Geosci. Model Dev. Discuss., 8, C1764–C1767, 2015
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Geoscientific Model Development Discussions
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Interactive comment on "Variability of phenology and fluxes of water and carbon with observed and simulated soil moisture in the Ent Terrestrial Biosphere Model (Ent TBM version 1.0.1.0.0)" *by* Y. Kim et al.

Anonymous Referee #2

Received and published: 24 August 2015

This manuscript describes the phenology module in a land surface model (Ent Terrestrial Biosphere Model, v1.0.1.0.0). The phenology module is Plant Functional Type (PFT) specific, and the authors focused on four PFTs including temperate broadleaf deciduous forest, C3 annual grassland, Mediterranean savanna, and evergreen needleleaf forest. The authors compared the simulated timing of leaf onset and senescence with in-situ observations at four deciduous sites. The simulated and observed carbon and water fluxes are also compared.

As the author stated, this manuscript does not describe a new phenology model.

Rather, the phenology module described here represents a combination of various phenology models designed for specific types of PFT. Yet, this manuscript represents an interesting and critical evaluation of the phenology model. Several critical issues (listed below) need to be address to make it a better contribution to the community.

1) One evaluation of the phenology model is the comparison between observed and simulated carbon fluxes (NEE, GPP, and Re). It should be noted that even when the phenology (i.e., start-of-season, and end-of-season) is correctly simulated, it is still possible that GPP and Re are not well quantified. The discrepancy is caused by other factors, one of which is the photosynthetic parameter Vcmax and Jmax. Description of photosynthesis and respiration is lacking in the current manuscript. Thus I recommend the authors provide a clear description of the following components:

a) Does Vcmax (and Jmax) change temporally? Field observations suggest it does (Wilson et al., Plant Cell and Envi., 2001; Bauerle et al., PNAS, 2012; Dillen et al., AFM, 2012). If Vcmax is fixed throughout the season, then it is likely causing the higher simulated GPP comparing with observation.

b) How is Vcmax determined for each site? Please provide citation to Table C1. The Vcmax of 50 umol m-2 s-1 is a bit low for Harvard Forest (see Dillen et al., AFM, 2012; Keenan et al., GCB, 2012). Yet, since Vcmax is the same vertically throughout the canopy, the overall canopy photosynthesis might be higher. Please consider using a decay function to describe Vcmax (Bonan et al., JGR-B, 2012).

2) A clear description of the data is lacking. I suggest that the authors provide a section solely for this purpose.

For example, LAI observations are used for validation at a couple of sites. How was LAI measured? If LAI was measured manually using LAI-2000, what was the temporal frequency? How were 20%, 50%, and 80% dates of observed LAI determined? Have you used any function to fit the data?

Eddy covariance data were used in the work. Thus it is necessary to briefly describe this dataset, including the temporal frequency and the separation method between the GPP and respiration (citations are needed).

3) Just food for thought on the phenology model for temperate deciduous forests. It has been recognized that for sites like Harvard Forest, the chilling requirements are always fulfilled, and photoperiod plays an important role in controlling the start of heat accumulation. Models with an explicit chilling requirement do not perform better than those without chilling requirements (see Migliavacca et al., Biogeosciences, 2008; Yang et al., JGR-B, 2012; Korner and Basler, Science, 2010). And the first two papers that used Harvard Forest as the study site suggest a temperature+photoperiod model is a better choice. I suggest that the authors consider other forms of phenology models, especially those with photoperiod controls – the dominant deciduous species at Harvard Forest include oak and maple are likely controlled by temperature and photoperiod (Korner and Basler, 2010). Even if a full comparison is not possible, a paragraph in discussion on the controls of spring phenology in temperate deciduous forests is necessary.

4) The fall phenology model for temperate deciduous forests produces results 30-50 days biased from the observation (Fig.3). This is not entirely an unreasonable result, as we know that fall phenology is even harder to model comparing with the spring phenology. However, recent advances in modeling fall phenology do show some promising results (see Delpierre et al., AFM, 2009; Archetti et al., Plos ONE, 2013). I suggest that the authors test these fall phenology models (using optimized parameters from these papers).

Specific comments (P for page, L for line):

P5810 L23: do you mean “a major weakness in DGVMs”? This sentence is not well structured and lacks connection with the next sentence. Please consider revising it.

P5820 L14: “ntropical” to “tropical”

P5822 L21: The correct form of eq (1) should have the sum of max(0,T10-Tbase), not the other way around. In addition, please state the date from which the heat accumulation (and chilling day accumulation) starts. Please be specific about "the beginning of the winter season".

P5823 L16: What control the rate of leaf dropping?

P5831 L14: Please add the unit for the "RMSE of $\sim$0.4"

P5833 L5: Where is Fig.9? Did you mean "Fig. 8"? Please also provide evidence that " a relatively small difference in ET was detected between the simulations with and without the frost-hardening scheme".

P5837 L10: Results from this manuscript do not provide evidence to the statement starting from "we found that the ED scheme . . .". Please provide additional evidence (or references).

Figure 3: This figure does not show how the simulated LAI compare with observation in terms of the absolute value of LAI. It will be great to see the seasonal patterns of LAI, for which if the absolute value is not simulated correctly might have a profound effect on the magnitude of GPP.

Interactive comment on Geosci. Model Dev. Discuss., 8, 5809, 2015.