

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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We would like to thank the reviewer for their thorough comments and we hope that our response and modifications to the manuscript will clarify the methods and the analysis.

How was the MODIS data aggregated from a 1km resolution to a 2deg x 2.5deg resolution? Mean? Median? And why?

The MODIS LAI data was aggregated using mean values within each model grid cell. This was done partially because of computational constraints (see below) and partially because after the quality filtering procedure data for a 1 km pixel has a large number

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of missing values and would be unsuitable for fitting a phenology model, which relies on timeseries information. This has now been clarified in the text.

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?

The model was run at a daily time step but only fitted at those dates when data was available, i.e. every 8 days, a method which did not require interpolation of the MODIS data. We have now added this information to the text.

How was the soil moisture data regridded to match the GEOS-4 resolution?

This was done through nearest neighbour interpolation and this information has now been added to the soil moisture data section.

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.

Our main limitation was indeed computational effort. The nature of the fitting algorithm and that we were running multiple fitting setups required a coarse spatial resolution. Trial runs with a higher spatial resolution (0.5° latitude x 0.66° longitude) showed that the problem was intractable. We have now added a justification for the resolution in the

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text, as advised.

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 PFT which show an abnormally low LAI is the evergreen boreal forest, which has some problems due to heterogeneity even at the native resolution, as discussed in Caldararu et al. (2014)

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.

We thank the reviewer for pointing out that this is not clear in the text and we have now clarified this. There are three main parametrisations which we show figures for and discuss in detail - local, PFT and combined - as well as two other parametrisations, global and regional, which we only show overall fit for.

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 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.

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The model has the capability of representing these ‘non-regular’ seasonal cycles, as, unlike traditional phenology models, it does not rely on start and end of season thresholds and can even represent the continuous leaf growth and loss necessary in evergreen tropical forests. With regards to model performance metrics, the only problem might arise from comparing timing prediction and to this end we also use time of maximum. Pixels classified as crops have not been used in the analysis and we thank the reviewer for pointing out that we have omitted to explain this.

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.

This problem is caused by overlaying large grid cells on a map. Missing cells are either non-vegetated (e.g. the Sahara desert) or cells that have been classed as majority water.

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.

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For the legend in figure 1, each number refers to the upper limit of the colour to the left of it and the '>0.8' label is necessary to designate the value of the darkest red. The legend and colour scale in figure 2 have now been rectified.

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.

The discrepancy between figure 1 and the text arose from a mistake on our part where we accidentally plotted absolute RMSE values in figure 1, a mistake which has now been corrected. We have also corrected the captions for figures 5 and 7 to denote boreal evergreen forest by BEF.

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-axis is 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?

The results in Figure 6 are from the PFT model and this has now been clarified in the legend and the results text. The reason the colourbar range in figure 1 and 2 do not show a larger range of values is because the local, PFT and combined model are all

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on the same colour scale and the PFT model has much higher errors.

In the local and combined models the parameters 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 its 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.

As we explain in the discussion, the *age_crit* parameter does not represent effective leaf e.g. in areas that are not limited by leaf ageing, such as the temperate regions, where the parameter is not well constrained. We aimed to have a general global model and let all parameters be fitted in any location of the globe, even where some of the processes, leaf loss through ageing in this case, are not applicable. The model correctly identifies the driving factor for leaf loss however and this does not lead to any errors in model results. We apologise if this was not clear enough in the text and we have expanded this explanation.

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

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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.

We acknowledge that the simplified way in which we use the PFT parameters is one of the major shortcomings of our study and that DGVMs which use the PFT concept allow for multiple PFTs in one grid cell. The fact that we are constraining our model parameters to data makes this a mathematically and computationally difficult problem however. Recent studies have compared the use of plant traits and PFT parameters in non-data constrained conditions and shown that the trait based models perform better (Sakschewski et al., 2015; Verheijen et al., 2013). We have chosen to explore the same question in the context of a data constrained model, which can bring more information to the model but also imposes some limitations on the parametrisation scheme. We have attempted to further discuss and justify our approach in the text.

First, some methods descriptions need to be more specific including some discussion of pixels which may not follow 'regular' seasonal cycles. Second, the

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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.

We hope that we have sufficiently clarified the methods and results and we have better explained the relationship between our age parameter and leaf age. We would like to thank the reviewer once again for their attention to detail in finding the discrepancies in the figures and text.

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.

Unfortunately comparing to different models would raise a number of completely different questions, as our model uses the plant optimality hypothesis, while most other phenology models use a degree day approach. Such a model comparison would be interesting, but would not answer the remaining question about our treatment of PFTs. We have attempted to discuss this problem openly and outline the limitations of our study.

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