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Interactive comment on "Evaluating the Community Land Model in a pine stand with $^{13}CO_2$ labeling and shading manipulations" *by* J. Mao et al.

Anonymous Referee #3

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General comments

Mao and co-authors present an interesting study evaluating photosynthesis and C allocation parameters in CLM4.0 with data from a 13CO2 labeling experiment in a young Loblolly Pine plantation. Although their efforts to evaluate process representation based on short-term experiments are novel and interesting, I'm surprised there's less introspection on the implications of their findings. It's not surprising that adjusting parameter values produces better results- but as the author's stress CLM is a global model used for climate change projections. As such what are the implications of tuning up model parameters for Loblolly Pine trees? Should the Ball-Berry parameters in CLM be changed for all plant functional types based on these findings? Would the modified parameters fit within observations constraints of data in databases like TRY or Glopnet? Are there larger structural uncertainties or biases in CLM that this study exposes? If so, how can they be corrected? As presented this work illustrates how models can be tuned with data, but misses a potential opportunity to draw broader conclusions or gain much insight.

Like any good study, this paper raises more questions than it provides answers, but the answers provided here are not very compelling. The authors very clearly state their objective and focus at the end of section 2.2. This is a paper that more narrowly focuses on building tools and capabilities in PTCLM to facilitate model-data comparisons from experimental manipulations and site-level observations. This is an important, valuable contribution. The aim here isn't necessarily to evaluate and improve CLM, but to build and document valuable tools that facilitate site-level comparisons. Perhaps one path forward would be to more narrowly cast the paper? As presented the paper seems to communicate that the authors were able to tune some parameters that modestly improved growth and photosynthesis parameterization for a particular tree species (Fig. 3), but neglected the heavy lifting of improving HUGE biases in soil physical & hydrology, plant C allocation, and photosynthesis / stomatal conductance (Figs 4-6). The authors adequately highlight these shortcomings and suggest solutions, although it doesn't really appear they're interested in addressing them in the future. One reading of this paper would conclude that by applying a novel (but incomplete) experimental design allows for fine tuning of parameters for a particular plant species that aren't really that bad in the global parameterization of CLM and highlights some huge biases and structural issues with a model that the authors can't address, or aren't really interested in fixing.

Specific comments

Why shading? What does this kind of manipulation tell you about model response to disturbance or environmental change? The 13CO2 seems novel and valuable- but the value of shading seems less clear, especially w/o a control during the experimental period of focus- unless it's to illustrate biases in low light (nighttime?) stomatal conductance.

It's confusing understand what features of different version of CLM were used in this study. For example, CLM4.5 (Oleson et al. 2013) documents the 13C features and PT-CLM configuration used here? However, the photosynthesis, C allocation, and hydrology came from CLM4.0? It seems ironic that the authors would note the significant efforts to improve the CLM (P 6974 L 1-6), yet used a version of the model that does not reflect those changes (CLM4.0)- in particular changes to canopy photosynthesis (Bonan et al. 2011, 2012) and known issues with soil hydrology in the models (e.g., Swenson & Lawrence 2014). Can this be somewhat clarified for readers not familiar with different versions and configurations of the model?

The experimental design that this study is trying to replicate seems surprising to me. As I understand it there are measurements for pre-treatment, two shading experiments, and post-treatment, but during the shading experiment there's no untreated control goup?! Repeating the experiment is well outside the scope of this manuscript, but a more compelling study would have been to parameterize the model for the pre-treatment period and then see if it can even replicate control conditions during and after the experimental shading. This is especially troubling since the 13CO2 pulse came at the start of the shading experiment, and for which there are no control (i.e. unshaded) data?

P 6980 L 10, I appreciate the honesty about the approach, but why was this approach chosen, instead of one that provides estimates of parameter uncertainty?

I appreciate the work that went into optimizing parameters for a young loblolly pine stand, but it's hard to see and never discussed how far off the standard configuration of the model was from observations (Fig. 3a)? Are there error estimates on the observations? If so, can they be displayed (or are they already)?

