Geoscientific
Model Development
Discussions

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.

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Anonymous Referee #2

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.

» We would like to express our appreciation to the reviewer for a very close, thoughtful, and helpful review.

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).

» Currently, in our model Vcmax is only variably by PFT and temperature, and the intrinsic quantum efficiency for Jmax is constant. We have been looking into how to model the seasonal variation in Vcmax mechanistically (both Vcmax and specific leaf

area vary seasonally together), and also have grassland SLA data and nitrogen data in addition to the temperate broadleaf trees in the studies above mentioned. We would like to introduce the seasonal variation of Vcmax, Jmax, and SLA in a next version of our model, pending better mechanistic understanding. As the reviewer suggested, a simple approach based on photoperiod as in Bauerle et al. (2012) would be a good candidate. Thus, discussion about this issue has been added.

In the 2nd paragraph of 5.2. Photosynthesis and respiration parameters of the revised manuscript: "Currently in the Ent model, Vcmax is only variably with PFT and temperature, and the intrinsic quantum efficiency for Jmax is constant. The seasonal variation of Vcmax, Jmax, and SLA could be introduced, pending better mechanistic understanding. A simple approach based on photoperiod such as in Bauerle et al. (2012) would be possible."

Furthermore, Vcmax has a large range of values, as well as large variation within a single site and single plant. Thus literature values for the Fluxnet sites where available were chosen, and the value within the literature range were tuned for the site. We have added the reference for Vcmax in Appendix Table D1 and also corrected the mistakes in numbers.

"1For all these plant functional types there is a large range of values, as well as large variation within a single site and single plant. We therefore have chosen literature values for the Fluxnet sites where available, and tuned the value within the literature range for the site. 2Oleson et al. (2004) 3Wilson et al. (2001) 4Wang et al. (2007)"

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).

» To describe the site data, we have written a separate subsection of 3.1. Fluxnet sites.

In 3.1. Fluxnet sties of the revised manuscript:

"The Ent TBM was evaluated at five Fluxnet sites, including Morgan Monroe State Forest, Harvard Forest, the Vaira Ranch, the Tonzi Ranch and Hyytiala, as briefly mentioned above (Table 3). From all sites, data from the flux tower systems were available. Meteorological driver data include radiation, precipitation, air temperature, air pressure, humidity and wind, used to drive the model. Soil moisture and temperature measurements were also used to drive the Ent standalone simulations. Flux data includes net ecosystem exchange (NEE) and evapotranspiration (ET), and were used to evaluate the simulation results. Among sites, data availability, such as LAI, varied and suited different types of model simulations as described in detail in the next section.

The Morgan Monroe Sate Forest (MMSF), located in Indiana, USA (Schmid et al., 2000) (latitude: 39.32315°, longitude: -86.413139°) is is an extensive managed temperate broadleaf deciduous forest with a total area of 95.3 km2. The area is covered primarily by a secondary successional broadleaf forest within the maple-beech to oak-hickory transition zone of the eastern deciduous forest, dominated by supar maple and tuilip poplar. LAI measurements at 5-14 day intervals during the growing season were available for 1998-2001 {Andrew Oliphant, 2006 #1646}). Harvard Forest (latitude: 42.5313°, longitude: -72.1898°) is an eastern temperate mixed forest dominated by deciduous trees. The area surrounding the flux tower is dominated by red oak and red maple, with scattered stands of Eastern hemlock, white pine and red pine. About 1/3 of the existing red oaks were established prior to 1895, another 1/3 prior to 1930, and the rest before 1940, and thus the stand is 75–110 years old (Urbanski et al., 2007). O'Keefe (2000) provides the leaf phenology of Harvard Forest. The timings of spring leaf development and fall leaf fall have been recorded for permanently tagged individ-

uals in the field from 1991. The leaf development and senescence data in percent of final leaf size have been used to obtain 'observational' LAI based on the maximum LAI in the model, i.e., (observed LAI) = (observed % of leaf development or fall) X (modeled maxmimum LAI).

The Vaira Ranch (latitude: 38.4066667°, longitude: -120.950733°) and Tonzi Ranch (latitude: 38.4316°, longitude: -120.9660°) in Ione, California, are located in an open grassland ecosystem and an oak/grass savanna ecosystem, respectively, in a Mediterranean climate of cool wet winters, and dry hot summers. The sites are less than 3 km apart. The grasses of both sites are C3 annual species whose growing season is during the winter to spring wet periods. Deciduous blue oaks domimate the savanna overstory of the Tonzi, with a growing season overlapping the grasses during the spring and continuing through the summer drought. In these sites, LAI measurements were available along the tower footprint for 2001 in approximately 2-week increments during the growing season (Kiang, 2002).

Hyytiala (latitude: 61.8474150°, longitude: 24.294770°) in Finland is situated in needleleaf evergreen forest dominated by Pinus sylvestris (Scots pine), in which the phenological behavior of interest is frost-hardening. The climate is boreal. Flux measurements and soil moisture and temperature are available. For seasonal LAI, we used the site investigator's description of a constant minimum all-sided needleleaf LAI (75% of maximum) in January-May, linear increase over June to its maximum of 3.9, remaining at the maximum LAI during July-September, linear decline to its minimum in October, and a constant minimum LAI in November-December (Kolari, personal communication, 2007)."

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.

» The suggested references point out the importance of photoperiod for spring phenology, but it is also implied that such sensitivity to photoperiod is not universal to all cold deciduous trees (Korner and Basler, 2010). We therefore have added such discussions with the suggested references.

