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Biogeosciences Discussions

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 comments and the time they took to read our manuscript. We agree that there re many more directions of discussion possible starting from our analysis, but we have chosen to discuss in detail what we termed the combination model, as a proposed middle ground between the existing approaches and our initial, potentially over-fitted and certainly unrealistic local approach. We agree that we have not discussed in detail the feasibility of using such a parametrisation in a

ESM framework and we have now included a more detailed discussion of this issue.

p. 16848 line 7: change 'assumption' to 'simplification' (more on this below)

Now changed

p. 16949 line 13-15: "...underlying assumption that all plants...show an identical be- havior" - this statement, while not incorrect, implies that modelers who use PFTs are ignorant of the limitations of PFTs. Most, if not all, PFT-based studies (including Sitch et al 2003) are careful to point out the limitations of this approach, but also to highlight the reasons such simplifications are necessary.

We did not mean to imply that the authors of studies which use PFTs are not aware of the limitations and we use the word 'assumption' in its scientific sense, in that every theory is based on a number of assumptions which do not make that theory necessarily wrong but only limited in its application. We have now clarified this in the text.

ine 25-26: This cursory explanation of why PFTs are used misses a critical point - for vegetation models that are intended to be used under future climate scenarios (including possible no-analog climates) it is critical to use physiologically based parameterizations. Because so many of the parameters in veg models are unknown globally at fine taxonomic levels, PFTs are used to generalize. Models that include fitted parameters that vary across space won't work in a DGVM context where plant communities may change under future climate scenarios.

We agree with the reviewer that we have not discussed in sufficient details the advantage of using PFT for future scenarios in DGVMs, and we have now added this to the text. However, we must add that efforts are being made to develop alternative methodologies (e.g. Fisher et al., 2015).

p. 16850 line 18: "three main different model parameterizations" - by my count there are at least five parameterizations treated equally (local, PFT, combined, global, regional), plus two more introduced later where you let tropical evergreen forest vary by but not other PFTs, for the combined and PFT options (p 16858 line 20). Which are the actual models used in all the figures, I believe. Please clarify.

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 parameterisations, global and regional, which we only show overall fit for.

p. 16853 line 15-17: any data to back this statement up? or a reference?

This result was included in our previous study (Caldararu et al., 2014), which we have now added a reference for.

p. 16854 line 2: It's confusing to me that leaf level compensation point is in W/m2 but canopy level is in umol/m2/s.

This results from the units that the original PAR data is in as well as the structure of the model.

line 5: "... do not represent measurable values in the field..." I read this as meaning q and phi are fitting parameters.

Absolute values for the parameters ϕ and q cannot be compared to measured values, but relative values and variations across the globe can be considered to have biological

significance. This has now been clarified in the text.

p. 16858 line 20: broadleaf tropical forest performing better. Is there a figure that shows this? This is a fairly significant change to the modeling approach and deserves a bit more discussion, I think.

We have now included a figure in the appendix with detailed results for the regional parametrisation.

line 25-26: So, based on these numbers and the change to using regional tropical forest parameters, Table 2 (references in line 14) includes this change? How did the PFT and combined models perform without this change? Also, doesn't this mean there should be 3 dashed lines in Fig 7 TEF?

All figures and tables present in the paper include regional tropical forest parameters. Figure 7 only shows parameter values for the TEF Amazon region; this has now been clarified in the figure legend and caption.

p. 16859 lines 1-4: again, are there figures to back up these statements?

Yes, all statements in this paragraphs are based on Figure 1.

line 25: I think "Biome" in Fig. 5 should be "PFT"?

Yes, we apologise for this mistake, this has now been corrected.

p. 16860 lines 10-12: the values reported in figures 7 and 8 are a concern, given that they range far beyond what is physiologically reasonable. For example, leaves in temperate deciduous forest rarely last beyond 8 months, yet a-crit for

these plants in your model goes out beyond 2 years.

As we explain in Section 5.1 , the age_{crit} values are only representative of leaf lifespan in the model in regions where leaf loss is driven by leaf aging, so that parameter values in, for example, broadleaf temperate forests are not constrained. We have now clarified this in the text. In the interest of space, we do not show a comparison of the age_{crit} values and effective leaf age, as we have done in our previous study (Caldararu et al., 2014).

I'm also finding the use of 'compensation point' confusing. There are three different compensation points mentioned in this ms - C- direct, C-diffuse, and q (and they have different units!). I'm fairly sure all the figures and the discussion refer to C-direct, but this needs to be clarified, and defined, as I'm not sure what the difference between a direct and diffuse compensation point would be, nor can I find any discussion of this in the literature.

Throughout the discussion, compensation point refers to the direct compensation point, C_{direct} parameter, and this has now been clarified at the start of the discussion. The direct and diffuse compensation point arise from a need to represent the two light components accurately without introducing a full canopy layer model, which would introduce additional complexity into our phenology model. Within a detailed canopy model, the two compensation point parameters would be more realistically represented by a sunlit and shaded compensation points, as it has been shown that leaves grown in different light environments will adapt to their light conditions. We have now included this explanation in the text of the discussion.

line 20: "The discrepancy..." I'm not sure what you mean by this statement.

This sentence refers to the differences in fitted parameter values between the model

parametrisations, as shown in Figure 8.

p. 16861 line 1: The discussion jumps right in to talking exclusively about the combined model, without any overall summary - why choose this model of your 5-7 models described?

We have chosen to focus on the combined model as this is a compromise between PFT level parameters and local traits, providing a much smaller number of parameters than the local model but attempting to overcome the disadvantages of using PFT level parameters. In the discussion we are trying to explore the possibility that this approach can be used more generally or if it is a result of our specific model structure or fitting procedure. We have now added a paragraph at the start of the discussion outlining overall results.

section 5.1: This section highlights the apparent importance of compensation point, but I would like to see some references to realistic values for these parameters, if they exist, or a discussion of why they don't and how this model is still useful if it's using un-measurable parameters.

Light compensation point values are calculated from photosynthetic light response curves at the leaf level or extrapolated to the plant level (e.g. Givnish et al., 2004; Baltzer and Thomas, 2007). We have now included a brief comparison with literature values in the text of the discussion.

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