I understand the focus of this paper is on biogeochemistry and C fluxes- but how well

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does PTCLM handle water and energy fluxes? How do changes in parameter values influence transpiration rates, latent and sensible heat fluxes? Has anyone looked at data from ‘nearby Walker Branch and Chestnut Ridge eddy covariance sites’, or other flux towers in loblolly pine plantations? At the very least there should be some introspection on how the suggested parameter changes influence other parts of the model, not just C fluxes.

Looking at soil respiration (Fig. 5) seems like a spurious analysis given the data presented. In models like this rates of respiration are largely determined by soil C pool size- but as these data are never presented, it’s hard to assess if the model is producing plausible results (as implied) with realistic initial states? Moreover trying to justify potential experimental differences in soil respiration seems speculative and distracts from the focus of this paper (photosynthesis and plant C allocation).

The authors seem to have begun this study acknowledging the C allocation scheme is and isotopic fractionation in CLM is very simple (section 2.2). Thus, findings that it does poorly against observations hardly seem noteworthy. Even still, I’m surprised the authors don’t go deeper in their discussion of these results (P 6990 L 15-27)

Section 4.3 reads it was taken from the DOE-MODEX website and/or a proposal the authors just submitted. No one is arguing about the value of bringing models and experiments closer together- however, this section is completely void of specific modeling needs that the results here highlight. It seems like the authors learned they need to collect some data more effectively, but it’s not clear how the results from PiTS-1 inform the model development directions (and measurements) that should be prioritized. This seems like an excellent opportunity to reflect on specific knowledge gained from both model and experimental work and how that insight will be applied moving forward.

Technical Corrections

The sentence on P 6980 L 6-7 seems incomplete. A genetic algorithm that does what? [“To reduce this possibility, we used a genetic algorithm (Runarsson and Yao, 2000)”]

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Please provide a bit of background for readers not familiar with this approach.

P 6982 L 12-21. I don't completely follow this discussion. Did the authors change how CLM estimates limitations on carboxylation rates? If so, where is this described? If not, this text seems out of placed and either should be moved to the discussion or removed (with preference for the later).

P 6986 L 23 Can statistics be provided for the statement that parameterized results are better than the standard configuration? This statement seems true, but the standard results don't look that bad (for using a generic evergreen pft parameterization to simulate growth of young loblolly pine stand).

Also in section 4.1 it seems like another important parameter change is the fraction of NPP that builds stems in the optimized parameterization. This also makes sense since loblolly packs on a large amount of wood for a relatively low LAI, making it a valuable timber / plantation tree. This is never discussed.

Figures are small, complicated, and hard to read. In my experience this gets even worse when papers are formatted to journal styles. Can text in figures be made larger, and cluttering information (e.g. formula be put into the caption). Some of the color choices for lines are either nauseating or unreadable (green and cyan), and insets in Fig. 6 are too tiny to be useful. Please take care to generate illustrative figures that help communicate & clarify the story being told here.

Where possible figures should communicate observational uncertainty. It's shown on Fig 4b, but not elsewhere.

In Fig. 6, why do simulated 13C concentrations seem to spike before the labeling experiment actually happened (day 0?). This an error in how the figure is drawn, how the label was applied in the simulations, or a misunderstanding on my part?

Throughout there seem to be formatting errors with the subscripts on 13CO2.

References: Bonan, G. B., Lawrence, P. J., Oleson, K. W., Levis, S., Jung, M.,

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Reichstein, M., Lawrence, D. M., and Swenson, S. C.: Improving canopy processes in the Community Land Model version 4 (CLM4) using global flux fields empirically inferred from FLUXNET data, J. Geophys. Res.-Biogeo., 116, G02014, doi:10.1029/2010JG001593, 2011.

Bonan, G. B., Oleson, K. W., Fisher, R. A., Lasslop, G., and Reichstein, M.: Reconciling leaf physiological traits and canopy flux data: Use of the TRY and FLUXNET databases in the Community Land Model version 4, J. Geophys. Res.-Biogeo., 117, G02026, 25 doi:10.1029/2011JG001913, 2012

Swenson, S.C., and D.M. Lawrence, 2014: Assessing a dry surface layer-based soil resistance parameterization for the Community Land Model using GRACE and FLUXNET-MTE data. Journal of Geophysical Research-Atmospheres, 119, 10299-10312, DOI: 10.1002/2014JD022314.

Interactive comment on Biogeosciences Discuss., 12, 6971, 2015.