

Interactive comment on “Probing the past 30 year phenology trend of US deciduous forests” by X. Yue et al.

Anonymous Referee #1

Received and published: 19 May 2015

General comments:

In this contribution, the authors parameterize an ensemble of models of spring and autumn phenophases and use the models (1) to derive continental maps of phenological trends in US deciduous forests over the period of, at most, 1982 to 2012 and (2) to evaluate the role of model components (chilling function, role of photoperiod, GDD/CDD summation) on the predicted trends.

The paper is overall well written though a bit lengthy. I do appreciate the details the authors give on methodological aspects, and the multiple checks (against ground observation and remote sensing data) they present to corroborate their results, but probably some of this material would better be placed in the supplementary material section (see my specific comments). As a formal comment, notice that the “Results” section is more

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a mixture of results and discussion, while the “Conclusion and discussion” section is rather a “conclusion”. I suggest the authors to rename the sections accordingly.

The science is sound, Results are well presented and, considering the spatial extent covered in the paper, new for the US zone (similar results centred on the European zone have already been published, see notably Menzel et al., 2006; Menzel, 2013).

Some aspects of the paper deserve further discussion / modifications:

(1) Phenological models are usually parameterized at the species level (parameters are believed to be species-specific, referring to species proper characters). The authors choose to parameterize the models on phenological metrics derived from LAI time series acquired in mixed deciduous forests. The model validation is undertaken against an ensemble of data, including a spectrum of forest and non-forest (e.g. Lilac) woody species, which (partly) differ from the species for which the models were calibrated.

Questions related to that aspect:

- The model validation is possibly affected by the choice of averaging at a given site all available phenological data (L2-3 p 6044). The local species composition will obviously affect the observed mean. Furthermore, interrupted pheno time series are frequent, so that i suppose the number of species observed at a given location can change over time, and bias the calculation of pheno average dates.

- The main results of the paper (trends derived from the simulations and the relative little influence of the chilling function / photoperiod on determining the date of budburst / senescence) are dependent on the authors choice (fitting ensemble models over all available data, with no attention to the particular response of a species). Yet, there is evidence in the literature that different, co-existing, species exhibit different phenological trends (in response to temperature changes: see e.g. Vitasse et al., 2009; Fu et al., 2014a). I expect the authors to at least discuss that aspect, and at best to test the genericity of their results with respect to particular species, known to be early/late

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flushers/senescent.

- The question of model parameterization at the PFT level (“temperate deciduous broadleaf species”, DBF) should be linked with recent attempts to re-define PFTs. The validity of the PFT concept for vegetation modelling is under discussion (see e.g. Reichstein et al., 2013; van Bogedom et al., 2012). If all DBF species show similar trends (see my point above), then using a PFT-parameterized model is OK when projecting the future / past of forest phenology

(2) Protocol of model validation: ground observation data from phenological networks are by far the most abundant source of data for validation (see Table 4). The reader would like to see a figure comparing dates derived from LAI metrics to autumn dates derived from phenological observations (Fig. 2 does not report such a comparison for autumn).

(3) The use of the “dormancy” term is not adequate to qualify leaf fall (as derived from LAI measurements or Phenocams). Please remind that dormancy is a physiological state of the bud, starting right after budset (i.e. concomitantly to the timing of height growth cessation, i.e. in the middle of summer, when leaves are still present and green) and ending at budburst during next spring (e.g. Delpierre et al., 2015). Hence, please replace occurrences of “dormancy” by “leaf fall” throughout the manuscript.

Specific comments:

L25, p 6039: the “temperature sensitivity to altitudinal trends” is not clearly defined. Altitudinal trends are first and foremost T-related. Please rephrase.

L6-14 p 6041: models calibrated at the species scale cannot pretend to estimate phenological trends for “US deciduous forests”, merely for certain species

L22 p 6041: were data from 1000 ground observation sites used? Not comparable with the 4 ground observation sites used for calibration.

L23 p 6042 – and on: unclear to me where are the four calibration sites located in the

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US. Refer to Figure 1 here.

P 6042 §2: only four out of the 5 calib sites are mentioned (Hubbard Brook missing). Is it because the LAI data were not available at the US-HB site? In that case, against which data were the model calibrated for US-HB?

P 6045: equations 6 and 7: which arguments have driven the choice of the parameters to optimize / to fix?

