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Interactive comment on "The effect of using the plant functional type paradigm on a data-constrained global phenology model" *by* S. Caldararu et al.

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General Comments:

The authors provide an interesting exploratory study regarding degrees of parameterization applied to a global phenology model and the resulting difference between models at multiple levels of parameterization and observations from a satellite record. It is an intriguing study as it attempts to cover the full range of possibilities when it comes to parameterizing a model; on one extreme, a single set of parameters applied globally, and on the other extreme a set of parameters for every pixel. The results presented may be useful for future modeling efforts, such as the concept of varying a limited number of parameters locally while maintaining the other broad PFT parame-

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ters. However, some unclear methods, the lack of attention to detail in both how the results are presented and the discrepancies between the figures and text, and the lack of constraining parameters within known biological limits need to be addressed; all of which limit the applicability of the results. One final problem pertains to how the general PFT model is applied and the interpretation of its results. Details on each provided below.

Questions regarding methods

How was the MODIS data aggregated from a 1km resolution to a 2deg x 2.5deg resolution? Mean? Median? And why? How was the 8-day MODIS data treated in terms of a model run at a daily time step? (Im assuming the model time step is daily, although this is not explicitly stated). Was the MODIS data interpolated from 8-day to daily values? Were comparisons of model output to MODIS LAI done at an 8-day or daily time step? How was the soil moisture data regridded to match the GEOS-4 resolution? Also, why did the authors choose such a coarse resolution when the primary datasets that describe the vegetation (LAI and the PFT map) are provided at a much finer resolution? There are PAR datasets at finer resolutions available (e.g. CERES 1deg x 1deg). I understand when running global scale models computational limits may be restrictive, but the reasoning for using such a coarse scale should be more specifically described. In particular this aggregation produces some curious 'observed' LAI values; for example it is a bit odd that the forest PFTs shown in Figure 3 have observed values of <1.0. The PFT classification based on dominance should be addressed more thoroughly beyond the quick analysis provided in Figure 6 (which needs clarification as well – see below).

The introduction says 3 main model parameterizations are applied, but appears to list 5 as it includes 'global and 'regional'. The Model Set-up section says 5 are implemented, this should be consistent to avoid confusion.

In the model performance metrics section, there is no mention of regions or pixels that do not conform to a 'regular' seasonal signal. Such as arid systems where multiple

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seasonal peaks may be present in response to precipitation events, crop systems with two planting/harvest cycles per year, or tropical systems where there may be minimal seasonal variation. The authors should address whether these non-standard seasonal cycles were present, and if so, how they were addressed.

Attention to detail and discrepancies in results

The maps of results (Figures 1 and 2) are key components of the manuscript, proving a global look at the results of a model applied globally. However, there is a spatial shift between the pixels and the geographic borders. There are pixels clearly over oceans. Either this is a basic problem due to an unresolved projection difference between layers, or some of the input layers have not been properly georeferenced calling into question the overall results. Second, why do large areas and certain pixels have no results in some maps; N Spain, NE Europe and W Russia, N America and Canada border region, SE U.S., S Africa, C America, N South America, Sweden, Norway? There is no mention of masking or screening pixels in the methods.

The legends of Figures 1 and 2 also need work. For the Figure 1 legend, the upper limit (>0.8) should either be placed where the current 0.8 text is, or be changed to >1.0; it is redundant in its current form. The legend in Figure 2 does not make sense. It currently implies that all gray pixels had no difference in mean or amplitude between predicted and observed, obviously not true. Also, the upper limit in each legend shows 0,7 instead of 0.7.

The results shown in figure 1 do not match results provided in the text. Tropical forests are said to have RMSE errors of 0.15 (local), 0.22 (PFT) and 0.16 (Combined). These areas would appear as primarily yellow or light orange on the maps, but for PFT results, nearly all tropical forests fit into the >0.8 category. The local and combined maps show values in the range of 0.4 to >0.8. In figure 5 and figure 7, Boreal Evergreen Forest is denoted as BEF in the figure and TEF in the text.

For figure 6, the authors do not say which model these results are from; this is not made clear until section 5.3 in the Discussion. The y-axes are labeled ‘Relative’, does this equate to the normalization used in other figures and results? And if so, there is a significant portion of pixels with LAI mean and amplitude biases greater than 0.7 (the maximum value used in figure 2). Why not display this larger range in figure 2?

Lack of consideration of realistic parameter values

In the local and combined models the parameters (specifically the age_crit in combined model) are not constrained to realistic ranges. This calls into question the applicability of these models. For example, the age-crit parameter approaches nearly two years in some temperate deciduous forests, and can be as short as a few months in boreal evergreen forests, this is not realistic. I understand that one goal of this manuscript was exploratory, to allow parameters to range to achieve the best fit. But when the parameterization is allowed to vary regardless of known biological limits, the resulting model loses it applicability to represent realistic conditions which is the ultimate goal of applying such models to predict future conditions. This lack of realistic representation is also apparent in the aggregation problem mentioned earlier, where Forest PFTs have observed mean LAI values less than 1.0.

PFT model application and results

A main goal of the manuscript was to demonstrate how more specific parameterization of a phenology model would improve upon the widely applied method of general PFT parameterizations. In order to make such a comparison and demonstrate model improvement, the widely applied method (general PFT) must be run in its true format; i.e. representation of multiple PFTs within a single grid cell. To their credit, the authors clearly make this point in Section 5.3, and stipulate that this may be main source of error in the PFT model. A main concern is that the PFT model shown here is not representative of the method used in the majority of global models, yet the results of this application are treated as though they are representative of this model in other applications. For example (P. 16850 L. 8-10), the authors state it is important to formally evaluate the PFT model in comparison to alternative approaches. Yes, certainly true, but that is not what is being done in this manuscript. Also, the authors claim in the conclusion that a model with PFT wide parameters cannot explain the observed spatial variation. . .and a response would be of course it can't explain the variation when the PFTs are aggregated to a single dominant PFT across a 2.0x2.5 degree extent. For instance the authors state in the Abstract and P. 16849 L. 13-14 that the PFT approach makes an assumption that all plants within a PFT show identical behavior. True to a degree, but in its application here this assumption is taken a step further in that the mix of PFTs (plants) in a grid cell are being forced to show behavior identical to a completely separate PFT, e.g. where a grid cell may cover both forested and shrubland systems.

Conclusion

Although the manuscript presents an interesting modeling exercise, improvements are necessary. First, some methods descriptions need to be more specific including some discussion of pixels which may not follow 'regular' seasonal cycles. Second, the results need to be clarified; with attention paid to the figures, clarification in why some areas show no results and discrepancies between text and figures sorted out. Third, in order for the results to be applicable to the current state of model development and application some form of constraint should be applied to parameters based on biological limits; presenting a model that provides a better fit without this consideration still does not allow for its application. Finally, in order to demonstrate improvement if phenology representation, the results should be compared to a PFT scale model as it is truly applied. This could be done by using existing model runs and results from other sources so that the authors do not face computational constraints.

Interactive comment on Biogeosciences Discuss., 12, 16847, 2015.