**Response Letter**

Dear Editor and reviewers,

Thank you for your precious and constructive comments and changes for improving our manuscript. As you can see from changes in the “Comparison\_of\_the\_MainText.docx”, “Comparison\_of\_the\_Figures.docx”, and the responses highlighted in blue below, we have carefully revised the main text (CLM\_PiTs\_v9\_09102015\_Biogeosciences.doxc) and the figures (CLM\_PiTS\_v9\_figs\_09102015\_Biogeosciences.docx). We do appreciate your wonderful ideas and insights, which made us rethink the whole picture and improve the quality of our paper.

Reviewer #1 (Comments to Author):

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 pretreatment 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).

Response: As emphasized in those references the reviewer listed, we agree that improving the model parameters is one of the most important and effective ways for model development. But as you can see, our effort on optimizing CLM parameters for the pre-treatment period is not the only story for this study. We found that even with the updated model parameters there were still some differences between the simulated and observed variables especially during the treatment period. So, by isolating the impact of model parameters, we addressed that those errors should be mainly attributed to structural model uncertainties, which can illuminate further model developments. We have modified the text to better explain the purpose of parameter optimization.

The following of 13C through the system is the more interesting component of the paper, 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 mis- match, addresses them, and runs the model again. That would be a very interesting paper.

Response: This is a nice point. We wish we could do all the model evaluation, development and calibration in one paper. However, we think this manuscript adds value to the community by introducing the capability to perform a detailed model-experiment comparison using a global land-surface model. This study adds new capability to CLM in particular: (1) the explicit use of the 13CO2 observational information by reading new stream file rather than assuming fraction of CO2 as in the default CLM version, (2) improvement of the site-level simulation structure and leverage of the capability for single-point implementation of the Point Version of CLM (PTCLM), (3) calibration of the selected model parameters for the pretreatment period with parameterization optimization techniques, and (4) accomplishment of the functional test capability for the photosynthesis subroutines and multi-scale evaluation using both leaf and canopy scale data. But as you can see from the title and content of this paper, we have mainly focused on the evaluations (not just the carbon isotopes but other important carbon and hydrological variables) of the CLM model with the unique PiTS manipulations for the first phase of our CLM-PiTS work. To achieve this purpose, we actually have conducted a lot of novel work. We think those efforts should be unique at least for the land-surface modeling community, and worth reporting as a separate paper. We have improved the discussion section to place our results more in the context of relevant canopy processes. A successful LSM model development effort will require that we understand these processes across broad temporal and spatial scales. A forthcoming manuscript will use not only the short-term datasets of this PiTS 1 experiment but the subsequent PiTS 3 experiment. Both experiments (and other published work) will be used to develop and test a new C partitioning regime in CLM – one that allows short term C storage pools that can reflect abrupt shifts in GPP, seasonal changes in ratios of C allocation to different pools, and inclusion of additional C sinks such as fruit or fungal symbionts.

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?

Response: Our interest in accurately simulating carbon isotopes through the ecosystem is not simply, or even primarily, to gain better understanding of biogeochemical processes. Rather we are interested in using the isotope tracer to identify areas of improvement specifically in the model representation of carbon allocation, and thus to improve the representation of C storage and CO2 exchange with the atmosphere, with the implications for simulation of climate-carbon feedbacks and future climate when these improvements are incorporated into ESMs. We have added text in the Introduction outlining this interest, the why, of our interest in simulating carbon isotopes in the ecosystem.

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.

Response: As suggested, we reduced the parameter optimization related part and placed it in the context of the ultimate goal of evaluating the model structure. We also added some sentences on the importance of isotope study, and presented potential model developments that could reduce the uncertainties and biases of the CLM in isotope simulations.

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.

Response: Quite as suggested by the reviewer, we have had the section 2.2.3 for this purpose.

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.

Response: The mp and bp parameters were proved to be very important for the pretreatment optimization against the observed transpiration but not for the treatment period. So, the lines 498-523 of section 4.2 were designed to discuss the possible reasons behind the failure of the mp and bp optimization during the treatment period. As suggested, we made slight changes on this expression in the Conclusions part.

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.

Response: We added a brief description of the calculation of del 13C in the caption of Figure 6.

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?

Response: Please refer to the section 25 of CLM 4.5 tech note for detailed description.

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

Response: The Suess effect is modeled using changing global atmospheric 13C and 12C from 1850-present as input. Model spinup was performed using pre-industrial 13C values and default model parameters for soil carbon turnover. The text has been modified to clarify the simulation procedure.

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).

Response: Changed as suggested.