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

Anonymous Referee #1

We thank the referee for the positive, helpful and comprehensive review that has improved the manuscript. Detailed point-by-point responses to the reviewer comments are provided below. The reviewers’ comments are shown in black with our responses are marked as blue. The line numbers below refer to the revised manuscript to be submitted separately.

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 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).

→ We have moved several parts to the supplementary material as suggested. We also renamed the “Results” to “Results and discussion” and changed “Conclusions and discussion” to “Conclusions”.

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 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

→ We agree that data incompleteness and species-aggregation may affect our conclusions. Other reviewers have also raised similar concerns. We considered all the related comments and added the following discussion in the revised paper:

"Our investigation of the roles of chilling and photoperiod is sensitive to the model structure, climate variability, and data availability. First, the similar performance between spring warming and chilling models might also result from the inaccurate representation of chilling / photoperiod mechanisms. For example, the chilling units used in our parameterization are calculated based on daily average temperatures, while Piao et al. (2015) suggested that leaf unfolding dates during 1982-2011 are triggered by daytime more than by nighttime temperature. The up-to-date autumn phenology model fails to capture interannual variability of dormancy onset (Fig. 2), suggesting that unknown processes might involve with the autumn leaf fall (Keenan and Richardson, 2015). It is unclear whether these processes are related to the variations of photoperiod. Second, the decadal changes in temperature may mask the role of chilling. The trend of winter warming is not significant for most areas in U.S. (Fig. S14a), suggesting that chilling requirements have been fulfilled in the past 3 decades. However, it is unclear whether the winter warming will intensify in the future, which may slow the advancement of spring budburst. Third, we choose to calibrate the phenological parameterization at the level of plant function type (PFT) because species-specific measurements are usually incomplete in time and space. Such incompleteness may influence the accuracy of derived decadal phenological records used for both model calibration and validation. In addition, PFT-level parameterization might be too broad for vegetation modeling as it fails to capture intraspecific variations (Van Bodegom et al., 2012; Reichstein et al., 2014). Observations at the community level suggest that budburst of some species is sensitive to fall/winter and spring warming but with opposite signs (Cook et al., 2012). In the next subsection, we examine the records of 13 deciduous tree species at Harvard Forest. Although we found similar intraspecific temperature sensitivity for both spring budburst and autumn dormancy onset for these species, it is unclear whether other species (or trees at other locations) may show divergent responses, as well as how such divergence may affect derived phenological trend at the continental scale." (Lines 589-615)

(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).

→ We have shown more date comparisons between phenological records and LAI-derived ones in the updated Fig. S2. The LAI-derived dates at Harvard Forest are not complete and we mentioned in the SI that: "However, for US-Ha1, date records are comprehensive while LAI measurements are incomplete and as a result we do not use the LAI-derived dates in the model validations at this site (e.g. Fig. 1)". In the main text section 3.4.3, we also explained that: "The 21-year average of the ensemble dormancy onset date is DOY 306, again close to the estimate of DOY 310 based on LAI (Table 3)." These statements and results show that LAI-derived dates are a reasonable proxy for missing phenological records for the model validation.

(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.

→ In the first paragraph of section 2.2, we added the following statement to explain the 'dormancy onset' date: "Most of phenological records are discrete and evaluation of the annual cycle of tree phenology is difficult. Following definitions in earlier literatures (e.g., Zhu et al., 2012), we validate spring budburst dates (or the onset of growing season, the dates D1 in Fig. S1) and dormancy onset dates (or the end of leaf fall period, the dates D2 plus falling length L2 in Fig. S1) predicted by phenology models with the site-level records. The dormancy onset date defined here is based on the canopy level instead of the bud dormancy examined in a recent review paper by Delpierre et al. (2015)." (Lines 172-179)

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.

→ We have removed this sentence because we do not examine the impacts of altitude on phenological responses in this study.

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

→ The models were calibrated with derived phenological dates from LAI measurements, which represent the average phenophase of multiple species. We also validated the selected models with average dates across species, though uncertainties exist due to data incompleteness. “Both the similar temperature sensitivity at site level (Fig. 8) and the predicted phenological trends at continental scale (Fig. S18) support the concept of phenological modeling at the forest and PFT level, and corroborates the further investigation of phenology-climate interactions at the continental and global scale.” (Lines 664-668)

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

→ We clarify that “In total, phenological observations from 1151 sites were used for model validation.” (Line 134)

L23 p 6042 – and on: unclear to me where are the four calibration sites located in the US. Refer to Figure 1 here.

