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Interactive comment on "A stand-alone tree demography and landscape structure module for Earth system models: integration with global forest data" *by* V. Haverd et al.

Anonymous Referee #2

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In this manuscript the authors adjust an existing vegetation model, the CABLE-POP model, for various forest systems world wide. The authors show that the model can reproduce self-thinning curves and leaf to stem biomass ratios in good agreement to observed data. Despite interesting model results, I have several comments.

One comment is related to the motivation of the model development. The POP model is presented in the context of DGVMs and it is presented as an important innovation for earth system models. I fully agree that DGVMs need to be improved and that the representation of vegetation structure is an issue in these models. Yet, my personal opinion is that model efficiency is not a problem and that improving model performance does not really bring dynamic vegetation modeling forward. I am sure that most DGVM de-

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velopers have huge computer resources and compared to climate models, vegetation models are cheap in terms of CPU time. I rather think that DGVMs can be improved by increasing the ecological realism of these modes for instance by improving competition models or by making use of the huge amount of plant trait data that is now available. Therefore, more sophisticated models such as ED or LPJ-GUESS or individual-based models such as SEIB-DGVM or aDGVM are necessary.

I do also not agree with the statement that DGVMs should be deterministic and that stochasticity in models is a potential disadvantage (p. 2346, l. 17). Nature is stochastic and there is also evidence for bistability in certain ecosystem types. For example, many regions where we find savannas and grasslands could also support closed forests in the absence of fire (Staver et al., 2011, Hirota et al. 2011, Higgins and Scheiter, 2012). The observed ecosystem type is not necessarily deterministic but it may be related to the system's history and stochasticity in vegetation dynamics and disturbance regimes.

I am not convinced that the model structure can adequately represent competition and the dynamics of different cohorts because interactions between cohorts are fully deterministic (but maybe I am wrong or misunderstood details of the model). For example, if several communities are initialised at the same time, then the properties and dynamics of all cohorts are identical and a cohort model is not really required. This could happen in a catastrophic disturbances where all cohorts are affected. When one cohort is introduced per year as it is done in the model experiments, the carbon allocation to a younger cohort is by definition less than carbon allocation to older cohorts. Hence, it is necessarily outcompeted. There is no chance that a new cohort may outcompete an older one. This is also shown in Fig. 6v and 6vi where biomass is dominated by only one cohort whereas all other cohorts are outcompeted.

p 2345, l 16: Replace ass by as

p 2348, l 21: Does the model simulate size or age classes? In fire driven systems such as savannas, trees have a high capacity for re-sprouting from their root resources such

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that they can be old but small.

p 2352, l 1: Calculation of Dy not described in main text

p 2352, l 10: Disturbances are, in the model, not linked to climate or the ecosystem state.

p 2352, l 15: It is not clear to me how the link between POP, CABLE and functional types works. These models only exchange biomass and turnover but no information about PFTs. However, in the analyses, needle-leaved and broad-leaved trees are considered. Are there two cohort models for two PFTs or is POP only used for the dominant PFT?

p 2354, l 24: fc not mentioned in eq A19, do you mean eq. 5?

p 2356, l 5: insert "=" after alpha

Large parts of the discussion could be moved into the results section.

p 2358, l 1: "trajectory never reaches the upper bound of the C-U data" Fig 6i suggests that the trajectory exceeds C-U data fit but does not converge towards this line

p 2360, l 21: The authors argue that big wood models "should be phased out from use in carbon cycle studies" which implicitly suggests to the reader that the presented POP model should be used. However, the authors do not provide evidence that the POP can improve our understanding of future vegetation dynamics or reduce uncertainties. The model is fitted to biomass and stem density data measured under ambient conditions and there is no benchmarking against carbon, water or nutrient fluxes.

Fig. 4: How good is the fit in non-log coordinate system? Fig 4v and vi suggest huge model uncertainty in biomass.

References

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Interactive comment on Biogeosciences Discuss., 11, 2343, 2014.