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Biogeosciences Discussions

Interactive comment on "Impacts of trait variation through observed trait-climate relationships on performance of a representative Earth System model: a conceptual analysis" *by* L. M. Verheijen et al.

Anonymous Referee #1

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The authors implement observed trait-environment relationships for SLA, Vcmax and Jmax into a DGVM embedded in an earth system model (ESM) and explore the differences in simulation results for GPP, NPP, biomass and biome distribution, as well as simulated climate . They show that default values differ strongly from the observed relationships and that implementing the observations has a huge impact on the simulation results, thereby demonstrating how important plant physiology can be in the earth system. The research is well represented and the paper is well written.

The topic is important and I applaud the authors for this effort. I think, however, that

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the manuscript should be improved by addressing the following points:

1.) It is stated that JSBACH is representative of most DGVMs in ESMs. This statement implies that other DGVMs probably would behave similarly. I don't agree that this is necessarily true. In JSBACH, vegetation dynamics are very much driven by the relative productivity of different PFTs, while, in other models, other factors could be more important, e.g. a different set of bioclimatic limits, competition for light and fire disturbance. In LPJ, for example, Vcmax is not a parameter, but is calculated in a prognostic manner.

2.) The authors state that model simulations improved with variable trait-environment relationships, which is shown for the biome distribution, but the opposite is true for GPP. All together, I don't think the model simulations improved. This is okay, for reasons given in the manuscript, but I think the improvement story should be emphasized less. It is still very interesting to see how large the impacts of the implemented changes are.

3.) The competition between the two tropical types changes substantially with the observed trait values, but evergreen and deciduous types were assigned the same Vcmax and Jmax values because so few data points were available. This should be discussed more, in particular as deciduous types are known (and shown here) to have higher Vcmax and Jmax on a leaf area basis. In other words, using one value is simply wrong (but models are always "wrong"; decisions like this are necessary to parameterise them).

4.) One should shortly discuss that the large sensitivity of the model results to a few physiological parameters casts some doubt on the predictive power of such models. Clearly more work is necessary to explore the model behaviour and to test the results. The model might be as sensitive to changes in other parameters, which are known to vary substantially, e.g. leaf turnover rates, rooting depths, wood density, max. longevity etc.

Presentation: I think it would be good to integrate Fig. S4.1 into Fig. 5, with the same

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legend and colour scheme as the other maps.

Minor:

Introduction: DGVMs were not only developed for the sake of coupled ocean-biosphere-atmosphere models. They are also important for biogeographical research in general and climate impact studies.

Page 18911, top: One should mention the aDGVM (Scheiter and Higgins 2009 in GCB)

Set of environmental predictors for trait-environment relationships: I am surprised that no drought index was used. For radiation and soil moisture, what values were used? Annual averages? Nutrient availability could be important and was not used as this version of JSBACH does not include an N cycle, but soil nitrogen levels from existing soil databases could have been used, e.g. from the Global Gridded Surfaces of Selected Soil Characteristics (IGBP-DIS) dataset.

Page 18923, comparison of kappa statistics with other studies: One should consider that achieving a high kappa value is easier with fewer vegetation classes. Furthermore, Hickler et al. did not use standard LPJ, but LPJ with an implementation of hydraulic architecture.

Page 18925, comparison of Chen et al. and Beer et al. Is the leaf clumping really that important for the estimate of Beer et al., which was primarily based on eddy-covariance, meaning real ecosystems with clumped leaves?

Page 18926, lines 20-29: Adaptive processes could be much slower, but it is only discussed that acclimation can be much faster than the implemented annual time step.

Typos and formulations:

Page 18910, line 29: “plants to adapt” (not plant)

Last line: “provides a first concept”: reformulate!

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Page 18914: “insights from Simioni et al.” sounds vague. Were their formulas or findings directly implemented?

Page 18918, line 14: “manifested”, not manifest.

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