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Biogeosciences Discussions

Interactive comment on "A time-stepping scheme to simulate leaf area index, phenology, and gross primary production across deciduous broadleaf forests in eastern United States" *by* Qinchuan Xin et al.

Anonymous Referee #1

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The study provided by Q. Xin et al "A time-stepping scheme to simulate leaf area index, phenology, and gross primary production across deciduous broadleaf forests in eastern United States" is mainly focused on development of a new modeling algorithm to parameterize the temporal LAI and GPP variability and its application to describe the spatial patterns of LAI, GPP and phenological properties of deciduous broadleaf forests across eastern United States. Adequate parameterization of land surface and vegetation properties is a very important scientific task for modern biogeochemistry. New algorithms can be very useful to solve different applied problems related to ade-

quate description of the land surface - atmosphere interaction in different spatial and temporal scales.

In the paper authors showed new modeling results and their comparison with data obtained using previously developed approaches. Obtained new results however didn't show any significant accuracy improvement in GPP estimations. The difference between measured (derived from measured NEE) and simulated GPP (fig. 3) is still very high.

Other point for discussion is model assumptions used in the study. In particular authors assumed a linear relationship between the steady-state LAI and the corresponding GPP. However in reality the GPP is non-linearly depended on LAI (not only on total LAI but even on LAI of green biomass) mainly due to non-linear PAR (photosynthetically active radiation) interception within a plant canopy. Such effects are especially pronounced in dense plant canopies with a high LAI. GPP rate is linked with forest and tree architecture. The leaf photosynthesis properties are also varied among different vegetation types. The assimilation rate is depended on biophysical properties of individual plants, water availability, nutrient supply, etc. So, the correctness of made assumption in the study is not obvious and it needs additional discussion.

Authors pointed out in result chapter about a gut agreement between leaf phenology derived by new method and MODIS data. It is true. But it is not clear from the paper the reasons for available differences between tower observed time of foliage expansion (indicated in shape of black GPP curve) and corresponding time predicted by developed model (fig 1 a-b, page 11)? The model actually predicts earlier leaf onset in spring than in situ observation (GPP data).

In the first half of introduction authors used many well known statements such as e.g. "energy and mass exchange in a plant canopy can be modeled as a function of environmental conditions (e.g., sunlight, soil moisture, temperature, and humidity) and vegetation LAI "and refereed them to most recent own publications only, and not to

available synthesis studies conducted during the last several decades and focused on the same problem.

I find that the discussion chapter is too short. It should be extended. All obtained results have to be discussed in more details.

The sentence in page 4 is not clear "leaf dynamics takes days or even months in response to climate variation". I guess authors mean weather not climate variations. Time scale for climate variation is much larger.

I'm not agreed also that the term potential evapotranspiration assumes the fixed LAI (page 3) for any hypothetic canopy. Fixed LAI can be obviously used for calculation of "reference evapotranspiration" but not potential evapotranspiration. Potential evapotranspiration rate can be estimated for plant canopy with different LAI values.

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