A New Concept for Simulation of Vegetated Land Surface Dynamics: The Event Driven Phenology Model Part I

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Response to comments from anonymous Referee #2

The paper presents a new approach to model vegetation phenology called the 'event driven phenology model' (EPDM) and illustrates its performance at two Ameriflux crop sites. The paper is well written and the EPDM concept is interesting. I also appreciate the aspect of satellite data assimilation and propagation of uncertainties. I recommend publishing the paper in BG if the raised points below will be adequately addressed in a revised version.

+ The authors should clarify for what exactly the model has been developed. Is it for site-level analyses, regional or even global applications? Is it useful for all vegetation types and climate zones? It seems to me that the heavy data requirements for training constrain the application domain of the model a lot.

Done. The first two sentences of the last paragraph in the introduction now read as "*Here we start the evaluation of event driven approach for potential regional application to predict seasonal trajectories of a key characteristic of the vegetated land surface while estimating the timing of phenological transitions. As the first step towards realizing its potential, the approach is tested on flux tower site-level, where the model runs at daily time steps simulating growth and development of maize and soybean canopies at two AmeriFlux locations: Mead, Nebraska (NE), and Bondville, Illinois (IL).*"

+ The authors should clarify if their model runs operationally or needs particular 'tuning' (I do not mean training) for specific sites. Operationality would be a requirement for large scale applications and the coupling to e.g. RCMs or GCMs.

Based on results presented in the paper, we cannot say that the EDPM is yet capable of running operationally, even though the reserved validation data (independent sites) were more than 1000 km apart from the training sites. However, we can tell that the model has been already tested on a regional level where it simulated three growing seasons for more than 18K of 5km pixels within croplands of Northern Great Plains. Assessment of the EDPM performance in these trials will soon be submitted to a journal. We are working toward operationality, but incrementally.

+ In the introduction the authors refer to simple phenology models in RCMs and GCMs. However, there are better phenology models implemented in some carbon cycle /terrestrial biosphere models where phenology is interactively modeled based on daily carbon allocation. The authors may also acknowledge the work by Knorr et al 2010 and Stöckli et al 2011 in the introduction.

Partially done. Both citations were inserted into the text. However, the superiority of interactive phenology models based on carbon allocation has been questioned by Yuan et al. 2008, Rötzer et al. 2010, and others. Constrained by paper length, we have omitted a lengthy discussion of process-based phenophase control. Instead, we focused on presenting our data-driven alternative

model which is meant to be interactive, but simple and flexible enough to serve the needs of RCMs, while producing results that are comparable with remotely sensed phenologies.

+ The authors mention that their EPDM approach opens the door for ecological understanding of interacting phenology drivers while this has not been demonstrated in the paper. Consider removing the statement.
Rephrased: "*This modeling approach enables representing interactions of multiple drivers/events that drive the temporal variability of canopy characteristics (Seastedt and Knapp, 1993; Knapp and Smith, 2001; Zhang et al., 2010; Schwalm et al., 2010).”*

+ The selected sites are crop sites. Please clarify if EPDM is specifically developed for crops or works for any veg type. The authors argue that one of the advantages of EPDM is that e.g. disturbances can be taken into account. Management is somewhat related and I wonder why irrigation has not been incorporated as a driver but instead the irrigated years were removed from the analysis. Also harvest should be an important factor that is not incorporated or discussed.

Done. The following text was added into the first paragraph of section 5.3(Discussion. Directions for Improvement and Further Development): *“The model realizations of soybean and maize crops suggest that the EDPM may be implemented for other kinds of vegetation. Although agricultural management practices are a modeling target for the EDPM, a lack of data about the timing of sowing, fertilizing, irrigating, and harvesting prevented the incorporation of these management events into the experiment presented here. The event driven approach has the potential to be used for modeling fluctuations of canopy dynamics induced by sudden factors that usually fall outside the range of many conventional vegetation models, e.g., insect outbreaks, canopy damage by hailfall, etc. The interface of events could also be adapted to other factors, including those that influence surface attributes before and after the growing season (e.g., off-season precipitation, snowpack depth and duration).”*

+ Page 5293: The countdown for a start of a season is the first day of a calendar year. There are various problems with that such as the growing season may be in northern hemisphere winter e.g. in the southern hemisphere or Mediterranean systems in the northern hemisphere. Can EPDM deal with multiple cropping/growing seasons per calendar year?

