Alternative Methods to Predict Actual Evapotranspiration Illustrate the Importance of Accounting for Phenology: The Event Driven Phenology Model Part II.

V. Kovalskyy and G. M. Henebry

Anonymous Referee #2

General comments

The paper addresses relevant scientific questions within the scope of BG. The manuscript presents evaluates the event driven phenology model (EDPM) against evapotranspiration (ET) measurements. This is important and the authors should be praised for trying to explore the impact of phenology on relevant ecosystem fluxes.

Overall I would recommend publication. I find, however, the conclusions not particularly substantial as they mostly consider where there are differences, but much less so deeper reasons for the differences (see my comments below). Results are sufficient to support the interpretations and conclusions. The language is fluent, but in places some definitions are missing or symbols are not clear.

Significant points

P5338 l. 29| Phenology was modeled interactively even in global models much before Pitman (2003), e.g. Lüdeke et al. (1994) and Kaduk and Heiman (1996a), and effects were explored (e.g. Kaduk and Heiman, (1996b) and Kindemann et al., (1996)). I suggest that if the authors wish to explore the historical development of phenological modeling, then they need to also consider earlier work much more comprehensively. Otherwise, I would recommend to rewrite this part such that it explains and contrasts the different approaches not attempting a historical perspective.

Addressed. We used Pitman (2003) citation here because this paper is essentially the historical overview of land surface models that also had a phenology section in it. We included suggested references and inserted the following text into the paragraph:*" Lüdeke et al., (1994), Kaduk and Heiman (1996 a), and Kindemann et al., (1996) developed basic interactive phenology modules and applied them in global terrestrial carbon cycle modeling (Kaduk and Heiman 1996b) . "*

P5339 l.10| I do not think that the concept of the EDPM stands apart from “traditional” models. “Traditional” models have also determined “triggers of change” from meteorology, e.g. heat sums for leaf appearance, or have directly used environmental events, e.g. temperature dropping below a certain threshold to initiate leaf fall. Also, the EDPM uses a serious of thresholds to convert continuous meteorology into events – this is not any different from using heat sums. I find the statements here inflate the differences between the EDPM and other models and distract from the really interesting questions, e.g. what can actually be gained by including real events, such as frost.

Addressed. Simultaneous use of multiple factors for both estimation of phenological timing and building seasonal canopy trajectories is the key feature that puts EDPM apart from other interactive phenology models. To clarify this we rephrased the mentioned sentences and now they read as: *“This concept stands apart from all traditional models that use air temperature, insolation, precipitation, or other weather variables acting as the sole continuous factor determining the phenological timing or the shape of seasonal canopy trajectory. The event driven concept uses multiple continuous weather factors to estimate phenological timing while further transforming them into discrete events—triggers of change in daily canopy dynamics. Hence, daily insolation, daily thermal time, precipitation, freezing temperatures, and heat stress can simultaneously contribute to timing and shape of phenological trajectories.”*

P5339 l15| Here the authors themselves say that continuous forcing is transformed into triggers of plant responses – this is really just the heat sum approach to model leaf appearance, which is very old. Moreover, there is the problem in that the model might use triggers for plant responses which in reality one might want to consider as a response to a continuous change, e.g. plants might not respond to precipitation, but the slow increase in soil moisture. By introducing the event “precipitation” (l15) the model might actually use the wrong forcing for plant development.

Addressed. Please refer to the previous response.

P5339 l19. Well, the EDBM is not the only model with that potential, and in fact many models do not use climatologies any more.

Addressed. The sentence now reads as: "*The EDPM can simulate daily canopy dynamics from the actual weather data and, thus, it has the potential to replace climatologies in LSMs that still rely on a static approach to phenology.*"

P5340 l3-6: Questions (1)-(4). I would think that a difference that is not statistically significant should not be treated as a difference. Hence I think there are really two questions here: (1) How does the interactive phenology differ from the static phenology? (2) If there are differences, then when and where are results from the interactive phenology significantly different from the static phenology?

Addressed. The text now reads as suggested by the referee # 2: *"1) How does the interactive phenology differ from the static phenology? 2) If there are differences, then when and where are results from the interactive phenology significantly different from the static phenology?"*

P5357 I do not understand why the EDPM should be better than the retrospective MODIS. I have difficulties believing that this is solely due to the 8 day temporal resolution – certainly not in the crops? I would like to see a bit more discussion about why that might be. TNDVI versus MODIS NDVI? Does the VPD calibration for the EDPM play a role?

