

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

**Anonymous Referee #3**

Received and published: 9 December 2013

General comments:

This manuscript presents a global mechanistic model of leaf phenology based on the hypothesis that trees maximize carbon assimilation by adding and losing leaves and thereby, optimizing their leaf area index (LAI). Leaf gain is governed by light availability and the difference between the current and the optimal LAI, while leaf loss is influenced by light and water limitation, as well as leaf ageing. The model is fitted to time series from satellite imagery and validated with an independent year of the satellite data as well as with ground observations from the Harvard forest site. It reproduces globally observed seasonal patterns of LAI and the length of the growing season well and allows to identify regions where either light, water, or age effects are dominating tree phenology.

C6518

The modelling idea presented here is novel and has great potential to improve the often rather artificially enforced bud burst and abscission in large-scale vegetation models. Using the presented scheme, a continuous spectrum of leaf longevities would be realizable depending on environmental conditions and tree traits. This approach is very much in the spirit of functional ecology, aiming to substitute fixed plant functional types with an eco-physiologically more meaningful framework. The manuscript is generally well written, the Introduction is concise, but would benefit from some reorganization. The methodology is presented in a comprehensible manner, but the Equations could be re-ordered for better clarity. The Figures are mostly clear except of the modelling scheme (Fig. 1) which needs improvement. The discussion is merged with the Results and could be enhanced by elaborating a bit more on possible applications of the presented approach in earth system models to live up to the authors' ambitious aim of reflecting tree growth strategies of carbon optimization with this model (see specific comments below).

Specific comments:

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

C6519

There are also some structural issues in the Methods:

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.

Some issues I have missed in the combined Results & Discussion:

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 leaflets 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 leaflets 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 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

C6520

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).

Technical corrections:

The line numbers were weird on some pages ranging from 1-29 and then restarting at 1-5 and so on (e.g. pages 15122 and 15124). Below I have tried to mark my comments at the correct position.

p 15109, l 2: process-based

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.

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).

p 15111, l 17: process-based

p 15112, l 17: missing comma after “fails”

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

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 15115, Eq. 2: The definition of  $LAI(x, t-1)$  is not consistent with the one in Fig. 1

C6521

(Modelling scheme) where this variable is called differently.

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“.

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

p 15116, l 23: The Equation how to derive  $I_{tot}$  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.

p 15118, l 22-24: “[. . .] falls below a threshold value  $A_{min}$ , 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: “[. . .]”. Change/insert words and insert abbreviation of leaf loss variable.

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

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

p 15121, l 25: progressively

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

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

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

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”? Fig. 1: The caption is a stub. LAI<sub>pred</sub> is not explained and the name of the variable is

C6522

inconsistent with Eq. 2.

---

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

C6523