→ In the revised paper, we refer to Fig. 1 even earlier at the first sentence of section 2.1 as follows: “Decadal measurements … to calibrate parameters of the phenology model (Table 2 and Fig. 1)” (Lines 141-143). We also refer to Fig. 1 in one sentence before the original L23 P6042. (Now Line 166).

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?

→ No, the LAI data were not available at the US-HB site. We do not use data from US-HB site to calibrate model parameters. Instead, we use the phenological dates at this site to validate the calibrated models and to check whether these models could perform well at locations beyond the calibration sites.

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

→ The fixed parameters in Equation 6 and 7 were adopted from Chuine et al. (1999). We calibrated other parameters based on RMSE between model prediction and observations. We presented all the calibrated parameters in Table S4.

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

→ Both the values and units of the optimized parameters have been shown in the new Table S4.

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

→ The name of spring phenology has been changed to f_S.

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

→ We further explained the sensitivity analysis as follows: "In this run, we do not include chilling constraint for the spring phenology by using a fixed and calibrated forcing threshold F^* of 50 degree days. As a result, forcing value S_f begins accumulation from winter solstice and budburst occurs if only $S_f > F^*$. The whole process is not dependent on the value of chilling units S_c. Meanwhile, we lift the photoperiod cap for leaf senescence by setting $f_P = 1$, so that the autumn phenology is only determined by temperature ($f_A = f_T$)." (Lines 367-372)

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

→ Yes, we have corrected 'decreasing' to 'increasing' as indicated.

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.

→ Physiological processes are important for plant phenology. For example, "the abundance or concentration of enzymes is determined by the rates of synthesis and breakdown. It is known that temperature and photoperiod play a prominent role in controlling these processes. Temperature, for instance, can act through purely physical mechanisms, as when it influences viscosity and diffusion" (Schaber and Badeck, 2003). However, we understand that physiological impacts are very uncertain and the specific pathway is unclear. We have deleted both this sentence and the one in the discussion section for clarity.

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

→ We have changed all the 'XXX DOY' to 'DOY XXX'.

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

→ We have added the following statement: “The temperature sensitivity of spring budburst date is also within the range from -2.05 to -7.48 days °C^{-1} for different species based on the field experiments performed by Vitasse et al. (2009).” (Lines 499-501)

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”.

→ We have changed “are thought” to “have been shown” as suggested.

L10 p 6056: a key missing reference reporting results for one of the datasets used in the paper is Archetti et al. (2013)

→ Yes. We have cited Archetti et al. (2013) in the revised manuscript.

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.

→ We have corrected the format errors and removed citation of Schaber and Badeck (2003) for clarity.

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.

→ We have cited Fu et al. (2014) PNAS as suggested.

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.

→ We examined this issue in the revised manuscript: “However, the predicted budburst date at these sites is 5 days on average later than the observations. Causes of such bias are unclear but might be related to the uneven spatial distribution of network sites, the distinct nature of the protocols between calibration and validation data, and the incompatibility between model parameters derived at regional scale and that for continental scale.” (Lines 447-451) For autumn phenology, we added: “Similar to the spring budburst, predicted dormancy onset is later by 11 days than the observations.” (Line 456)

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

→ We added the following figure caption to clarify: “Derived phenological dates at US-MOz are used for model calibration but not validation and are not shown here.” We corrected the names of phenology model as suggested.

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

→ We have removed the words “and autumn model A4” to make sure that this figure is for validation of spring dates.

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

→ The original Figure 6 has been moved to SI (now Fig. S14) as suggested.

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

→ Reviewer #3 also proposed the similar comment. This is a good suggestion but might be difficult to implement for two reasons. First, regional phenological changes varied significantly across the U.S., while most of previous studies, if provided numeric values, show only numbers on the continental scale or for the latitude belts. For example, Jeong et al. (2011) reported almost zero changes in start of season over U.S. for 1982-1999. However, at the regional scale, this study showed both positive and negative trends in U.S. Second, the definition of phenological events varies among different studies (White et al., 2009), making it difficult to quantitatively perform the inter-comparison. As a result, we qualitatively compared phenological changes based on spatial maps from those studies, so as to estimate the ensemble spatial distribution of phenological changes in the past decades.

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

→ We clarified as follows: “Data from three out of the four calibration sites (US-Ha1, US-UMB, US-MMS, Table2 and Fig. 1) are also used for validation; however, we use them in different ways.”

Table S3: categorize spring and autumn parameters.