Done. We modified the sentence in the section 3.2 that now reads: *“The choice of time point to commence accumulation can be used to address the particularities of phenological development of various vegetation types in different geographic regions.”*

Also, section 5.3 of the paper has a statement “*The EDPM framework is flexible and potentially can accommodate additional phases or even double and triple cropped growing seasons*”.

As for the starting point for countdown, it was chosen arbitrarily and can be customized for systems with multiple growing cycles per annum as well as for single cycles in southern hemisphere and Mediterranean. Yet, details on any of such customization would be unnecessary in this paper and speculative since such work requires consultation with experts and literature.

+ Page 5296 lines 17-19: I expect a problem with co-linearity between the drivers. Does that matter here?
This is a valid and important issue that we watched for during the EDPM training. To avoid collinearity, our training procedures used only the rain, heat stress, and insufficient insolation events that were well separated in time (at least 3 days apart from any other event). Formulation of the model [Eq. 1-3] allowed for *adequate insolation* and *thermal time* events to be treated differently (described in second paragraph of section 3.4). Further, the order of training was permuted to yield smallest variance (*J*) and to insure adequate reaction of the EDPM to the driving factors.

+ I see a potential for circularity because the 'y' variable (TNDVI) is calculated from radiation data while radiation data are also used as 'x' variable, i.e. it is on both sides of the equation. Please comment.

We have to respectfully disagree since such logic would invalidate all calibrations of photosynthesis and carbon allocation models done using, for instance, LAI observations collected with an LAI-2000. Please consider that adequate insolation events were derived from *daily* total downwelling insolation ($Jm^{-2}\ day^{-1}$) that does not depend on optical properties of the canopy to produce daily change coefficient first and the TNDVI value later. Observations of TNDVI were derived from *instantaneous* upwelling and downwelling energy fluxes (measured in $W\ m^{-2}$ at 11:00 LT) as a ratio of broad band reflectances that depend on optical properties of underlying canopy. It is possible to double the instantaneous insolation here, but the reflectance (as constant property of illuminated surface) will remain the same, and consequently the TNDVI will be the same. Meanwhile if the daily insolation is doubled the daily change (greenup phase) in the TNDVI will increase. We hope that this comment clarifies that the circularity is not an issue here.

+ I do not understand the point of including predefined phenological dates in the analysis. Please clarify or consider removing that from the paper.

Done (Clarified). The sentence was inserted into the last paragraph of the section 3.5*: "Model testing with predefined PTPs aimed to show the ability of the EDPM to mimic canopy responses to various events isolated from errors in estimation of phenophase timing that can be eliminated with more training data."*

+ I suggest indicating training and validation data in table 1.
Done

+ The draw-back of the event concept is that events have to be defined using some thresholds. Is there a way to also optimize the thresholds instead of using some literature values?
It is possible and would probably produce better results and smaller uncertainties, yet the literature gave a good starting point for the new model.

+ I would find it more interesting if the data-model comparison would explicitly elucidate to what extent the ‘seasonal curve’ vs ‘anomalies’ are captured by the model. One of the motivations for an interactive phenology model was that it can model also deviations from the mean seasonal cycle but it was not demonstrated how well that actually works. The r2 values in table 5 are dominated by the strong seasonal cycle which is relatively easy to capture.

This is done in companion paper in the section where the EDPM predictions during different phenophases were contrasted with climatologies and observations using nonparametric scores. There we avoided using the coefficient of determination for the exact reasons suggested.