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Discussions

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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 #2

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I have read the discussion paper "Probing the past 30-year phenology trend of U.S. deciduous forests" by Yue et al. with great interest.

In this paper, the authors test a suite of phenology models against site-level data in the US. Models represent spring budburst and autumn dormancy with various degrees of complexity as shown by differences in the required input data and the number of parameters. Models are first calibrated against phenology observations at four deciduous forests. Then, the selected best spring and autumn models are used to produce time series of phenology for the last 30 years over the whole conterminous US. The authors conclude to a temperature-driven advance in the spring budburst in the East and a delay in dormancy in the Northeast and West with large regional variations.

Phenology in ecosystem models is a topical question and this study falls well within the

scope of the journal. The paper is well-written and easy to follow but I have several comments about the modelling procedure that needs to be clarified before publication (see below).

My main comment is that most of the spring phenology models have been derived from the same site, and calibrated against only 4 sites. How reliable are these models once applied to represent forests with varying species composition under very different climatic conditions? An assessment of the uncertainty in at least the models used to produce the regional maps is needed to check whether trends are a robust, or a result of over-fitted models.

Specific comments

P6042 L2: Please define "long-term"? Perhaps the term "decadal" would be more suited to describe the station data.

P6043 L18: Would it be more correct to estimate budburst and dormancy based on significant changes in the LAI time series rather than a threshold?

P6045 L1: See my main comments. Also, why is Jolly et al.'s (2005) phenology model not used for spring?

P6046 L11: More detailed are needed to describe the calibration method. I am concerned that calibrating "by hand", as it appears, rather than using an automated tool does not allow to find the "true" optimum. Generally, using different model structures introduces more uncertainty than the equifinality of parameters of a single model. However, a few words about how well-defined the parameters are is required here.

P6051 Section 3.1.2: Can you compare these results with satellite based estimates from Buitenwerf et al. (2015)?

P6052 L12: $p < 0.2$ is a very generous threshold

Figures Please add coordinates on Figures 3 to 7.

In Figure 3, why do the coloured surface appear to have different shapes in panels a and c, especially in the Southeast? Is it an artefact due to the colour scale that draws missing areas and areas with lower values in white?

More generally, why are the coloured areas in all maps different from the map in Figure 1?

References

Buitenwerf, R., Rose, L. and Higgins, S. I.: Three decades of multi-dimensional change in global leaf phenology, Nat. Clim. Chang., 5(4), 364–368, doi:10.1038/nclimate2533, 2015.

Jolly, W. M., Nemani, R. and Running, S. W.: A generalized, bioclimatic index to predict foliar phenology in response to climate, Glob. Chang. Biol., 11(4), 619–632, doi:10.1111/j.1365-2486.2005.00930.x, 2005.