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Discussions

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 #1

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In this paper, the authors attempt to reproduce observations of canopy behavior during a shading experiment following introduction of a pulse of high-concentration 13CO2 and subsequent shading treatments. The results are that CLM performs better in pre-treatment conditions when tuned to site-specific values, and CLM is unable to capture the observed track of the 13CO2 pulse through the canopy and roots.

The finding that tuning improves model performance at a site is hardly new or novel. This has been done many times before (e.g. Collello et al., 1998; Prihodko et al., 2008; Rosolem et al., 2012, and many more).

The following of 13C through the system is the more interesting component of the pa-

per, and, in my opinion, a lost opportunity. Instead of describing what the observations mean with respect to the behavior of the natural canopy, the authors simply gave a clinical description of how the model differed from the observations. "We ran a model, here's what happened." Come on. I can think of a conceptual model whereby the 13C pulse is first taken up by the leaves, then takes time to work through the system. You can see some of this in Figure 6, particularly 6(a) and 6(d), but the behavior in the phloem and bulk roots is more subtle and complex. But a description of processes and mechanisms at work in the real canopy are never addressed, and the reason for model departure from observations is glossed over, the authors merely saying that the allocation scheme "needs attention" and a more labile storage pool should be added. Isn't this the time to do it? I would be very interested to see a paper that demonstrates the mismatch between modeled and observed 13C, posits some reasons for the mismatch, addresses them, and runs the model again. That would be a very interesting paper.

No real discussion was given for why we are interested in simulating carbon isotopes through the ecosystem. Is it simply to gain a better understanding of biogeophysical processes? Could we expect to see better simulation of net carbon flux and/or the Bowen ratio with better understanding of 13C? Are there implications for ecosystem response to changing climate?

My initial inclination is to recommend rejection for this paper, but I think there is an opportunity here. Take out, or at least minimize the sections on parameter tuning. The community has already done this. A more detailed focus on what is going on with the isotopes as they move through the real system is needed, as is discussion of model success/failure in reproducing the observations and what it means. Finally, the authors should hypothesize some ways to modify CLM, and implement them. This would result in some actual hypothesis testing, as opposed to a paper that reads "We ran a model: here's what happened." My formal recommendation is acceptance with major revisions.

Specific Comments:

The unit testing was mentioned as being very important, but not described. If the Wang (2014) paper is all the reader needs to know, cite it and move on. If more detailed description is needed, share it with the reader.

Increasing the Ball-Berry slope and intercept parameters to extreme values made little or no difference in the one plot where they were shown (Figure 4c). Obviously, then, this was not the reason for model error. Why not just say that modifying the BB parameters made no difference and move on? Also, after demonstrating that the BB parameters were NOT important, the authors state in the conclusions that they ARE. This is a contradiction.

Figure 6: there is no explanation given for del 13C, the y-axis on all plots. The scale amplitude differs by an order of magnitude between the panels; the reader needs to be told what is going on here. I'm assuming that the standard treatment is used, where the sample 13C/12C ratio is compared to a standard; is it PDB? Not all readers are isotopists, so some description and context would be helpful. The change of the del value from negative to positive might confuse some readers, so more explanation is warranted.

Where did the carbon isotope treatment come from? I'm familiar with Suits et al. (2005) and van der Veld et al. (2014). Does the CLM methodology follow these or something else?

The del of the respiration is extremely dependent on the spinup, and changing del 13C through the industrial era. How was this treated?

In section 4.2 the authors say "...modeled soil CO2 efflux was too high on the first day of labeling and too small afterwards." Actually, Figure 5b shows this to be false. In actuality, the del 13C was too high on the first day, and too small afterwards (Figure 6d).

References:

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