

Interactive comment on “Improving the dynamics of northern vegetation in the ORCHIDEE ecosystem model” by D. Zhu et al.

Anonymous Referee #2

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Authors attempt to improve and test the dynamic vegetation module of the ORCHIDEE model to primarily show that inclusion of new bioclimatic constraints that induce mortality lead to better simulation of fractional coverage of PFTs in mid- to high-latitude regions.

The manuscript is reasonably written but as a reader I have some concerns, which if addressed will strengthen the manuscript significantly. In addition, I am attaching the scanned version of the annotated manuscript, as a supplement, on which I have made several comments. These are primarily minor comments.

Major comments

1. My first major concern is that there is no equation in the manuscript that will allow

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a reader to see how competition between PFTs is modelled. Scanning through the Krinner et al. (2005) GBC paper, I am unable to find an equation like the following ...

$df/dt = \text{establishment} + \text{encroachment into inferior PFTs} - \text{mortality} - \text{take over by superior PFTs}$

where f is the fractional coverage of a PFT and I assume is the primary variable of interest.

2. Second, the paper fails to acknowledge that by including more and more bioclimatic constraints we are essentially turning DGVMs into biogeography models. We all realize that the current generation DGVMs use phenomenological approaches. If the physiological processes in the model were sufficiently process-based we would never need bioclimatic constraints to include mortality. Yet, as modellers, we keep digging empirical evidence to find more and more bioclimatic constraints. Consider the three additional constraints used in this manuscript - tree mortality during extremely cold days, broadleaf tree mortality caused by spring frost and growing-season temperature limits to tree extension - all of which are temperature related in one form or another.

In absence of a df/dt equation, and an overall large stress on mortality due to bioclimatic constraints, I am inclined to ask to what extent has ORCHIDEE become a biogeography model, in which the spatial distribution of PFTs is determined primarily by their bioclimatic constraints and not by the explicit competition between them.

3. As a reader, I found several of the new metrics difficult to appreciate.

The beta metric used in equation (7) and (8) is essentially the square root of sum of square of difference between model and observations over all PFTs. I am unable to understand why is this limited between 0 and square root of 2. If there is only one PFT in a grid cell covering 100% of the grid cell and model simulates its fractional coverage to be zero, maximum value of beta is obtained equal to 1. If there are two PFTs covering the grid cell say 50% each, and say the model again simulates zero

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fractional coverage then $\beta = \sqrt{(0.5-0)^2 + (0.5-0)^2} = 0.70$.

Why not use the already established root mean square error (RMSE). Beta in essence is very similar to RMSE. Why unnecessarily confuse your reader?

The SV metric used in equation (9) is okay, but would make more sense if it were based on RMSE rather than the beta metric.

Finally, another metric D (absolute difference) is introduced when comparing PFT groups and although an argument is made at the bottom of page 2231 why beta is not used, I am unable to follow this argument.

Note that, with all these new metrics, the manuscript still does not compare the good old mean fractional coverages of PFTs with observations. What is instead shown is the composite color map, which if I am not wrong shows relative abundances and not the absolute values. I realize that a composite map can show more PFTs but relative abundances is a derived quantity and that's not what the model simulates. In my humble opinion, composite maps should be complementary to the usual maps of absolute fractional coverages, not something that replaces them.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/8/C519/2015/gmdd-8-C519-2015-supplement.pdf>

Interactive comment on Geosci. Model Dev. Discuss., 8, 2213, 2015.