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

Anonymous Referee #1

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Summary:

In this study, Dahlin et al. globally assess predictions of drought deciduous phenology using the Community Land Model (CLM). Specifically, they use the existing CLM representation of drought deciduous phenology to predict time series of leaf area index (LAI) and compare these results with the LAI3g product derived from Advanced Very High Resolution Radiometer (AVHRR) data. Additionally, they develop and test a new prognostic scheme that includes a cumulative precipitation threshold. Results demonstrate that the existing CLM model poorly predicts LAI phenology and spuriously triggers leaf onset during dry conditions across grassland and deciduous savannah plant functional types. Meanwhile, the modified scheme improves overall model performance and resolves the dry season leaf flush, especially across tropical regions.

Major Comments and Suggestions:

In general, I found this manuscript to be very well written and relatively easy to follow. The study brings much needed attention to a largely ignored facet of phenology modeling and the authors thoroughly investigate the performance of existing and modified prediction schemes at point (individual grid cells) and global scales. Moreover, the authors provide a lengthy and useful discussion interpreting results and addressing future modeling needs.

Despite these strong characteristics, I have a few major concerns regarding the authors' choice of materials and methods used in the analysis:

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=$\sim$0.7; 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.

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 correct

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.

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.

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.

Minor Comments and Suggestions:

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

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

Page 5808, Equation 1: offset → onset

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

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

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?

Page 5822, Line 12: phonological → phenological

Figure 2: Need to show letters in each panel.

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.

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.

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.

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