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Biogeosciences Discussions

Interactive comment on "Alternative methods to predict actual evapotranspiration illustrate the importance of accounting for phenology – Part 2: The event driven phenology model" *by* V. Kovalskyy and G. M. Henebry

Anonymous Referee #1

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Kovalskyy and Henebry present an application of their recently developed EDPM phenology model. The EPDM model is coupled to a simple model of evapotranspiration (VegET) and simulations of ET using this version of the model is contrasted with phenology based on satellite NDVI data (climatologies are retrospective time series). The authors aim at illustrating the advantages of the EPDM model version over the more traditional approaches. Overall, the manuscript is well written and the topic is interesting. However, there are many points which I criticize below.

Major points: + I don't understand why the authors do not directly compare the phenol-

ogy from EPDM and satellite data but use an additional modeling step and compare it with flux tower data. Looking at the aims and objectives, it is clear that the authors are interested in how the phenology differs among the different approaches. Using the VegET model on top causes additional problems such as the bias of eddy co-variance ET data, which are used as benchmark, but also the modification of the model to take VPD into account is problematic in this context (see below).

+ The authors do not make the purpose of the model clear, i.e. if it is meant to run in diagnostic or prognostic mode. Depending on diagnostic vs prognostic, different validations/model comparisons should be made. I assume that the model is meant to be used in prognostic mode such as forecasts since for the past phenology data from satellites are available. Therefore, I expected to see a validation where some recent years of potentially available training data are left out in the training and used for validation. In such a comparison the authors could contrast their results with those based on MODIS phenology data of the specific years (not using climatologies). In this context, a comparison with another state-of-the-art interactive phenology model implemented in biosphere models would have been interesting to see to judge on the actual value of the EPDM model.

+ In my opinion using the tower NDVI values for training is not a good test for the actual application of the model because the TNDVI values are only available at measurement stations. Training instead with satellite data would make more sense to me and be a more realistic test of the model. The comparison of results based on satellite NDVI and TNDVI is quite unfair because the towers of the used sites are short so that they have a small footprint of only a few 10 meters (I guess) while the satellite footprint is in the order of 1 km and coarser. These footprint differences may cause differences in the performance of the models caused by the resolution and may not be due to 'errors' in phenology.

+ The authors motivate their approach based on the poor representation of phenology in existing models, in particular regarding the representation of interannual variability (which is e.g. not captured when using a climatology). However, the authors also do not explicitly analyze the performance regarding anomalies (i.e. deviations from the mean seasonal cycle) although this is expected given the motivation presented in the introduction (interannual variability etc). This could have been done if the analysis was based on a validation in forward model where recent years are left out from training.

+ ET measurements from the eddy covariance method are usually low biased by 10-30%. The authors do not account for that problem nor discuss it properly. Given the often slight differences in performance (RMSEs) of the different ET models and the problems with the tower ET data I find it very difficult to judge on the adequacy of some of the models.

+ Page 5346, lines 2-8: The authors use additional information of VPD to correct some biases of predicted ET by EPDM; which means they change the model and it is not longer only a phenology model. This modification seems not to be made for other approaches that are used in the comparison, which in my opinion is unfair.

+ The performance of the EPDM (using automatic PTPs) based ET models measured by RMSEs is also not really outstanding (only in 2 out of 4 sites is (slightly) better than using MODIS NDVI) despite the fact that the EPDM model uses VPD in addition and is based on much better training data (tower NDVI vs satellite NDVI). I do not find that very encouraging.

Some specific points: + Page 5337, lines 14-28. The authors may also mention data-driven approaches to modeling ET based on eddy covariance, remote sensing, and machine learning approaches (e.g. Yang et al 2006 IEEE; Jung et al 2010 Nature)

+ Page 5338, lines 16-28. The authors refer to models with very simplistic representations of phenology (e.g. climatologies). Many models, in particular those designed for the biosphere model dynamic LAI most of them on daily time-step based on daily carbon allocation.

+ Page 5345, lines 18-20: not clear if it's only due to limited data or also due to limitations of EPDM.

+ Page 5347, line 25: absolute residuals?

+ The usage of prescribed PTPs seems to be useless to me and unfair comparison. If we knew the PTPs then we wouldn't need a model. What is the point of including them in the analysis?

+ I don't understand why PET is included in the comparison. I suggest to remove PET.

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