

Interactive comment on “Variable tree rooting strategies improve tropical productivity and evapotranspiration in a dynamic global vegetation model” by Boris Sakschewski et al.

Anonymous Referee #1

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In this manuscript, the authors extended the LPJmL4.0 dynamic vegetation model to simulate variable rooting depth. Comparisons between different model versions for tropical South America showed that the novel approach improves most of the benchmarks used in the study. Overall, the study is well-conducted and solid. I agree with the authors that including variable rooting depth is important in DGVMs and the results and the presented model approach are therefore relevant for the modeling community. Previous models typically assumed PFT specific fixed rooting depth and/or a fixed soil depth globally.

My main point is, however, that the manuscript reads like a collection of many interest-

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ing results while I was missing more specific aims or questions regarding for example implications of root diversity for the ecology or biogeography of the simulated vegetation types. Currently, the aims of the study are to 'describe an approach' and to 'evaluate its effect on' various model variables.

Specific comments:

L 25: PFT-specific instead of biome-scale?

L 29: realistically simulated

L 41: delete quotation marks for evergreen and deciduous?

L 47-48: Isn't it the other way round? Traits are aggregated to define PFTs and the fractional cover of different PFTs in simulations then defines the biome type?

L 54: reword 'different attempts were carried out', maybe 'different approaches were presented'?

L 54: Schymanski et al. also developed root models (e.g. www.hydrol-earth-syst-sci.net/12/913/2008/). Although these models were not developed explicitly in the DGVM context, they might be relevant for the introduction or the discussion.

L 55-56: 'study ... searched for...' reword, I would think that a study can't search.

L 74: 'allocation strategy' instead of 'direction'?

L 97: in competition with other rooting strategies/plants/PFTs?

L 100: this suggests to me that rooting depth is related to or a function of tree height.

L 105-108: I think that such a general overview is not necessary and could be removed (also elsewhere in the methods section). Further, it only refers to some selected section and it is incomplete.

L 115: simulates instead of employs?

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- L 116: Should it be 'bioenergy functional type'?
- L 122: 'how different tree rooting strategies (implemented in the new scheme) compete'?
- L 131: Does that mean that soil texture is similar for all layers in the soil column?
- L 142: PFT scale instead of biome scale?
- L 144 and elsewhere: is it really the depth which is reached by 95% of the roots or does it rather mean that 95% of the fine root biomass is between the surface and D95_max? Wouldn't 'reached by 95%' mean that 95% are deeper than D95_max?
- L 146: that's a question related to LPJm4.0 and not to the VR version: why were different values for evergreen and deciduous selected if the resulting root profiles are essentially identical. Would model results differ for similar values?
- L 149: why 18m and not 20m or 95% of 20m?
- L 154: a new carbon allocation scheme?
- L 163: 'sapwood ... proportional' which constant was used to describe this relation? And shouldn't root and stem sapwood be identical to be able to transport the same amount of water?
- L 180: 'to derive a functional relation between tree height and rooting depth'?
- L 203: check brackets in Huang et al
- L 233: I suggest to delete 'The new features... direction.' and say 'In LPJm4.0-VR, PFTs can...'
- L 234: 'formally', I assume this should be 'formerly' or 'in previous model versions'.
- L 233-240: An increase of mortality rates from 3% to 7% is quite substantial and more than twice as high. In addition, it is stated that observed rates do not exceed 6% which means that 7% is not in the real world boundaries as stated in L 237 but it overestimates

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this rate by >16%.

- L 239: 'We regard ...' I don't understand this sentence. And what does 'right direction' refer to? I suggest to reword.
- L 243: Why were four different climate inputs used? Most simulation results are shown only for CRU anyway. Which climate variables were taken from the different datasets?
- L 267: Reword to 'caused by the the presence or absence of variable rooting strategies'?
- L 269: Given that model spin-up was conducted for different time periods for different climate input datasets and at 278ppm. Is there a jump in CO2 in the transition between spin-up and transient phase?
- L 280: I understood that replicate simulations were not conducted. I was wondering how robust or deterministic the selection of rooting strategies is? Would you expect substantial differences in the results when conducting replicate model runs?
- L 285: which method was used for re-gridding?
- L 289: check brackets in Brienen
- L 287: I found it difficult to understand the description of the Rammig et al method and I had to go back to the original paper. I suggest to check the paragraph again for clarity.
- L 304: check bar in 'average \bar{x} '
- L 311: I am skeptical when using gridded climate products to simulate local scale EC fluxes, because these products might not capture some local rainfall events (for example) that have strong impacts on the fluxes. Hence, models will fail to simulate the fluxes. I assume that there are there local scale meteorological data available for the flux sites that could be used for running the model or at least for comparing agreement between gridded data products and observation at EC sites.

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L 313: I suggest to state why NEE was only simulated for 3 sites, this information is currently hidden in the figure caption.

L 336: replace 'called' by ':'

L 376: replace 'over' by 'instead of'?

Generally the results section contains some statements or explanations that do not only describe the results but already go beyond and might be more more appropriate for the discussion.

L 387, Fig 5: when looking at this figure, I was wondering if simulated distributions are always unimodal or if there the model can also simulate bi-modal or multi-modal distributions indicating that very distinct rooting strategies can coexist? I also suggest to add to the figure caption which site is wetter and which site is drier.

L 406: Fig 11 (not 9f?)

L 434: why 4m? Is there a reference for this value?

L 445: 'behavior: Whereas..' Full stop or small w in whereas.

L 453: what exactly does 'reversely' mean?

L 377: The text in this paragraph and the figures are mainly about PFTs, not biomes.

L 519: 'uncertainty...is' or 'uncertainties...are'

L 541: I agree that it's important to look at below ground biomass but comparing Fig 15 and Fig 13 suggest that the ratio between aboveground and below ground biomass is extreme in some areas with high aboveground biomass but low below ground biomass (300-400t/ha aboveground vs ca 20t/ha belowground). Are such ratios realistic in these regions and how can this be explained?

L 557: according to figure caption in Fig 12, these are PFTs not biomes.

L 565: 'where' instead of 'were'

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I was surprised that grasses and fire were only shortly mentioned in the discussion, given that the study region also includes seasonal areas with Cerrados and not only evergreen forests. How are these systems represented? Only by deciduous forest or does the model also simulate a grassy component and fire? Fire also has some impacts on biomass in these regions and it has been argued that lateritic layers constrain rooting depth and might thereby influence grass-tree coexistence in these regions.

L 606: the extent of evergreen forest has not been presented, but rather the extent of the evergreen PFT and the deciduous PFT. Further, Fig 12 shows that the extent of the evergreen PFT is very similar in the original and the VR version (although the FPC is much lower in the original version). I suggest to clarify or to classify vegetation into biomes based on the FPC of different PFTs. This would allow comparisons of biome cover in different scenarios.

L 627: 'Expansion' instead of 'Extent'?

L 638: PFT instead of biome types?

In the discussion I was missing some discussion of the results in the context of previous modeling studies, such as the studies cited in the introduction.

As the study region als includes the Cerrados, the rooting niche separation ideas that explain grass-tree coexistence in savannas might be relevant for the discussion, e.g. Van Langevelde et al 2013 Ecology.

L 1083: Figure S3 not provided.

Just out of interest, can the model easily be adapted to global scale, and will these model developments be included in the global 'default' version of LPJmL4.0? Or would this lead to computational constraints?

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