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

Anonymous Referee #3

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:**

Yue et al studied the changes of both spring and autumn phenology using different spring and autumn phenology models and focus on multiple scales, i.e. in situ and continent, and they found spatial difference in phenology trends over the period 1980-2012. Importantly, they concluded the temperature is the dominant driver of spring/autumn phenology, because phenology models including a chilling requirement or photoperiod limitation does not improve the model performance. The results are interesting, while there are some major comments in the model calibration and evaluation, as well as the explanation of the model results.

The phenology models are normally parametrized for specific species, i.e. difference specie holds different parameters, even at different sites for same specie … the authors calibrated the model using LAI-based dates (species-mixed), and applied these models in the forest sites that probably have different species composition, as well as for the shrub i.e. Lilac. Uncertainty would be raised, therefore, the best model, which was selected for the continent scale prediction (while, actually the US scale), may be not the best, as the parameters might be not accurate. In additions, the models were calibrated using four site dates. How you calibrate these models, i.e. using mixed- dates from all four sites or using the yearly-average-dates across the 4 sites? Need to be clarify. . .

→ The uncertainty of model parameters is one of the main sources of uncertainty in phenological modeling. We include the following text to address this in the revised manuscript: "The validation shows that the predicted spatial pattern is reasonable and the long-term average matches observations within sampling uncertainty (Figs. 3-4). However, due to the data scarcity, all the selected sites are located in temperate areas ranging from 38°-46°N, suggesting that the model should be used cautiously at other latitudes and parameters may require re-calibration." (Lines 695-699)

We clarified how to calibrate models as follows: "the measurements of LAI are not evenly distributed from year to year, and data at some years are too sparse to form the full annual cycle. As a result, we derive the decadal average phenological dates by regressing against all available LAI records at one site." (Lines 153-156) "For each model in Table 5, we apply the exhaustive enumeration method to evaluate all combinations of the discrete parameters. We select the optimized parameters that jointly predict the lowest

RMSE for the long-term budburst dates at the four calibration sites." (Lines 279-282)

For the most important conclusion, i.e. the temperature is the dominant driver of spring and autumn phenology, because the chilling and photoperiod models does not improve the model performance. It's is not completely right. (1) First, for spring phenology, the chilling requirement may be fullfilled, so the similar model performance can be expected, as reported by previous studies that compared different phenology models. So you could not evaluate the chilling effect from the model performance only...(2) Second, the similar model performance between one- and two- phase models may also suggest that the chilling / photoperiod mechanisms may not be accurately represented in these models, such as the chilling units that are counted as the chilling days or is a daily temperature function, while the day and night temperature may be play a different role in phenology (see Piao et al 2015 Leaf onset in the northern hemisphere triggered by daytime temperature). (3) Given the models are reliable, and the temperature might be a dominant driver for phenology in current climate, while with climate warming, the chilling and photoperiod may play an important role, might be dormant in the future. At least, these issues should be discussed in the manuscript. . .

→ We appreciate reviewer's thoughtful comments that reveal possible uncertainties in this study. In combination with comments from other reviewers, we added the following discussion in the revised manuscript:

"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)

One more comment, the authors compared the trends of phenology between the modeling and the RS results, but you could not identify the robust of the model results using the RS data, because the two results are not comparable, i.e. the modeling outputs and the RS data may refer to different phenophase. The statement need to be improved.

→ We agree that the definition of phenological events with RS data is very uncertain. We added the following statement in the revised manuscript: "Since the definition of phenological events varies among different studies (White et al., 2009), we qualitatively compare the simulations with the remote sensing retrievals so as to evaluate the ensemble spatial distribution of phenological changes in the past decades." (Lines 522-525)

**Specific comments:**

P6039 L2 : Körner, not Korner, please check through the manuscript;
Körner and Basler discussed the importance of photoperiod on the spring phenology in their science paper. Better to rephrase it as 'is sensitive to temperature variation', and cite review papers, such as Cleland et al 2007; Polgar,C.A.,Primack,R.B.,2011.

→ We have corrected the author name of the reference to Körner. We have rephrased the sentence and added those review papers as suggested.

P6039 L10, for the phenology changes in Europe, you should also cite Menzel et al 2006 GCB

→ We have added the suggested reference in the revised manuscript.

