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Geoscientific Model Development Discussions

# *Interactive comment on* "Evaluating the E3SM Land Model at a temperate forest site using flux and soil water measurements" *by* Junyi Liang et al.

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Liang and coauthors present a nice study exploring sensitivities in the E3SM land model to changes in the calculation of soil water potential and subsequently to parametric changes related to plant physiology. They compare simulated results to observations from the MOFLUX site, focusing on carbon fluxes (GPP and soil respiration, SR).

There are a host of changes suggested that generally improve agreement with observed results, but in general it's hard to follow what changes are most important for the improvements. I appreciate the need to keep text and display items simple & di-



gestible for readers, but a bit more complexity would help shed light on the factors responsible for the site-level improvements in the model made here.

For example, it looks like the modified soil water potential scheme (Hanson, I think), provides a better fit to GPP, SR and soil moisture (Figs. 2-5), but it remains unclear if the modifications are significantly better (or different) from the Clapp & Hornberger scheme that's been tuned to local edaphic characteristics? It's not that surprising that the parameterization for a global model would not be a good fit to local results, so does the model just need tuning for site-level runs, or are underlying physics and assumptions in the Hanson scheme fundamentally superior to another approach? Addressing this question matters if the long-term aim of this work is to document changes made to ELM from CLM4.5.

Similarly, how important are the suggested parameter changes for capturing the annual cycle of LAI and C fluxes vs. changes to the soil moisture scheme (Fig. 6). Stepping through these changes sequentially in the text and display items will clarify the source(s) of the improvements.

Finally, although the authors claim that improving SWP directly improved soil respiration estimates, it's not clear if this is a direct effect of soil moisture on soil respiration, or merely reflective of the larger plant and soil C stocks simulated as a result of having higher GPP. Concurrently presenting changes to ecosystem C stocks and the soil moisture effect on GPP and heterotrophic respiration (btran and w_scalar, respectively in CLM4.5) will help clarify how / why improvements were made.

Major concerns

I appreciate the effort used to explore alternative formulations for SWP in the model (Fig. 1, Table 1). Two questions come to mind. First, is it worth doing a more thorough model selection process like AIC or BIC that penalizes more complex models for their additional parameters instead of just showing RMSE. Second, as E3SM is intended to be run in global simulations, I wonder what effect alternative formulations for SWP have on water and energy fluxes from the model in site level, and ultimately global, simulations? The GPP results (Fig. 2) are a good start for this, but presumably these changes really modify ET fluxes (and runoff). It seems documenting these changes are likely important (if only in SI)?

If Hanson or van Genuchten formulations are 'better' fits to the observations, why aren't they used for GPP simulations in Fig. 2? What's the purpose of exploring alternative SWP schemes, if they don't follow through the C cycle simulations in the model? Reading the text on the bottom of page 7, however, maybe (MODswp) is using the Hanson scheme? If so, does the calibrated Clapp & Hornberger approach provide similar improvement by removing the high bias in the default configuration (Fig. 3). Please clarify in the text and figure captions what's being shown and why none of the models adequately capture the effect of the 2012 drought.

Minor concerns

Section 2.3 This is really a broader comment on how author groups working with E3SM intend to articulate the version of the model on which they are working, esp. for readers less familiar with nuances of CLM4.5 development branches and subsequent ELM developments. For example, how is this code different from other publications (e.g. Brunke et al. 2016; Riley 2018)?

Section 2.3. Please justify the decision to use the CLM-CN decomposition module for a paper focused on soil respiration when Bonan and others (2013) clearly demonstrated shortcomings of this model version? It seems like the wrong tool for this job?

Section 2.3. This is also a little confusing, as the opening line of the section states the soil biogeochemistry is vertically resolved, but to my knowledge CLM-CN does not apply vertically resolved soil BGC? Please clarify

Page 4, Line 20. Single point runs (especially with CLM) forced with flux tower measurements have a long history that should be acknowledged here.



Page 7, Line 7. What changes were made to the Clapp and Hornberger parameterization, there are lots of hard coded parameteris in eq. 11-13.

