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Interactive comment on "Phenology as a strategy for carbon optimality: a global model" *by* S. Caldararu et al.

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I have noticed some structural issues in the Introduction. p 15109, l 4 begins with cold deciduous forests and focuses on this subject for an entire paragraph. The following one (p 15109, l 24), however, continues with more general issues in modelling phenology, whereas the other forest types do not follow until much later (p 15110, l 18 and p 15111, l 4). This flow of information could be better and more concisely presented because the problem statement is scattered throughout these lengthy paragraphs. Not until p 15111, l 17, it becomes clear which approach the authors have taken to address the afore-mentioned modelling problems. The authors' approach is only very shortly mentioned on p 15109, l 3 for the first time, but the interruption until p 15111, l 17 makes it a bit hard to figure out what the authors have actually done. I would sug-

gest re-organizing the Introduction to make it easier to follow its storyline (suggested order: introductory sentences, research need/problem statement, other work, authors approach).

In response to the reviewer's suggestion, we have now re-arranged the material in the introduction for clarity.

This manuscript revolves around a Method to optimize carbon gain via the LAI. Therefore, the equations should be ordered in such a way that the reader can follow easily how changes of LAI are calculated. I think Eq. 12 is central for understanding this point and should not be "hidden" in the section 3.4 about "Leaf age effects". The order of sections appears confusing to me. I would present Eq. 12 first in 3.1, explaining that this is the net change in LAI which results from its constituents, leaf gain and leaf loss. I would then explain that leaf gain is calculated by the difference of the current LAI and the target LAI (Eq. 1 + 2), while leaf loss has three reasons and corresponding model components (light, water, and age effects). This way, the methodology and the relationship between the equations would become much clearer for the reader.

We agree with the reviewer that equation 12 is one of the main equations in the paper and this has now been moved to the start of section 3.

Given that the manuscript is titled "Phenology as a strategy of carbon optimality", I would have expected a more thorough discussion of the simplistic way in which leaf production is achieved in this model. The tree actually produces new leafs as it sees fit, depending solely on the actual difference to its target LAI and the incoming radiation. This raises the question, how realistic the tree strategy of "carbon optimality" appears in view of the model lacking an explicit representation of photosynthesis and carbon allocation, and without accounting for essential physiological trade-offs determining e.g. production and maintenance costs of leafs vs. other living tissues. In natural communities, competitive processes add to the complexity of tree strategies which crucially influences their individual optimal C-balance. As the model defines a

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normalized amount of assimilation with only qualitative information value, the authors have wisely abstained from showing maps of primary production or any carbon-related output variable. This is fine because the model is meant to be simple and comprehensible, but it deserves a paragraph in the Discussion to justify the title of the work and evaluate the model's potential for incorporating it in coupled land surface & vegetation models. The Discussion could be further enhanced by suggesting a roadmap how the presented model could be applied to "improve existing representations of phenology in earth system models, thus improving our estimates of the global carbon budget" (p. 15125, l 28-30).

While a roadmap towards the implementation of our phenology scheme within an earth system model is outside the scope and structure of the current paper we believe that such an implementation is certainly an avenue for further research and that coupling our leaf optimality approach with a full carbon allocation scheme would be possible given that a large proportion of the variables and parameters in our model (e.g. carbon assimilation and water limitation parameters) are already explicitly included in vegetation models. As the reviewer points out, our proposed model refers only to the processes in the canopy and, as this is a simple model, we have not included any other carbon pools such as roots that might limit allocation to leaves. However, we believe that our central concept of phenology as a strategy for carbon optimality can be extended to include these limitations as additional factors affecting the target LAI. As the structure of our paper does not allow for an in-depth discussion of this very interesting point raised by the reviewer, we have now included a short paragraph on this in the 'Concluding remarks' section.

p 15110, l 16-18: ": : :and a model that captures landscape rather than species level seasonality would be more appropriate for such models". Unclear sentence structure – how can a model "be more appropriate for such models"? What does "such models" refer to – the "large scale modelling studies" in the preceding clause? Probably this is just a tautology. Please correct.

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We have now clarified this sentence.

p 15110, l 21-23: "Dry tropical forests and shrublands are generally thought [: : :], but leafing is often asynchronous between species and can occur during the dry season [: : :]". I do not understand why "but" is used as a conjunction here (there is no contrast).

We have now clarified this sentence.

p 15113, l 10: "[: : :] we aggregate all forest types into a mixed forest class." Did you mean all boreal forest types?

No, for consistency we have aggregated all forest types.

p 15115, Eq. 2: The definition of LAI(x, t-1) is not consistent with the one in Fig. 1 (Modelling scheme) where this variable is called differently.

We have now corrected the notations in Figure 1.

p 15116, l 16: The light compensation point is rather the point where net assimilation rate is zero than the point "at which there is no photosynthesis".

This concept has now been clarified.

p 15116, l 23: The Equation how to derive Itot should at least be found in the Appendix (although a reference is given, the paper should be understandable on its own). Figure 1 also makes use of the direct and diffuse PAR, while the text does not explain these variables. This inconsistency should be corrected.

We thank the reviewer for pointing out that we are missing an equation. This has now been rectified, as well as the omission about direct and diffuse radiation.

p 15119, l 2: As LAI is dimensionless, the unit m2/m2 can be spared. Is there a reason why this is kept here?

This is a common notation when dealing with LAI measurements.

p 15124, l 29 Figure A4 is described before Figure A1-A3. Figures should be described

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in the correct order.

Figures A1-A3 are first mentioned briefly on line 25 of the same page.

p 15124, l 6: "However, we estimate that in drought-deciduous regions [: : :], plants are generally well adapted to low water conditions [: : :]". How exactly do you "estimate" this in this manuscript? Is this explained here? Or should it rather be, "We assume"?

We infer the plant response to water limitation through the estimated parameter values obtained through model fitting. If plants in water limited regions were highly sensitive to water stress we would expect high values for the water use parameter, which is not the case. We agree with the reviewer that this is not very well explained and we have now clarified this paragraph.

Fig. 1: The caption is a stub. LAIpred is not explained and the name of the variable is inconsistent with Eq. 2.

We thank the reviewer for identifying this discrepancy between text and figure notations, which we have now rectified.

We have corrected the following small mistakes:

p 15109, l 2: process-based

p 15111, l 17: process-based

p 15112, l 17: missing comma after "fails"

p 15113, l 16: "[: : :] require any further information about the type of forest and its phenology type." Insert missing word.

p 15114, l 5: aboveground, belowground

p 15116, l 21: "[: : :] at different depths." Perhaps missing: "in the canopy"?

p 15118, l 22-24: "[: : :] falls below a threshold value Amin, the specific leaf age cohort is lost. We then calculate leaf loss L(x,t,a) for each age cohort LAI (x,t,a) as: [: : :]".

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Change/insert words and insert abbreviation of leaf loss variable.

p 15120, l 29: "[: : :] which the model cannot correct for." Insert missing word and full stop.

p 15121, l 25: progressively

p 15125, l 10: This should be Figure A3, I think.

p 15125, l 19-24: This refers to which Figure(A4?).

Interactive comment on Biogeosciences Discuss., 10, 15107, 2013.