Equations 2 to 12: report parameters units in the text, or in a dedicated Table.

Equations 8 and 11 share the same variable name (fT) for two independent variables. Use different names.

L17-20 p 6049: conditions of the SA are unclear to me. Please reconsider and rephrase.

L10 p 6050: replace “decreasing” by “increasing” (higher AIC means the model has a worse accuracy-parameterization trade-off).

L15-16 p 6050: which physiological processes do you point here? What are the “synthesis, viscosity, diffusion” processes you mention? Which role do they / are they supposed to play in leaf senescence? If no precision is given, this sentence should be deleted.

L 24 p 6051 and on: I’m not a native English speaker, but I’ve always read calendar date to be reported as DoY XXX rather than XXX DoY

L15-17 P 6053: compare with altitudinal trends in Vitasse et al. 2009

L9 p 6056: the question of the species-specificity of phenological model parameters is well established (see Vitasse et al., 2011 for instance). I would write “has been shown” instead of “is thought”.

L10 p 6056: a key missing reference reporting results for one of the datasets used in

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the paper is Archetti et al. (2013)

L26 P 6058: The sentence beginning with “Missing. . .” should be rewritten, with appropriate places for parentheses and citations. Again, the notion of “synthesis” cited along the Schaber & Badeck (2003) paper, should be precised or deleted.

L 1 p 6059: the potential link between spring and autumn phenophases was demonstrated in the Fu et al. (2014b) paper. This citation should appear along with the Keenan & Richardson (2015) paper.

Section 3.1.2 and Figure 3: predicted dates are systematically biased (occur later than the ground observed dates). Probably caused by the distinct nature of the protocols used for obtaining calibration data vs. validation data. This should be investigated and reported in the paper.

Figure 1: 5 sites are mentioned on the map, only 4 sites appear on the graphs. Clarify. Replace “literature-based phenology model” by “models S9 and A4” in the caption

Figure 4: no autumn data appear on the figure, contrary to what is reported in the caption.

Figures 6 should be moved to the Suppl. Mat. Section.

Table 1: numeric values of the trends should be reported, and discussed in section 3.3

Table 2-3: Four sites appear, when sites are cited in the text (p 6042) for calibration. Clarify.

Table S3: categorize spring and autumn parameters.

Figure S8: “CDD-photoperiod model” has changed name to “literature-based” (compared to e.g. Fig. 9)

SI, section 1 (“Derivation of phonological observations”): no mention is made to the autumn pheno dates. Are the LAI threshold used identical to those for spring?

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Literature cited:

Archetti M, Richardson AD, O'Keefe J, Delpierre N (2013) Predicting climate change impacts on the amount and duration of autumn colors in a New England forest. *PLoS ONE* 8:e57373. doi:10. 1371/journal.pone.0057373

Delpierre, N., Vitasse, Y., Chuine, I., Guillemot, J., Bazot, S., & Rathgeber, C. B. (2015). Temperate and boreal forest tree phenology: from organ-scale processes to terrestrial ecosystem models. *Annals of Forest Science*, 1-21.

Fu, Y. H., Piao, S., Op de Beeck, M., Cong, N., Zhao, H., Zhang, Y., ... & Janssens, I. A. (2014a). Recent spring phenology shifts in western Central Europe based on multiscale observations. *Global Ecology and Biogeography*, 23(11), 1255-1263.

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Menzel A, Sparks TH, Estrella N et al (2006) European phenological response to climate change matches the warming pattern. *Glob Chang Biol* 12:1969–1976. doi:10.1111/j.1365-2486.2006.01193.x

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Reichstein, M., Bahn, M., Mahecha, M. D., Kattge, J., & Baldocchi, D. D. (2014). Linking plant and ecosystem functional biogeography. *Proceedings of the National Academy of Sciences*, 111(38), 13697-13702.

Van Bodegom PM, et al. (2012) Going beyond limitations of plant functional types when predicting global ecosystem-atmosphere fluxes: Exploring the merits of traitsbased approaches. *Glob Ecol Biogeogr* 21(6):625–636.

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Vitasse Y, Porté AJ, Kremer A et al (2009) Responses of canopy duration to temperature changes in four temperate tree species: relative contributions of spring and autumn leaf phenology. *Oecologia* 161:187– 198. doi:10.1007/s00442-009-1363-4

Interactive comment on Biogeosciences Discuss., 12, 6037, 2015.

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