Addressed. We expanded the explanation for setbacks in MODIS NDVI representation of phenology. The text now reads as: "*The retrospective time series of MODIS NDVI appear to do a better job than climatologies, but the temporal details may have been lost because of missing observations due to clouds (Roy et al., 2006), 8-day release period and 16 day rolling compositing algorithm (Schaaf et al., 2002) that may have smoothed out larger fluctuations in temporal canopy dynamics.*"

Overall I would like to see more discussion how the differences come about. OK, so there are some statistically relevant differences. Where do they come from? What really makes the EDPM better than the retrospective MODIS? As part of that I would like to see more discussion about how relevant differences in phenology actually are for the differences in evapotranspiration. There is a start on that by looking at the growing

season length, but does this explain the differences in ET? What about maximum ET rates or similar, that would allow to factor out the phenology?

Response. We shared the concerns of referee #2 about the sources of differences between sets of ET estimates. We tried to look at the residuals and RMSE during anomalous years on individual sites and saw drastic difference between e.g. MODIS NDVI or EDPM results and ET derived with climatologies. Yet, because of lack of complete temporal overlap and vast distances between flux tower sites (different precipitation regimes) we could not make such analysis. We clarified this issue in the discussion as follows: *"The analysis conducted for this study is incomplete without year by year comparison of performance between the five arrangements of VegET during different phenophases. It would help reveal reasons for poor performance by climatologies during anomalous years with shifts in the timing of spring or late season droughts. ack of complete temporal overlap and vast distances between flux tower site locations prevented us from including such an analysis in this study. "*

Minor points

P5338 l1 What "factor" is meant?

Addressed. Changed to *"Temporal changes in canopy"*.

P5338 l3 What is the "canopy factor"?

Addressed. Changed to *" dynamics of canopy properties"*.

P5340 l14 I would not claim "all" climatic factors.

Addressed. Removed.

P5340 l20 What is Kcp and Kc? Please explain.

Response. We believe that "K_{cp} *is a plant coefficient driven by phenology and distinct from* K_c*, the traditional stage standardized crop coefficient recommended by the FAO (Allen et al. 1998).*" is a succinct and sufficient explanation. We have inserted a citation to the relevant FAO publication.

P 5344 l2 What do you mean by: “depending on the nature of weather factor or surface attribute.”?

Addressed. Replaced that text with this: “*daily time series of 2 meter air temperature [K] (daily average, daily maximum, and daily minimum); 2 meter specific humidity [kg/kg] (daily average); surface pressure [Pa] (daily average); U wind component [m/s](daily average); V wind component [m/s](daily average); downward shortwave radiation [W/m^2](daily sum); downward longwave radiation [W/m^2] (daily sum); total precipitation [kg/m^2] (daily sum). ”*

P5346 l21 Do you consider five not four procedures?

Response. Yes we considered 5 procedures. 1) t-test on residuals; 2) K-S test on residuals; 3) overall seasonal RMSE; 4) phenophase specific F scores; 5) season duration and total seasonal ET.

P 5353 l11 What do you mean by “coupling scheme”? This term was not used before.

Addressed. Replaced with “*VegET arrangement*”

Fig. 2. Shouldn’t there be more labels? Or state which apply to what panel in the caption.

Addressed. The caption is appended with “*Two realizations of EDPM have identical labeling. ”*

Fig. 2, 3, 6. Provide more explanation in the figure captions

Done.

Fig. 7: Unit on y axis – really day-1? I think this should be just mm m-2, no?

Addressed. “day^{-1}” is removed.

References

Lüdeke, M. K. B.; Badeck, F.-W.; Otto, R. D.; Häger, C.; Dönges, S.; Kindermann, J.;

Würth, G.;Lang, T.; Jäkel, U.; Klaudius, A.; Ramge, P.; Habermehl, S.; Kohlmaier, G. H. (1994). The Frankfurt Biosphere Model. A Global Process Oriented Model for the Seasonal and Long term CO2 Exchange between Terrestrial Ecosystems and the Atmosphere. Part 1: Model Description and Illustrating Results for the Vegetation Types Cold Deciduous and Boreal Forests. Climate Research 4(2): 143-166.

Kaduk, J. and M. Heimann, 1996a A prognostic phenology scheme for global terrestrial carbon cycle models. Climate Research, 6(1), pp.1-19

Kaduk, J. and M. Heimann, 1996b Assessing the climate sensitivity of the global terrestrial carbon cycle model SILVAN Physics and Chemistry of the Earth, 21(5-6), pp.529-535

Kindermann, J., G. Würth, G. H. Kohlmaier, and F.-W. Badeck (1996), Interannual variation of carbon exchange fluxes in terrestrial ecosystems, Global Biogeochem. Cycles, 10(4), 737–755, doi:10.1029/96GB02349.