value.

However, I would recommend a thorough revision/restructuring of the methods section before publishing. I found it quite confusing to work out how exactly the model calculates certain things on a first reading, and a second reading (with pen and paper) still left things unclear. Because of this I couldn't rate the presentation and the reproducibility high. I clicked "major revisions", but the focus is mainly on the explanation of the methods rather than the science. I am of course willing to review the revised manuscript.

It is stated in line 139 that the variables that are exchanged on the "fast" time step are: canopy humidity, soil temperature, canopy temperature, roughness length and albedo. Some first questions that come to mind:

- Does canopy temperature refer to bulk surface temperature?
- Do all the tiles in a gridcell have the same "canopy" temperature (even the bare soil fraction?)
- What is soil temperature and why does the atmosphere need it?
- How does the atmosphere use roughness length to calculate the turbulent fluxes? Does it assume neutral stability conditions?

EVAPOTRANSPIRATION AND CANOPY HUMIDITY CALCULATION

The aim of this calculation is, given q_{flux} and e_a , from the previous time step, calculate updated q_{ca} , which will be used by the atmosphere (along with roughness length) to calculate an updated q_{flux} and e_a , and so on.

Since the atmosphere provides q_{flux} , this means it must be assuming neutral stability conditions or using some parametrized stability functions (e.g. Louis 1979) to avoid the need for the LSM to iteratively calculate q_{flux} . How does this happen?

The algorithm proceeds as follows

1. - Calculation of potential evapotranspiration. According to Eq. 1, you are using a daily value of ET_0 . Why is this? Are you calculating this quantity on a daily or a subdaily basis?
2. - Potential canopy conductance from assimilation using Medlyn's model. Is assimilation daily or subdaily? Assimilation, like stomatal conductance, varies greatly during the diurnal cycle.
3. - Supply/demand approach to calculate TRANSPIRATION, from which an stressed canopy conductance is derived using Penmann-Monteith again. -> OK.

I am guessing all these steps occur at a subdaily timestep but this is not clear from the text, especially because of the daily potential evapotranspiration (Eq. 1).

4. - Calculation of parametrized soil evaporation conductance -> OK
5. - Calculation of parametrized interception conductance -> What is P_r in Eq. (5)? Why are you using equilibrium evapotranspiration (Eq) in this formula, rather than the new Penmann-Monteith based ET_0 ?

Now we get to the updating of Dq_{ca} (I use D for the Delta symbol).

6. - Where does Eq. (9) come from? If there is a reference for this equation, you should mention the principle from which it is derived and give the reference (e.g., q_{ca} is calculated from water conservation as in Author et al. (year).). If you have derived this equation yourself, I would like to see it derived in the paper too, maybe in an appendix if you don't want to put it in the main text. Apart from the derivation, a clear explanation of where or how each of the terms is calculated would be welcome (for example, the evaporation-humidity gradient).

7. - ET in Eq. (9) is the total evapotranspiration, coming from all the gridcell tiles, i.e., using the g_c , g_i and g_e calculated above. How do you arrive to ET from g_c ? I guess you use Penmann-Monteith again, but this is not specified in the text.

SURFACE ENERGY BALANCE

- Line 245: K and L : incoming, outgoing or net? If it is incoming it cannot be net. Same for outgoing. Net means incoming minus outgoing. Also, if one is incoming and the other one outgoing, shouldn't they contribute in opposite ways, and thus have different signs? I suspect $K+L$ is just net radiation. In the reference you give for this equation (Milly and Schmakin 2002) the radiation balance in that paper looks different. Maybe you could simply use R_n in Eq. (10), and mention that net radiation is calculated as in Milly and Schmakin (2002)? Also, how do these two relate to the net radiation, R_g , used in Eq. (1)?

- Line 248: In this implementation, the boundary temperature to the soil layers and the canopy temperature are the same as in LaD (Anderson et al., 2004). -> Is there a comma missing here? Are the boundary temperature between the soil layers and the canopy temperature the same, as in LaD? Or are they the same as in LaD? So I guess what this means is that the soil temperature calculation is now driven by the bulk surface temperature rather than the air temperature. Do tiles in LPJmL have separate soil columns or do they all share the same soil column?

- Again, reading the LaD reference, it is clear that they do calculate fluxes taking into account air stability, while it is not clear how you do this in your model. It is normally the

job of the LSM to calculate the turbulent fluxes. I guess this is now done in some sort of surface layer in the atmosphere model. But it should be clear why you can replace Lad with LPJmL and avoid the stability calculation.

I want to reiterate that I find the scientific quality of the paper good. However, I think all the points above should be addressed in order to make it clearer to the reader how the model works, especially since this is a model description paper. This would substantially improve clarity and reproducibility. Special attention should be given to:

- The air stability question.
- Derivation of Eq. (9).
- The confusing daily/subdaily issue in Eq. (1). If ET0 is subdaily, the equation needs a correction. If it is daily, it needs justification, given that it is used to calculate g_c , which varies diurnally.

Some further suggestions to improve the exposition, apart from addressing the above points:

- Figure 2 is a bit cluttered. Maybe you could add a similar figure besides it where you explain the logic of the evapotranspiration scheme.
- A table of symbols would be very helpful. You could list the symbol, the units, whether it is an input to the LSM or an output to the atmosphere and the time step at which it is calculated/exchanged.

There are also a few minor points:

- Line 435: "Simulated AGB shows overall a good pattern, with largest values in the tropics, decreasing biomass in the subtropics and a local maximum in the temperate and boreal zone (Fig. 7b)." I think this is more easily seen in Fig. 7d than in Fig. 7b.
- The DOI for Shapoff et al 2018a is wrong (it takes you to the second part of the paper instead of the first one) (<https://doi.org/10.5194/gmd-11-1343-2018>)
- The lines in plot 7d can be confusing for a colorblind person. I would suggest changing either the green or the red one, and also making the lines a bit thicker.