→ We categorized parameters for spring and autumn models as suggested.

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

→ The figure title has been changed to “CDD-photoperiod model” as indicated.

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?

→ Yes. We clarify in the SI as follows: “The same spring LAIt is applied to derive dormancy onset dates, which also show low RMSE against other sources of autumn phenology (Fig. S2b).”

Reference

Chuine, I., Cour, P., and Rousseau, D. D.: Selecting models to predict the timing of flowering of temperate trees: implications for tree phenology modelling, Plant Cell Environ, 22, 1-13, doi:10.1046/J.1365-3040.1999.00395.X, 1999.

Cook, B. I., Wolkovich, E. M., and Parmesan, C.: Divergent responses to spring and winter warming drive community level flowering trends, P Natl Acad Sci USA, 109, 9000-9005, doi:10.1073/Pnas.1118364109, 2012.

Delpierre, N., Vitasse, Y., Chuine, I., Guillemot, J., Bazot, S., Rutishauser, T., and Rathgeber, C. B. K.: Temperate and boreal forest tree phenology: from organ-scale processes to terrestrial ecosystem models, Annals of Forest Science, doi:10.1007/s13595-015-0477-6, 2015.

Jeong, S. J., Ho, C. H., Gim, H. J., and Brown, M. E.: Phenology shifts at start vs. end of growing season in temperate vegetation over the Northern Hemisphere for the period 1982-2008, Global Change Biol, 17, 2385-2399, doi:Doi 10.1111/J.1365-2486.2011.02397.X, 2011.

Keenan, T. F., and Richardson, A. D.: The timing of autumn senescence is affected by the time of spring phenology: implications for predictive models, Global Change Biol, 21, 2634-2641, doi:10.1111/gcb.12890, 2015.

Piao, S. L., Tan, J. G., Chen, A. P., Fu, Y. H., Ciais, P., Liu, Q., Janssens, I. A., Vicca, S., Zeng, Z. Z., Jeong, S. J., Li, Y., Myneni, R. B., Peng, S. S., Shen, M. G., and Pennuelas, J.: Leaf onset in the northern hemisphere triggered by daytime temperature, Nat Commun, 6, 6911, doi:10.1038/Ncomms7911, 2015.

Reichstein, M., Bahn, M., Mahecha, M. D., Kattge, J., and Baldocchi, D. D.: Linking plant and ecosystem functional biogeography, P Natl Acad Sci USA, 111, 13697-13702, doi:10.1073/Pnas.1216065111, 2014.

Schaber, J., and Badeck, F. W.: Physiology-based phenology models for forest tree species in Germany, Int J Biometeorol, 47, 193-201, doi:10.1007/S00484-003-0171-5, 2003.

Van Bodegom, P. M., Douma, J. C., Witte, J. P. M., Ordonez, J. C., Bartholomeus, R. P., and Aerts, R.: Going beyond limitations of plant functional types when predicting global ecosystem-atmosphere fluxes: exploring the merits of traits-based approaches, Global Ecol Biogeogr, 21, 625-636, doi:10.1111/J.1466-8238.2011.00717.X, 2012.

Vitasse, Y., Delzon, S., Dufrene, E., Pontailler, J. Y., Louvet, J. M., Kremer, A., and Michalet, R.: Leaf phenology sensitivity to temperature in European trees: Do within-species populations exhibit similar responses?, Agr Forest Meteorol, 149, 735-744, doi:10.1016/J.Agrformet.2008.10.019, 2009.

White, M. A., de Beurs, K. M., Didan, K., Inouye, D. W., Richardson, A. D., Jensen, O. P., O'Keefe, J., Zhang, G., Nemani, R. R., van Leeuwen, W. J. D., Brown, J. F., de Wit, A., Schaepman, M., Lin, X. M., Dettinger, M., Bailey, A. S., Kimball, J., Schwartz, M. D., Baldocchi, D. D., Lee, J. T., and Lauenroth, W. K.:

Intercomparison, interpretation, and assessment of spring phenology in North America estimated from remote sensing for 1982-2006, Global Change Biol, 15, 2335-2359, doi:10.1111/J.1365-2486.2009.01910.X, 2009.

Zhu, W. Q., Tian, H. Q., Xu, X. F., Pan, Y. Z., Chen, G. S., and Lin, W. P.: Extension of the growing season due to delayed autumn over mid and high latitudes in North America during 1982-2006, Global Ecol Biogeogr, 21, 260-271, doi:10.1111/J.1466-8238.2011.00675.X, 2012.