Fig 2. Why are observations shown with a black line and purple bar (inset)? Consistency within and among figures will help readers understand display items more easily? Similarly, using the same color for line of the default model and modified model in Figs. 1 and 2 would be helpful

One strength of using flux tower data in single point simulations seems to be examining the seasonal cycle of carbon and energy fluxes. This is somewhat lost in Fig. 6, and I wonder if the display item would be more powerful if simulations are results were averaged over the whole observation record (e.g. just show 1 year instead of 9, as the interannual variability isn't that obvious (and already shown in Fig. 2)

Fig 4 is never really discussed and doesn't add much to the paper in my estimation. Can it be removed from the text? More, it follows that that changes in productivity would have a linear effect on soil C stocks and therefore respiration rates in a first order model like CLM-cn (Todd-Brown refs from the text), so the relationship shown here isn't really surprising.

Out of curiosity, how do simulated soil (or vegetation) C stocks compare with observed stocks at the site? The focus on fluxes is fine, but given that fluxes are linear related with stocks, do suggested modifications to the model improve estimates of fluxes AND stocks for the site?

Seasonal biases in SR and GPP fluxes look pretty bad with default and 'swp' versions of the model (Fig. 5). The parametric changes in Table 2 seem to address some of these seasonal biases (Fig. 6), but it seems like showing the scatter plots on Fig. 5 (maybe with a 3rd color) would be helpful? Along these lines, should both Figs. 5 and 6 show the same 3 simulation ('default', 'swp', and 'swp_param')? Showing the mean annual cycle (+/- 1 sigma on the observations) for all panels in Fig. 6 would help to make this figure easier to digest.

SLA is something that's measured, maybe not at the site for similar trees to the ones at the site? Is building 3x thicker leaves (Table 2), a reasonable assumption? Similarly, if the authors need to decrease LAI while increasing GPP, flnr necessarily has to increase in the model, but is the 20% increase here supported by databases like TRY, or are these parameter changes just illustrating big nobs in the model that are poorly constrained by observaions?

Page 10, line 12 please report statistics to support claims being made. Visually, the red line looks closer to the observations than the blue one (Fig. 6b,c). How do the annual totals look?

As with comment above, how do changes in annual fluxes or total stocks compare with observations following parametric changes suggested in Table 2?

Page 12, line 4. This doesn't seem like a fair statement or comparison, as results from the tuned Clapp & Hornberger scheme are never presented.

Page 12, line 10, given the dominance of Rh in contributions to soil respiration (Fig 7). I'd suspect that changes in SR have more to do with larger SOM stocks than they do links between substrate supply through GPP, as suggested here, but no data are presented along these lines?

Page 12, line 20. This statement may be true, but it's not clear that changes to VMC proposed here had much of an effect on the Rh component of the model. To show this, it seems like showing the soil moisture effect (w_scalar) on soil decomposition rates from different configurations of the model would be needed. Otherwise, I'd suspect that improvement to SR (Fig 2, 5) are predominantly driven by larger soil C stocks (via higher GPP), but not from direct improvement in the SWC on soil biogeochemistry, as suggested in the Powell paper referenced.

Page 12, line 20 what is SWP-VMC, should be VWC?

Page 13, line 2. Again this claim is poorly supported by the data presented. Yes, the



tuned Clapp and Hornberger model is not the 'best' model in Table 1, but are results for GPP or SR markedly different than the Hansen results shown?

Page 14. The q10 analysis is nice, but I wonder if a more ecological explanation is relevant here- specifically highlighting the role of root exudates in supplying labile C substrates that are important for SR? The land model here doesn't consider these ecologically important C fluxes that likely have an important control over the seasonal dynamics of soil respiration and microbial biomass already discussed?

References: Bonan, et al (2013) Evaluating litter decomposition in earth system models with long-term litterbag experiments: an example using the Community Land Model version 4 (CLM4), Global Change Biology, 19, 957-974.

Brunke et al. 2016. "Implementing and Evaluating Variable Soil Thickness in the Community Land Model, Version 4.5 (CLM4.5)." Journal of Climate 29(9): 3441-3461, doi:10.1175/JCLI-D-15-0307.1.

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