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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 #2**

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This paper presents an approach to improving the representation of plant diversity in ecosystem models. The authors utilize the TRY plant trait database to generate a linear regression-based predictive model of three plant traits which is driven by climate and soil moisture variables. The model has reasonably good predictive power for Vcmax and Jmax but performs more poorly for specific leaf area. The model is applied within the JSBACH land surface model to adjust these three traits each year in each grid cell. The resulting model is slightly better at predicting the distribution of plant types, but now significantly overestimates GPP.

This approach is probably the most transparent way of including the variation of plant traits observable in databases into a dynamic vegetation model, and thus it addresses a key concern with predictions of future and present vegetation behavior, which is that 'static' plant traits significantly underestimate both diversity of plants and the capacity of plants to adjust to their environmental conditions.

My issues with this approach are primarily that I do not really think that the new model predictions of how plant growth adjusts with climate/trait relationship have any significant meaning. This is primarily to do with the fact that the choice of traits appears to be slightly arbitrary, and ignores the concomitant changes in other traits that are implied by virtue of altering SLA, Vcmax and Jmax. The most obvious example of this is the leaf lifespan trade-off with SLA, which, while being documented extensively by many of the authors of the paper, is ignored here. Given this tradeoff, increases in SLA are very likely to be associated with shorter leaf lifespans, and ignoring this relationship means that the plants with higher SLA are not paying the full 'cost' of this new life history strategy. Under these conditions, it would be unsurprising if changes in the competitive ability of different plants were to result from changing SLA because there is no cost associated with having thinner leaves.

This is less true in the case of the Vcmax changes, as JSBACH also increases respiration costs when Vcmax increases, but increases in photosynthetic capacity also imply other changes in, for example, the amount of sapwood required to transport increasing amounts of transpired water and in the quantity of roots required to supply Nitrogen for the higher N demanding tissues. While tackling all of these tradeoffs is challenging, I think that including the leaf lifespan trade off in the calculations is critical, as is at least discussing the potential for other traits to alter in sync with these three chosen traits.

Similarly, more discussion is needed of alternative methodologies for representing plant trait diversity. For example, while it does not use trait databases in this way, the 'adaptive DGVM' framework has contained methods for adjusting parameter values with environmental conditions for some time. Similarly, many plant traits are actually vari-



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able in some existing DGVMs, including Vcmax within the LPJ model (as discussed in a previous comment). Further, other models integrate trait variation by increasing the number of plant functional types such that diversity is better represented (e.g. the JEDI model) and others (ED1.0, CLM4.0) have already utilized trait data to asses mean parameter values. Ignoring the wider literature on this topic will make the paper a less interesting contribution to the discussion.

Specific Comments. P18909 L10: Reporting the deviations of the original trait values from the new ones doesn't strike me as the most interesting aspect of this paper. I would report on the strength of the climate-trait predictive model, as that forms the core of the methodology.

P18910 L20: This method doesn't address within-community variation, so this comment is slightly misleading.

P18911 L18: Habitat filtering may lead to convergence, but the figures in Wright et al. 2005 seem to indicate that within community variation is still very high. Presumably this database can offer some insights into the strength of trait convergence. Could this be discussed in some more detail?

P18912 L10: Could there be some discussion of why you chose these three traits, particularly with reference to the fact (discussed later) that Vcmax and Jmax are very closely correlated?

P18912 L 6: This sentence about how this DGVM is representative of other DGVMs is imprecise and could be omitted.

P18912L15: How do the feedbacks of plant traits on to their environment work in this study?

P18913 L 22: This description of SLA is slightly odd. Surely it controls the relationship between leaf area and leaf mass?

P18915 L 8: What are 'global quantitative deterministic predictions' in this context?



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Interactive Comment



P18917 L 26: AS far as I am aware, the Saatchi et al. 2012 biomass data are the most up to date, especially for the tropics.

P18919 L 22: This part of the paper - reporting how predictable plant traits are from climate drivers - is to me the most interesting section and could be expanded a lot. It would be interesting to see a) how the traits relate to individual climate variable and b) how the regression models predicts that they vary in space.

P18920 L 15: The GPP predictions are now much higher, even though the values of Vcmax are in theory much more accurate. Some discussion of the reasons for this would be useful. How does the model treat within-canopy variation of photosynthetic capacity, or colimitation of photosynthetic rates, or stomatal conductance? This is obviously a major structural issue that needs addressing before this can be deployed in any standard version of the JSBACH model.

P18924 L15: Similarly, it would be useful to see some graphical depiction of the biomass outputs of the different models, either latitudinal plots or maps?

P18926 L 25: If this approach is broadly accepted and applied, then why would this paper be novel? I don't think it is broadly applied, so it seems strange to suggest that here, and is in conflict with most of the introduction.

P18926 L 5-10: The comparison with the nutrient studies is slightly odd, as 1) those models are much more complex and are predicting trait values rather than assigning them from empirical look-up tables and 2) their aim is to incorporate nutrient dynamics, not variable PFT definitions. I think this section is a bit unnessecary.

P18926 L 15: Does the word 'causality' really fit here? Should this sentence not use 'correlation'?

P18927 L1: I think the timescale of 1 year is reasonable, but it is likely that plants vary in their plasticity as well as their mean trait values, so there is probably no 'correct' answer here.



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P18929 L3 : The GPP values are made significantly worse here, so I don't think that 'DGVMs are improved' is the best conclusion here. I would concentrate on the fact that some plant traits (vcmax & jmax, not SLA) are highly predictable from climate drivers. This is the strongest message here. Obviously, these key traits have large influences on ecosystem models, but as you state at the beginning, the models are already 'tuned' to give OK results, so whether they get better or worse is largely irrelevant. If they get worse, it is likely that correcting one set of parameters reveals compensating deficiencies in another set of parameters or processes. Certainly, the representation of plant diversity and plasticity of the traits in question is improved by this process, and that is almost certainly a good thing.

P18939 L5: You haven't tested all DGVMs here (that would be an interesting figure. . .) so you can't make this claim.

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Interactive comment on Biogeosciences Discuss., 9, 18907, 2012.