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Discussions

Interactive comment on "Evaluating the Community Land Model (CLM 4.5) at a Coniferous Forest Site in Northwestern United States Using Flux and Carbon-Isotope Measurements" *by* Henrique F. Duarte et al.

Anonymous Referee #1

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This paper describes an application of the CLM model to the Wind River flux tower site.

The first point to note is that model parameterization is done in the good old-fashioned way. When applied out of the box with standard PFT parameters, the model does not fit very well. Hence, the model needs to be calibrated. This calibration is done by adjusting parameter values manually, based on the literature and some trial-and-error, until the fit to the data is not too bad. Most groups are moving away from this approach to parameterization, to a more rigorous statistical framework such as Bayesian calibration, which yields more defensible parameter values. I don't think it's essential that the

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authors do this, but it would be good if they could give at least some justification for sticking with the traditional method of parameterization.

The second striking thing about this manuscript was the almost complete lack of reference to the literature in the Discussion. The Results and Discussion are combined into the one section – never a good idea in my view. Here, there is almost no discussion of the results, and no attempt to place the results in the context of the literature. Overall, I came away with a strong "so what" feeling: the authors do not do a good job of articulating why they want to calibrate CLM for this site, nor what we get out of it. There is little in the Introduction to motivate the study, and nothing in the conclusions about how this work advances the field in general. I very strongly suggest that the authors - Better motivate the study in the Introduction, with an expectation of the kinds of questions that this work can address - Separate the Results from the Discussion - Focus the Discussion on what we learn from this study, and ensure that it is placed in the broader context of the literature with appropriate citations.

Some comments on the methods:

The drought stress factor should be more clearly defined: I'd like to see the equation for the plant wilting factor, which apparently depends on both soil water potential (state variable) and the plant dependent response to water stress.

I don't understand the use of the factor 'd' in equations 5 & 6. As I understand it, the relationship A = gs/1.6 (Cs – Ci) is a physical description of the diffusion process through the stomata. How can this be modified by nitrogen limitation? Or is this something that affects the "isotopic" Ci/Ca only?

I note that mesophyll conductance also affects the isotopic ratio – is this accounted for in this model?

Please add a description of how the model scales from leaf to canopy. As all of the comparisons are with canopy-scale GPP, LE and Gs, it is important for the reader to know the principal assumptions underlying this scaling. How is leaf isotopic composition modeled for the whole canopy? How is leaf conductance scaled to the canopy?

The model is evaluated against gap-filled flux data. In my view that's not acceptable: evaluating a model against gap-filled data is comparing one model against another. The model should only be evaluated against non-gap-filled data.

Please describe more clearly the process used for calibration. For example, p16 says that SLA0 is optimized by aiming to minimize model errors in site observations of LAI and CI – was this done using a solver function, or simply by manual trial and error?

On the results:

Figure 2 could show observations as well as model output, making it easier to visualize the model-data correspondence. Please indicate in Table 2 what the errors refer to (+/- SE? 95% CI? Range?)

I was unsure how to evaluate the leaf isotopic data. Are the modeled values to be compared with the top, bottom, or average of the canopy? See note above about how isotope discrimination is scaled to the whole canopy.

It would have been good to see the model performance with the parameters out of the box, as well as model performance with calibrated parameters, in order to visualize the effect of altering model parameters.

Please discuss the lack of energy balance closure at this site. The model assumes the energy balance is closed; if the data show a lack of closure the model must show a bias in its predictions of either LE or H. How large is the lack of closure at this site, and how does it affect the model comparison to data?

What is the average rooting depth? The SWC data shown are only to 30 cm – how much deeper than this do the roots penetrate? Is the lack of response to low SWC a function of only considering the very top soil? The demonstration that the model over-estimates the effect of low SWC in the topsoil is interesting, but difficult to interpret without the rooting depth and the formulation for soil moisture stress being given. Nothing is said about how the model might be improved based on this observation – it would be good if the authors could identify the root cause for this mismatch and suggest how it could be addressed.

Finally, a very important point, something that needs to be said: why on earth is CLM still using the Ball-Berry stomatal model? The Ball-Berry model is physiologically incorrect (see Aphalo & Jarvis 1991, and pretty much every stomatal physiology paper since). It is consistently outperformed by models based on VPD. It will incorrectly predict stomatal behaviour in future climates, when VPD is predicted to change but RH stay the same (see Sato et al. 2015 JGR). It is quite odd to read here the justification that "such improvement is expected to be small". I think it is well past time that CLM moved on from the Ball-Berry model.

Very finally, debating whether to mention this or not, but: I was also struck by the extreme gender imbalance of the authorship list. Ten male authors! I hope and trust that the PIs here are actively taking steps to address this imbalance in their group of collaborators.

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