Geoscientific Model Development Discussions

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Comment on gmd-2021-154

Anonymous Referee #1

Referee comment on "Testing stomatal models at the stand level in deciduous angiosperm and evergreen gymnosperm forests using CliMA Land (v0.1)" by Yujie Wang et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-154-RC1, 2021

This is a very good paper that describes a new stomatal conductance model and compares it with two well-established models using eddy covariance data at two contrasting sites (coniferous and deciduous forest). The paper is well written, the model is adequately described, and the simulation protocols are appropriate. My comments are mostly intended to clarify the analyses and improve the discussion.

- Figure 1. The drawing shows the leaf layers at the top of the stem (and this is explicitly stated on lines 93, 99-100), but the model is multilayer in its radiative transfer. This means that profiles of light can drive profiles of stomatal conductance. But if each leaf layer is at the same height in the canopy, does each layer experience the same gravitational potential and same hydraulic conductance (i.e., the path length for water flow is the same for all layers)? A statement to this is needed, and what is the implication of this assumption?
- Figure 5 should be discussed in more detail. The three stomatal models have quite different values for maximum stomatal conductance (at low VPD and high soil water potential). This is a fundamental difference among models, which then should affect the estimates of Vcmax25 and Kmax obtained from the inversion.
- Figures 7, 8. Why is NEE used for comparison with observations instead of GPP? I understand that GPP is a derived product whereas NEE is a direct measurement. However, NEE requires ecosystem respiration, which is obtained by fitting Eq. (9) to the NEE measurement. It is not surprising then that all 3 models do a good job at simulating NEE (in contrast to ET)?
- Line 264-269. A more thorough discussion of the fitted values for Vcmax25 is needed. The fitted values vary with stomatal model. BBM and MED have similar values, but OSM has a much lower value. The explanation (that this results from the beta_w=K/Kmax tuning factor) is inadequate. Also, how realistic are the fitted values? The values cannot be compared directly with leaf estimates, but the values (which represent a bulk canopy of leaves) are actually comparable to (or even smaller; OSM) that

representative leaf values of 40-60 umol/m2/s found in leaf trait databases. This suggests almost a one-to-one scaling from the leaf to the canopy. Another point to discuss is that the estimated Vcmax25 is only appropriate for a specific stomatal model, meaning that the land surface model always needs to be recalibrated if the stomatal model is changed. The authors never acknowledge this point.

- Line 281-290. All 3 models have similar Kmax at the coniferous site, but quite different values at the deciduous site. The discussion relates this to differences in the C parameter in the Weibull function. How realistic are the fitted values for B and C in the function for the deciduous site?
- Line 322-331. A more thorough discussion of the prescribed (CLM) values for g1 in BBM and MED and the fitted values is needed. The fitted values in Table 3 (BBM at Niwot and MOFLUX; MED at MOFLUX) are much higher than in CLM and much higher than estimates based on leaf gas exchange. Also, the fitted values for Vcmax25 are lower (and more comparable to the values for OSM) when g1 is fitted. This, again, tells me that fitting a model to eddy covariance data is not a robust means to obtain Vcmax25 (the fitted value depends on the model and what other parameters are used in the fitting).
- Line 332-336. I was expecting a more thorough discussion of parameter estimation for land surface models. Yes, using prescribed PFT-dependent parameters has shortcomings. Yes, the optimization model may perform better than empirical stomatal models. But the authors have not adequately outlined a strategy for parameter estimation. What I see from their results is that one can estimate parameters by fitting a model to flux tower measurements, but a common parameter (Vcmax25) depends on the specific stomatal model and what other parameters are also fitted. I would like to see the authors discuss this further.

Minor

- Line 5-8. I agree with the comments about big-leaf radiative transfer being unable to resolve vertical gradients in the canopy. However, the current study does not address the impact of vertical gradients in the canopy (though the radiative transfer model does provide the necessary vertical profiles).
- Line 50-53: These sentences need to be explained better. Why is it impractical to compare models with eddy covariance data? This has been done many times and is a common way to evaluate land surface models.
- Line 58: Why is it ideal to invert required parameters from flux tower observations? Doing so just fits a model to the observations without testing the theory. In fact, this paper demonstrates that. Different estimates for Vcmax25 are obtained between the BBM, MED, and OSM models. Vcmax25 is no longer a quantity that relates to leaf gas exchange measurements. It is merely a tunable knob.
- Line 67-73. This thought needs to be stated more clearly. What is meant by "simple" and "complex"? Is the point that big-leaf radiative transfer is inadequate and that multilayer models are needed?
- Tables 1 and 2. It is not stated how the data are used. How are chlorophyll, tree density, and basal area used in the model, or are these merely to show that the sites differ in stand structure and physiology?