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

Anonymous Referee #3

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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

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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. . .

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. . .

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.

Specific comments:

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12, C2400–C2404, 2015

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.

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

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

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

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

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

P6041 L18, define chilling requirements

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

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';

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

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;

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

P6043 L12-13, to evaluate the model, you used the averaged dates over all trees and

BGD
12, C2400–C2404, 2015

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?

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

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...

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?

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...

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.

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

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

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?

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12, C2400–C2404, 2015

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

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

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

P6055 3.4.2 section, please see the general comments.

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. . .

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

Table 2, define the 'n' in the legend;

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

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