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# *Interactive comment on* "Identifying environmental controls on vegetation greenness phenology through model-data integration" *by* M. Forkel et al.

**Anonymous Referee #2**

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This article aims to improve the phenology of the LPJmL model by introducing a new phenology scheme based on the GSI model of Jolly et al. (2005), and by optimizing the original and new phenology, model productivity and albedo using several datasets. Forkel et al. evaluate the results of the optimised models against different metrics of the same datasets at we're used in the optimisation, and against independent data. They find that the temporal and spatial dynamics of most variables are improved using the new GSI-based model. The authors also use this model to infer the main environmental controls over phenology, and find that watt limitation plays a role globally, which has not previously been documented.

This is an important contribution to the literature as phenology controls the seasonal dynamics of net C uptake as well as impacting the hydrology and energy budgets, with



important short- and long-term feedbacks to the climate (Penuelas et al., 2009). Given that studies have shown that DGVMs cannot always reproduce observations of Leaf Area Index (LAI), it is important to that the representation of phenology in DGVMs be improved, as suggested by several authors (Richardson et al., 2012; ).

This is a very comprehensive and detailed study, and overwhelmingly provides evidence towards the main conclusion that the GSI model performs better than the original phenology scheme. The paper is well-written and structured, and the aims, methods and conclusions are very clear. There are possibly too many figures in this paper however, especially for the model evaluation, which I fear could prevent some from reading the manuscript thoroughly. The number of figures could be reduced by focusing on fewer features of the temporal and spatial analysis, such as choosing between the mean seasonal cycle, monthly time series and annual time series. This choice could be guided and justified by which scales the authors think are most important in terms of getting a correct representation of phenology in DGVMs.

The weight given to benchmarking the model, although useful, precludes more discussion on remaining model deficiencies and what further work could be done to improve the phenology schemes. It also leaves less room to discuss the wider impact of the improved phenology on other variables and feedbacks between the phenology and the carbon, water and energy cycles. These two points are discussed at various points, but the balance between this type of discussion and the results of the benchmarking could be slightly more even. For example, the relative lack of impact of the new GSI-based model on ET is clearly stated but the significance of this in terms of using this new model, and what further work might be needed to improve this situation, is not discussed further.

The authors stop short of making recommendations to other modelers. The results show, and they state clearly, that the GSI-based model performs better, even before optimization. Both implementing new models and optimizing existing ones takes considerable time and effort (which is one reason why this exhaustive study is so impres-

sive), so what can they suggest to other modeling groups. Would they suggest that in order to improve the phenology, researchers working with other models might want to consider implementing some version of the GSI model, even without optimization? This discussion would be a further contribution, given the GSI model is a significant shift from the typical type of phenology models used in many other DGVMs.

One final remark, the authors emphasize that water can be limiting in all ecosystems, but in the boreal tundra regions for example water may be limiting because the permafrost has not thawed, but this surely this is ultimately controlled by temperature? Light also seems to be limiting in all ecosystems. Also, one technical point, it would be interesting to see the correlations between these environmental limiting parameters, as their sensitivity and and posterior value is conditioned on their prior ranges (which may be unrealistic for certain ecosystems) and the uncertainty on the observations. The authors have acknowledged some deficiencies with the different datasets in some regions. Could such deficiencies influence one particular parameter value which could then alter the values of other parameters? If so, could this possibly result in an over-emphasis of conclusions drawn from analyses using these posterior parameters?

Minor points: - The sentence structure could be improved in places, for example: p21 lines 21 and 22 p26 line 26 p28 line 6

- Some figure references do not appear to be what they should be, particularly in section 3.2.3

- p 26 line 12 should be Congo Basin.

- In figure E2 the y and x axis scale and labels could be removed except on left and bottom in order to make figures bigger and more easy to see.

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Interactive comment on Biogeosciences Discuss., 11, 10917, 2014.