P6039 L16, please rephrase as 'some species may require cold temperatures', the chilling may not only occur in winter, also in early spring. . .

→ We have removed the word "winter" as suggested.

P6039 L20, for the tree age, you can cite Vitasse et al 2013: Ontogenic changes rather than difference in temperature cause understory trees to leaf out earlier

→ Yes, we have cited the paper as suggested.

P6039 L25, what is the 'temperature sensitivity to altitudinal trends'? Please rephrase..

→ We have removed this sentence because we do not discuss it in the paper.

P6041 L4-8, how you calibrate the models, using average date of all species from the four forests? No clear..

→ We rephrased it as: "We first calibrate each model using derived phenological dates based on the decadal ground observations of leaf area index (LAI) at four deciduous forests." In the discussion section (3.4.3), we showed that LAI-based phenological dates could be a good indicator of species-aggregated phenology.

P6041 L18, define chilling requirements

→ We have defined it in the introduction section: "Some species may also require cold temperatures before budburst (named chilling requirement)"

P6041 L22, ~1000? Why not provide the exact number of sites?

→ We changed it to the accurate site number of 1151 in the revised paper.

P6042 L7, because you determined the phenological dates from the LAI, better use 'Start of growing season' or 'onset of growing season', instead of 'budburst';

→ Our comparison showed that the 'onset of growing season' derived from LAI is very close to the 'budburst' from phenological records (Fig. S2). As a result, we continue to use 'budburst' for the LAI-derived spring phenology. In the revised text, we have the following definitions about spring budburst: "spring budburst date (or the onset of growing season, the dates D1 in Fig. S1)."

P6042 L6-10, D1, L1, D2 and L2, you should point these dates out in a Fig S?

→ We have shown D1, L1, D2, and L2 on the revised Fig. S1a.

P6042 L6-10, you determined the phenological dates for each year, and then calculated the average dates for each site, right? After that, you calibrated the model using only four date, i.e. four average dates from four sites? I'm not sure that, maybe need to rephrase to make it clear;

→ In the revised paper, we clarified as follows: "The measurements of LAI are not evenly distributed from year to year, and data at some years are too sparse to form the full annual cycle. As a result, we derive the decadal average phenological dates by regressing against all available LAI records at one site." (Lines 153-156)

P6042 L14-18, this is results, move to the results section. . .

→ It is a good suggestion. However, as you may read, the result section presents model calibration and validation. The sentences you mentioned here are the introductions of sites and data, and as a result might be suitable for the method section.

P6043 L12-13, to evaluate the model, you used the averaged dates over all trees and species at each site. This means that the difference of average phenological dates among sites not only determined by the climatic variables, but also the composition of species. . . So, the questions is that can the parameters determined at four-sites be applied in other sites that have different species composition?

→ As we respond to the general comments, we tried the best to collect the most complete datasets (phenological records, LAI, and photos) to calibrate parameters and validate model performance at sites and locations outside the calibration sites. “The validation shows that the predicted spatial pattern is reasonable and the long-term average matches observations within sampling uncertainty (Figs. 3-4). However, due to the data scarcity, all the selected sites are located in temperate areas ranging from 38°-46°N, suggesting that the model should be used cautiously at other latitudes and parameters may require re-calibration.” (Lines 695-699) We also discussed the uncertainties due to the differences in species composition in the new paragraph of section 3.4.3.

P6043 L21-25, define the budburst and dormancy when it first occurred. . .

→ We defined them in the revised manuscript: “Following definitions in earlier literatures (e.g., Zhu et al., 2012; Richardson et al., 2013), we validate spring budburst date (or the onset of growing season, the dates D1 in Fig. S1) and dormancy onset date (or the end of leaf fall period, the dates D2 plus falling length L2 in Fig. S1) from phenology models with the site-level records.” (Lines 173-177)

P6043 L22, how you define the rapid change from gray to light green? Using the maximum change rate? Difficult to understand the ‘the middle of the few days when tree colors change rapidaly. . .’. Please rephrase, as well as the definition of Dormancy start …

→ We explained it more clearly in the revised text: “Based on the changes of tree color in these photos, we can easily identify the phase changes in phenology. For example, changes from gray to light green in spring could occur within several days. We select the middle of these few days as the budburst date. Similarly, changes from brown to gray in late autumn may happen within one week and the middle day of the week is selected as the dormancy onset date. An example of autumn dormancy at US-UMB is shown in Fig. S3. The dates derived from photos may have comparable precision as the observations from site-level phenological records (e.g., Fig. S2c), because the latter are also reported weekly or half-weekly.” (Lines 197-205)

P6043 L23, the dormancy start normally is around the date when the bud set, i.e. much earlier than the leaf coloring. . ..better use other terms, such as offset of growing season?

