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Interactive comment on "Environmental drivers of drought deciduous phenology in the Community Land Model" *by* K. M. Dahlin et al.

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Many thanks to the reviewers for your thorough and helpful responses. We have addressed the comments within the manuscript and please find our responses to your comments and suggestions below, identified by paragraphs that start with a ##.

Anonymous Referee #1 First, I am curious why the authors chose to use the LAI3g dataset rather than the MODIS LAI product (MCD15A2). While these datasets have similar agreement with respect to in situ LAI observations (RMSE=âĹij0.7; Zhu et al. 2013 RemSen), both validation exercises used a limited number of sites located in drought deciduous regions (e.g., Figure 2 in Zhu et al.) and MODIS data are known to have higher fidelity than AVHRR data (Huete et al. 2002 RemSenEnv). Moreover, while the LAI3g dataset is twice as long as MCD15A2, it is not clear how additional

years of data actually benefit the model assessments made in this analysis. It would be helpful if the authors provided explanation for why they chose to use LAI3g rather than MCD15A2. If this is not possible, then I recommend that the authors redo the analysis using MCD15A2 to ensure that the highest quality data are used.

We agree that the MCD15A2 data set is more robust, but we felt that the length of the LAI3g data set was important, as semi-arid ecosystems have inconsistent rainfall and green-up patterns, making more data useful to cover a broader range of weather patterns. In addition, since we were focused on broad issues in CLM, not pinning the LAI values exactly to the satellite data, we expect that switching data sets would not significantly change the overall results, given that the discrepancies in the original model were so profound.

Second, while it is certainly necessary to assess model performance over the entire annual cycle, I believe it is equally important to consider how well models capture timing of seasonal metrics such as start and end of season since they largely control annual carbon uptake (e.g., Ma et al. 2007 AgForMet). For example, in Figure 2a it is apparent that the LAI3g growing season across NH C3 grasses is shifted early by an entire month. Despite the relatively high R2 and low RMSE across this region as shown in Table 2, this result suggests that the model does not incorporate the mechanistic controls for triggering leaf onset or autumn senescence. Therefore, for regions with stronger seasonality in LAI (mainly temperate grasslands), I suggest the authors perform a quantitative assessment of model bias in start and end of growing season dates.

We agree that this would be an additional interesting assessment, however, we do not think it would change the overall results of this paper, and algorithms like those designed to assess start and end of growing season, growing season length, or amplitude do not perform well when trying to assess areas with more than one growing season per year, which are a common part of savanna-type ecosystems. We have added comments regarding this in the methods section of the manuscript (see response to

reviewer 2, below). Further, our study is focused on semi-arid systems, and not on cold-deciduous phenology, which has been studied more extensively in the past (e.g. Levis et al. 2012).

Third, perhaps this is outside of the context of this study but if the authors have access to in situ observations of LAI data from a semi-arid/drought deciduous region it would be interesting and worthwhile to validate the authors' novel cumulative rainfall model using these data. This is briefly mentioned on Page 5821, Lines 23-24.

We agree! There is lots of room for more assessment of drought deciduous phenology, however, there are only a small number of in situ data sets (e.g. Phenocams) in drought deciduous areas, as far as we know. Adding more data sources (both on the ground and space-borne) is a focus of ongoing proposals, etc.

Finally, while the authors describe and perform parameter sensitivity analyses in the methods and results sections (research question #2), there does not appear to be any significant discussion or formal conclusions made regarding these results later in the manuscript. Moreover, in Figures 4 and 5 the relationship between each model run and the magnitude of the varied parameter is unclear. Overall, I believe this is an important aspect of the analysis and, therefore, suggest that the authors make appropriate changes to resolve these issues.

Latin hypercube types of analysis are difficult to illustrate, however, we agree that the discussion of the sensitivity analysis could be more detailed, and appropriate changes have been made to the manuscript explaining the results of the sensitivity analyses and what is shown in Figures 4 and 5. We've added the following paragraph to explain this challenge in the methods, as well as described this issue elsewhere in the manuscript:

To assess the performance of the different models in the Latin hypercube test we originally plotted the coefficients of determination between the different models' LAI values and the LAI3g data at those points. However, this result did not illustrate any clear optimum in model performance either for the parameters of the existing model,

nor for the rainfall threshold. We illustrate this using the time-series data in Figure 4, which highlight the unusual behavior of the model, and to assess whether the extra green-up period during the dry season had been eliminated in any of the parametric permutations. We ascribe the lack of a clear parametric signal to two effects. First, the LAI3g data were necessarily aggregated to monthly values, meaning that the primarily sub-monthly variation between ensemble members was masked. Second, the timing of the secondary leaf-on period in the dry season was the emergent property of the oscillatory (and thus somewhat chaotic) dynamics of the soil-vegetation feedback on soil moisture.

Regarding the rainfall threshold, we've added the following statement in the results:

While this new rainfall threshold improved model performance both at our points and globally (see below), we note that, except in a few exceptionally dry areas, the model did not appear to be particularly sensitive to the rainfall threshold, as long as some rain did fall, but this threshold, and the drought deciduous algorithm as a whole, deserves more research into seasonal drivers.

Minor Comments and Suggestions:

Page 5804, Line 23: Not sure that quotes are necessary here. ## Removed

Page 5807, Line 7: Please explain what BGC stands for.

BGC stands for Biogeochemistry and designates a particular version of CLM that includes an active carbon and nitrogen cycle, but is different from the CLM-CN version (also with active C and N) which was used primarily in earlier versions of CLM. Since CLM4.5BGC is now the default version, we have included an explanation in the beginning of the paper but changed further mentions of CLM4.5BGC to CLM for simplicity.

Page 5808, Equation 1: offset → onset

Good catch! Looks like this error was introduced during typesetting – per reviewer 2's comment we've changed all of these to psi-threshold

Page 5810, Line 6: Please explain what CRU-NCEP stands for.

CRU-NCEP = Climate Research Unit (University of East Anglia) – National Centers for Environmental Prediction (NOAA), we did not define this following other papers published in Biogeosciences, but will defer to the Editor's preference.

Page 5810, Line 19 (and instances afterward): gridcell → grid cell

Changed

Page 5816, Section 3.3: The CLM naming convention (e.g., CLM4.5BGC, CLM, CLM-MOD) gets a little confusing here. Perhaps it makes sense to only use CLM and CLM-MOD?

See comment above – we've changed CLM4.5BGC to just CLM

Page 5822, Line 12: phonological → phenological

Changed. Whoops!

Figure 2: Need to show letters in each panel.

Letters are in the bottom right corners of each panel – e.g. "A. NH C3 Grasses"

Figure 3: This figure is somewhat busy, although I am not sure what makes the most sense to remove. Perhaps it is okay but I suggest the authors consider alternative representations, if possible.

We agree that these figures (and Figure 10) are challenging, however, we opted for this presentation as it shows all of the relevant details, and we felt that stacking all of the data allowed for easier comparisons than plotting things in different panels.

Figure 4: If the authors choose not to explain differences between each model run, it may also make sense to just plot a mean curve with +/- 1 standard deviation or some other indicator of variance.

Related to the comment above, we've added a more clear description of the Latin

hypercube analysis and discussion of these figures to better explain how to interpret them.

Figures 8/9: Perhaps it would make sense to mask out grid cells with less than some fraction of drought deciduous land cover so that it is easier to depict spatial patterns in the improvement of model performance.

We've opted to leave all of the data clearly represented (instead of masked) with the hope that these figures will be useful to researchers interested in study regions beyond those we have focused on.

Interactive comment on Biogeosciences Discuss., 12, 5803, 2015.