

## Response to Referee comment C7026–C7029.

*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 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.*

Response:

We thank the reviewer for his elaborate commentary on our manuscript.

We did not mean to imply that JSBACH has the same setup as other DGVMs, neither that it will respond in the same way as other DGVMs. What we implied with the comment that JSBACH is representative of most DGVMs, is that it has been elaborately tested and validated and used in model intercomparisons for the IPCC, and as such accepted as a state-of-the-art DGVM. We will rephrase the statement to clarify this, as it was unclear for another referee as well.

*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.*

Response:

Looking at biome distribution and biomass, the model with variable traits performs better than the default model. We agree GPP does not improve, but we would like to point out the default model also overestimates GPP in the tropical areas. However, we agree that calling this 'improved' might be too strong, as another referee also commented on this, and we will de-emphasize this aspect.

*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).*

Response:

Indeed, modeling Vcmax<sub>25</sub> and Jmax<sub>25</sub> in the same way for both tropical tree-PFTs was necessary due to the limited number of available data points. We believe that grouping data for these 2 PFTs together will give a better estimate than modeling those traits based on their few individual data points, which would lead to even larger uncertainties. We agree that our approach is a simplification and in our revised manuscript, we will indicate it is likely that in reality the deciduous tropical PFT

will have higher  $V_{cmax}$  and  $J_{max}$  values. Despite our simplification and the combined regression, the results show that there were still shifts in dominance among the 2 vegetation types. This can be explained by differences in other plant properties affecting productivity (e.g. leaf shedding rate) and consequently dominance, as NPP determines the competitive ability of the PFTs. Some of those effects might be amplified or reduced if we would have had better data to distinguish among trait values of the tropical PFTs. We will add these notes to the revised manuscript.

*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.*

Response:

We agree that the model can be sensitive to other physiological parameters as well and that this questions the predictive power of DGVMs in general. However, the default version of JSBACH (as most DGVMs) has been parameterized to approximate realistic results. This may result in correct predictions, while on the other hand it might be less realistic with respect to parameter (trait) values as used in the model - as our trait-analysis already showed. So we agree that it casts doubt on the predictive power of such models for projections, which may partly explain the large variation in projected model behavior, found frequently (e.g. Sitch et al. 2008; Friedlingstein et al. 2006).

Unfortunately, we cannot investigate the sensitivity of some other important traits, like the ones suggested by the reviewer, although that would be interesting, particularly if the trade-offs among the traits would be accounted for too. The reason is that we already selected *all* the PFT-dependent trait parameters in the model for which a link to observational trait-data was possible (e.g. wood density and rooting depth are not parameters in the model, while leaf turnover rates are fully determined by climate in the model). It seems that this was not clear from our original manuscript, as another reviewer had a similar comment, and we will explain this in more detail in the revised manuscript. Note that despite this limitation, it is known that such models are more sensitive for leaf traits, as traits like  $V_{cmax_{25}}$  and  $J_{max_{25}}$  to a large extent determine productivity, than other plant properties and therefore choosing  $V_{cmax_{25}}$  and  $J_{max_{25}}$  presents a good test case for our proof of principle.

*Presentation: I think it would be good to integrate Fig. S4.1 into Fig. 5, with the same legend and colour scheme as the other maps.*

Response:

Fig. 5 shows the global distribution of dominant PFTs for the 3 simulations. Fig. S4.1 shows the potential vegetation map with aggregated PFTs of Ramankutty and Foley (1999). We think it is a good idea to integrate figure S4.1 into figure S5, such that vegetation distribution of the simulations can be easily compared with the observational vegetation map, and we will do so in the revised manuscript.

*Minor:*

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

Response:

We agree with this and will add other purposes for which DGVMs were developed.

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

Response:

We agree with the discussion about different approaches on incorporating more variability in PFTs should be extended, as another referee suggested a more elaborate discussion as well. We refer to the revised manuscript for this, where we will discuss the adaptive DGVM too.

*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.*

Response:

In our choice of variables, we were constrained by environmental predictors that were modeled in JSBACH to derive our environmental drivers. Given these constraints, soil moisture was the best available estimate of a drought index.

We used annual averages for soil moisture and radiation (as for the other climate variables) and updated these once a year. We will make this more explicit in section 2.2. We also refer to this section for reasons why we updated only once per year.

Furthermore, we are aware of the existence of global soil maps. However, a global soil nitrogen map would not change over time (as it is not calculated/updated within the model) and can therefore not induce variation in traits, and as such is not useful in our approach. Note: the current version of JSBACH does not include a nitrogen cycle.

*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.*

Response:

We agree this could be explained more elaborately. We will add the number of PFT-classes to the kappa statistics and include a remark on the dependence of kappa on the number of classes. We will also extend the formulation about the LPJ-version used in this comparison.

*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?*

Response:

Beer et al. (2010) also uses remote-sensing data for global mapping of GPP from eddy-covariance data from flux-towers. As remote-sensing data of fAPAR and LAI are used to extrapolate data, this leaf clumping may therefore have an effect on global GPP estimates. Of course we don't know to what extent, we only want to point out that Beer et al. (2010) might underestimate these values, especially in areas where the clumping of leaves is high, e.g. in dense tropical forests. We also would like to point out that the number of flux-towers in tropical regions used to estimate GPP is low compared to other regions (see Fluxnet website: <http://fluxnet.ornl.gov>), so the estimated GPP might not be as certain as the ones in temperate regions.

*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.*

Response:

For the selected traits, we believe the time step of a year is reasonable, given their high potential for acclimation (without the need for genetic adaptation). We also suggest (line 25/26) that for other traits the potential trait shifts of acclimation are much more constrained, e.g. for wood traits. Shifts in those traits would rely more strongly on genetic adaptation and species turnover. However, as this seems not to be clear enough, we will make the possibility of adaptation and species turnover over longer time-scales for other traits more explicit in the text.

*Typos and formulations:*

*Page 18910, line 29: "plants to adapt" (not plant)*

Response:

We will change the formulation.

*Last line: "provides a first concept": reformulate!*

Response (of page 18911):

We will change this into 'provides a proof of concept'.

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

Response:

The formulas were used directly. This will be made explicit in the manuscript.

*Page 18918, line 14: "manifested", not manifest.*

Response:

We will change the phrasing.