In the 1st paragraph of 5.2. Cold deciduousness in the revised manuscript:

"For cold deciduous trees, we used the growing degree days and chilling requirements in spring phenology (Botta et al., 2000) and temperature and photoperiod in fall phenology (White et al., 1997; Jolly et al., 2005). While we have taken a widely used approach, some recent studies suggest other possible approaches. For spring phenology, the importance of photoperiod has been pointed out in recent studies (e.g., Korner and Basler, 2010; Migliavacca et al., 2012). Korner and Basler (2010) suggested that when the chiling requirement is fulfilled, plants become receptive to photoperiod signals and such sensitivity to photoperiod is found in late sucessional species in mature forests. For fall phenology, Delpierre et al. (2009) used chilling degree day-photoperiod to model leaf coloring change for deciduous trees in France, and Yang et al. (2012) and Archetti (2013) found the model suitable for New England, USA, with different parameter fits. In general, despite agreement about overall climate cues for cold deciduousness, further work is needed to uncover 'site-independent parameterizations.'"

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).

» For fall phenology of cold deciduous tree, we have used the temperature and photoperiod as in Eq. 5 with modifications to White et al. (1997) and Jolly et al. (2005) as their approaches have been evaluated for US and the globe, respectively. On the other hand, Archetti et al. (2013), modified from Delpierre et al. (2009), used the same variables but with different functional forms and evaluated the model for a New England Forest. We agree that it would be interesting to evaluate Archetti et al.'s model, and thus we have added the suggested references in the text to note them as a possible approach could be utilized in the future.

In the 1st paragraph of 5.2. Cold deciduousness in the revised manuscript:

"For cold deciduous trees, we used the growing degree days and chilling requirements in spring phenology (Botta et al., 2000) and temperature and photoperiod in fall phenology (White et al., 1997; Jolly et al., 2005). While we have taken a widely used approach, some recent studies suggest other possible approaches. For spring phenology, the importance of photoperiod has been pointed out in recent studies (e.g., Korner and Basler, 2010; Migliavacca et al., 2012). Korner and Basler (2010) suggested that when the chiling requirement is fulfilled, plants become receptive to photoperiod signals and such sensitivity to photoperiod is found in late sucessional species in mature forests. For fall phenology, Delpierre et al. (2009) used chilling degree day-photoperiod to model leaf coloring change for deciduous trees in France, and Yang et al. (2012) and Archetti (2013) found the model suitable for New England, USA, with different parameter fits. In general, despite agreement about overall climate cues for cold deciduousness, further work is needed to uncover site-independent parameterizations."

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.

Please be advised that a mistake in the copyediting process led the incomplete sentence in the beginning of the manuscript. We have corrected it.

In the 1st paragraph of 1. Introduction of the revised manuscript:

"Phenological timing remains a major weakness of land surface dynamic global vegetation models (DGVMs) that are coupled to general circulation models (GCMs), and a primary cause of uncertainty in predicting the trajectory of global atmospheric CO_2 (Friedlingstein et al. 2006, Friedlingstein et al. 2014)."

P5820 L14: "ntropical" to "tropical". » We have corrected it.

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".

» We have corrected the equation and the phrase was clarified as follows:

"at the beginning of the winter season (when Phenostatuscd switches from 4 to 1)."

P5823 L16: What control the rate of leaf dropping? P5831 L14: Please add the unit for the "RMSE of âĹij0.4"

» We have added unit as follow: "RMSE of ~ 0.4 μmol/m2/s."

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".

» We have corrected it to Fig. 8. As well, we have clarified the sentence with the

evidence.

In the 1st paragraph of 4.3.2. Fluxes of the revised manuscript:

"Frost-hardening suppressed photosynthetic capacity during the winter (particularly in Feb-April) and therefore GPP and NEP. It also suppressed transpiration and thus ET, but a relatively small difference in ET was detected between the simulations with and without the frost-hardening scheme as the RMSEs with observations were 7.88 mm/s and 7.89 mm/s, respectively (Table 6)."

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).

» The ED scheme impacts our simulated carbon fluxes, although not the phenology, so we wished to mention this for future work. We did not think a plot of sapwood variation as necessary for this paper, because it was not realistic, is not a widely used scheme, and would not have lent insight to the literature. As per reviewer's suggestion, we have re-written the paragraph as follows:

"Although the ED carbon allocation/growth scheme is not the topic of this study, it is necessary to address how deficiences we encountered in this scheme impacted our carbon flux results. Although the current carbon allocation and growth scheme results in LAI that is reasonable, with some phenological timing issues as noted, the maximum LAI is achieved thanks to a cap on LAI by allometric relations to stem structure and plant density, while the rest of the plant carbon balance is not realistic, particularly with regard to rate of LAI growth, amount of seasonal sapwood growth and conversion to heartwood, accumulation of carbon reserves, and allocation to reproduction. The on/off cues of the Ent phenological factor for cold deciduous trees results in unrealistic fast full leaf-out, which could be rectified by introduction of a physically-based cell growth elongation factor (Lockhart 1965). For future work, we determined it would be more realistic to make carbon allocation to each live pool independent. The ED scheme's

allocation to one live biomass total and then partitioning among the live pools can lead to unrealistic behaviors for sapwood patterns during spring growth and fall senescence, due to a partitioning scheme for live carbon that does not account for the different seasonal behaviors of each live pool."

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.

» In this study, we use an "active biomass" phenology-only mode: canopy stem structure is prescribed and static, while seasonal leaf and fine root dynamics are prognostic. Therefore the annual maximum of LAI was fixed in the model as observed. We therefore simply presented the LAI relative to it annual maximum.