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

By V. Kovalskyy and G. M. Henebry

Response to comments from anonymous Referee #1

a) Yet the overall appearance of the article is too long. If reduced by words the main information will still be carried to the reader. For example the 4 pages for the introduction part.

We find this comment to be irreconcilable with feedback from other referees seeking more detail; however, we have condensed the introduction from the original version.

b) Being more explicit in some parts. When you state that for irrigated fields you exclude heat and rainfall stresses for the model training, because they are not relevant for vegetation development. There is wide consensus about the effect of extreme events in their ability to shape plant development, due to direct input as well as lagged effects, and so on. To me it is quiet substantial to base such declaration on a more detailed basis.

We agree that this is an important issue; we have clarified this statement in the revised version. The reason for excluding irrigated sites from rainfall and heat stress training was that there were no irrigation records available (neither timing nor amount). Therefore, we could not know if there was a lack of reaction to rainfall or heat stress at these sites or the crop responses to these events were mitigated by irrigation. The amount of irrigation could have been inferred from inverting water balance equation and soil water content records, yet such an inversion would carry substantial uncertainty. Moreover, there were sufficient records of canopy responses to heat stress and rainfall at the rainfed flux tower sites which we used for training.

c) The model, as described, certainly has the potential to support Global Models, but this is not well documented. It rather leaves the impression that quiet a bunch of individual tuning is needed to produce feasible results, which in Global Modeling is a thing to prevent.

The empirical EDPM is mostly data driven and cannot avoid training since at the beginning the model has very little knowledge about the vegetation type it is meant to represent. After training, however, the generality of the EDPM is yet to be evaluated for the use in global climate models. A manuscript soon to be submitted to JGR (Atmospheres) evaluates performance of the EDPM at a regional scale in spatially explicit manner. We hope that with our further effort the EDPM will be able to realize its full potential by supporting more vegetation types, event types, and forcing factors.

d) Data sources: The article clearly lacks concerning included data. Beside the fact that a model which is advised to be used for global application is tested on more or less 2 agricultural spots, which might be representative for the region, it contains a too little temporal range to be analyzed for climatological/phenological shifts. In this context the Bondville site contributes only with 3 out of 12 potential years. To improve the manuscript needs to consider longer time spans and should additionally include major vegetation types to convince.

We agree with the comment that the temporal and spatial coverage of the data used for a big study should be considerably wider to capture the extent of phenological shifts. However, we do not intend to jump ahead of things by doing a spatially explicit application study with a poorly tested model. Addressing the referees' concerns, we have clarified in the introductions of both manuscripts that, to follow an orderly development, we report the results of initial testing demonstrating basic capabilities of the EDPM (Part I) and point based validation of the EDPM in the coupling scheme with VegET (Part II).

Minor:

5286/12 – PAR is not explained where first mentioned: Now explained

5288/04 – bsands: Corrected.

5304/72â˘ARˇ 29 wording": Comment not clear but we revised the wording at 5304/8-9