Biogeosciences Discuss., 12, C7807–C7810, 2015
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Biogeosciences Discussions
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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.

Anonymous Referee #1

Received and published: 19 November 2015

Overall I think this paper is a technically sound exploration of a range of possible phenology parameterizations that could be applied in Earth system models (ESMs). It shows that, unsurprisingly, a model with 14 parameters fitted at every gridcell with disregard to biology performs much better than a series of other models with more strict limitations on the parameter options. This paper could be framed as encompassing the endpoints of a realistic solution to the phenology issue in ESMs - at one end an overfitted model that will not work in future climate scenarios, at the other oversimplified models that do not capture the variability that is likely critical to modeling the land surface, and our goal should be to land somewhere in the middle. Instead, however, it is framed as a pitch for the overfitted approach, with only cursory acknowledgement

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that this approach is unrealistic in an ESM context (more on that in the specific comments below). Confusingly, the 'Discussion' section then focuses exclusively on the 'Combined' model, which, while interesting, also poses some serious problems in that the varying values, compensation point and leaf age 'crit' do not seem to relate to any measurable biological variables.

This paper does a lot of careful analyses and shows some novel approaches to the phenology challenge (i.e. regional and combined approaches) but misses some key reasons why the proposed approaches might not be the panaceas that they are framed to be.

Specific comments:

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

p. 16949 line 13-15: "...underlying assumption that all plants...show an identical behavior" - 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.

line 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.

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

BGD
12, C7807–C7810, 2015

region, 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.

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

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.

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

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.

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?

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

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

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. 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.

BGD

12, C7807–C7810, 2015

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

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?

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.

Overall this section confirms my impression that these (C-direct and age-crit) are fitting parameters that don't seem to have any basis in reality.

p. 16864 line2: "... in our conclusion..." what, exactly, is your conclusion?

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