→ The ‘dormancy’ here represents the ending period of leaf fall. It is defined based on the canopy level instead of bud level. In the revised paper, we added following statement: “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 177-179)

P6044 L22-26, similar model performance of 1- and 2- phase models suggest the chilling maybe sufficient over the study period. You can expect the chilling effect as the large scale, but I did not find the correlation between model performance and difference in phenological response among species, please rephrase …

→ We have changed the phrase "for most species" to "at the site level".

P6044 L28, The parameter values of the Sarvas function were determined from the experimental results on Betula pendula Betula pubescens and Populus tremula in Finland (Sarvas, 1972). Whether the 'northern' parameters can be used in the temperate trees? At least, you need mention it in the text, and discuss these issues.

→ In the text, we mentioned this problem as follows: "We apply the same fixed thresholds (e.g., 3.4 and 10.4) for equations (6)-(7) as that in Chuine et al. (1999); however, we re-calibrate other parameters (e.g. $T_c$ and $C^*$) so that these functions adapt to the phenological changes in U.S. deciduous forest." (Lines 268-271)

We discussed this issue as follows: "However, due to the data scarcity, all the selected sites are located in temperate areas ranging from 38°-46°N, suggesting that the model should be used cautiously at other latitudes and parameters may require re-calibration." (Lines 697-699)

P6046 L20, provide reference for the statement: temperature and photoperiod affects the autumn phenology.

→ We have cited Delpierre et al. (2009) and Richardson et al. (2013) in the revised paper.

P6050 L5-13, the figure S3-S9, no RMSE, AIC and correlation coefficient values, better to show these values in a table S.

→ We have added a Table S5 to summarize those numbers.

P6050 L14-15, no model could predict the autumn phenology, i.e. correlation smaller than 0.5, how you conclude the temperature dominant the autumn phenology process?

→ We conclude that temperature dominates autumn phenology because simulations with and without photoperiod limit show similar results. However, as we discussed (refer to the responses to the general comments), this conclusion is model-dependent and is uncertain.

P6051 L18, you studied the country-scale, i.e. USA, no on the continental scale..

→ The scale of USA is large enough to be considered as continental scale. Similar definition could also seen in previous studies. For example, Schwartz and Hanes (2010) who investigated phenology for western U.S.

P6052 L11 the model-observation correlation -> the correlations between modeled and observed budburst dates. . .

→ Changed as suggested.

P6053 3.3 section, you should discuss the difference between the remote sensing based phenology and the model results. . .

→ In section 3.3, we showed differences between RS and simulations: "…while remote sensing studies largely disagree over this area" and "However, the examined studies do not exhibit significant delays in the northern states, in contrast to our results." (Lines 535-536) "The discrepancies between simulation and remote sensing indicate that the model parameters calibrated at site level may not comprehensively represent phenological responses at large scales and for different species." (Lines 537-540)

P6055 3.4.2 section, please see the general comments.

→ We have carefully discussed the uncertainties in our conclusions about the impact of chilling and photoperiod.

P6057, the conclusion and discussion, it's kind of a 'Conclusion'... in the results section, you have a lot discussion, I would suggest to make a separate discussion section. . .

→ The similar comment has been proposed by the Reviewer #1 and we have renamed the section "Results" to "Results and discussion", and the section "Conclusions and discussion" to "Conclusion".

Table 1, the results, better providing the values of changes if you can, such as days per year or decade.

→ Reviewer #1 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, define the 'n' in the legend;

→ Defined it in a table footnote.

Table 3, the phenology dates are the average dates, right? Provide the std . .

→ The regressed (now has changed the name to "LAI-derived" in Table 3) dates are based on one regression against all available records. As a result, there are no year-to-year values and standard deviations are not available. The modeled dates are based on interannually varied meteorology and the standard deviations have been shown in the revised Table 3.

**Reference:**


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