Response to the comments of referee 1 on Sakschewski et al. 2020: Variable tree rooting strategies improve tropical productivity and evapotranspiration in a dynamic global vegetation model

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 bench-marks 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 interesting 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.

Dear referee,

Thank you for your positive evaluation and many constructive comments on our manuscript. We agree that the paper benefits from focusing on a specific research question which creates a stronger storyline. As we will now highlighted in the introduction in line XX we now focus on the research question "What is the role of diverse tree rooting strategies for productivity and evapotranspiration in tropical South America?" and structure the manuscript accordingly. In connection to comments of referee 2 we will shorten the manuscript and move parts of the results to the supplement. This also helps to create a stronger and clearer storyline.

L 25: PFT-specific instead of biome-scale?
Thank you for this comment. In the abstract we wanted to avoid specific terms like "PFT" to keep it easy to understand for readers which are not familiar with those terms, but still convey the main message. Therefore we now changed the phrase into "often condense this variety of tree rooting strategies into averages of tree growth startegies from the ecosystem- to biome-scale" in line XX.

L 29: realistically simulated
Thank you for this suggestion. We changed it accordingly, now in line XX.

L 41: delete quotation marks for evergreen and deciduous?
Thank you for this suggestion. We changed it accordingly, now in line XX.

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?
Thank you for raising this point. We apologize for the sloppy usage of "biome" and "PFT". To clarify the text we now state "In general these models condense the diversity of such functional tree traits into so called plant functional types (PFTs) which represent average tree individual on scales as large as biomes" in line XX.

L 54: reword 'different attempts were carried out', maybe 'different approaches were presented'?
Thank you for this suggestion. We changed it accordingly, now in line XX.

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 theDGVM context, they might be relevant for the introduction or the discussion.
Thank you for this suggestion. We will incorporate this important study in the suggested sections.

L 55-56: 'study ... searched for...' reword, I would think that a study can's search.
Thank you for this correction. We changed the sentence to "In a pioneering study more than 20 years ago, Kleidon and Heimann (1998) systematically searched for rooting strategies which yield highest net primary productivity ..." in line XX.

L 74: 'allocation strategy' instead of 'direction'?
Thank you for this suggestion. We changed it accordingly, now in line XX.

L 97: in competition with other rooting strategies/plants/PFTs?
Thank you for this question. We changed the respective sentence to "all tree rooting strategies of this spectrum grow in competition..." in line XX as it is meant to contrast this study with some of the former approaches. A more detailed sentence would lead to a full description of the competition scheme which can be found in the methods now in line XX formerly in line 208-221.

L 100: this suggests to me that rooting depth is related to or a function of tree height.
Thank you for this comment. Yes as we clarified in the methods we used this approach. Formerly in line 168-206. Now line XX.

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.
We agree with the referee and deleted this part.

L 115: simulates instead of employs?
Thank you for this remark. We changed it accordingly, now in line XX.

L 116: Should it be 'bioenergy functional type'?
Thank you for pointing out this mistake. We inserted "functional" now in line XX.

L 122: 'how different tree rooting strategies (implemented in the new scheme) compete'?
Thank you for this suggestion. We implemented it, now in line XX.

L 131: Does that mean that soil texture is similar for all layers in the soil column?
Thank you for this question. Yes. In LPJmL4.0 each grid cell has one soil texture information for its 3 m soil column only. We followed this approach for our larger soil columns as well. In fact a high resolution soil texture information in 3 dimensions for the study region is so far not available, or only partly available. We wanted to keep things simple and comprehensible also with regard to comparing results of LPJmL4.0-VR to LPJmL4.0. We now better clarify the approach in line XX stating "Equal to LPJmL4.0, we apply a single grid cell specific soil texture information to the whole soil column as in (Schaphoff et al., 2018)".

L 142: PFT scale instead of biome scale?
Thank you for this suggestion. We changed it as suggested, now line XX.

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?

Thank you very much for pointing out that out. We changed the sentence to "…we here calculate the depth at which the cumulated fine root biomass from the soil surface downwards makes up 95% of all fine root biomass", now in line XX.

L 146: that's a question related to LPJml4.0 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?
Thank you for this question. The parameter values are average values for those 2 PFTs found in Jackson et al. (1996). At model development of LPJmL those values were the best available. We now specifically mention where those parameter values came from in line XX.

For now it is clear that increasing the difference between the beta values of the 2 PFTs would most probably enhance model performance. Even with only 2 PFTs present. Here the best way to go would probably be to increase the beta value of the evergreen PFT to reach a D95_max of maybe 2-4 m, which would buffer against some dry season signal. Regarding the second question: If regarding beta values only, the answer is no, there would be no difference between the evergreen and deciduous PFT. Fortunately, they also differ in other functional traits such as specific leaf are (SLA) or leaf longevity (LL), determining their phenological strategy and therefore their performance under different climate regimes.

L 149: why 18m and not 20m or 95% of 20m?
Thank you very much for this question. As we have chosen a maximum soil depth of 20m we wanted to avoid a significant accumulation of fine roots in the last soil layers. A D95_max of 20m in a soil column of 20 m would mean that the additional 5% of fine roots are also distributed between 19-20m. In principle it was a choice between a round number of soil depth or a round number at the largest value of D95_max. In the current model version of LPJmL4.0-VR a D95_max of 20m is not missing in the study area as distributions of D95_max always flatten towards the 18m bin in grid cells where deep roots dominate. Nevertheless, it is possible to substantially increase soil depth and create more sub-PFTs with larger D95_max values. This step might even be necessary in future versions of the model or other study areas. We now explain why we chose the largest D95_max at 18m in line XX stating "We chose 18m as the largest D95_max value in order to avoid that roots of the respective sub-PFT significantly exceed the maximum soil depth of 20m (see also 2.2.4 and Fig. 2 right panel)."

L 154: a new carbon allocation scheme?
Thank you for this suggestion. We changed it accordingly, now in line XX.

L 163: 'sapwood ... proportional' which constant was used to describe this relation? And shouldn't root and stem sapwood are be identical to be able to transport the same amount of water?
Thank you for pointing this out. Our explanation was misleading. We do not use a constant here. The term proportional is falsely used. It is as your second question suggests and as it is written in the description of Fig. 2. We now clarify the approach in the methods section in line XX with "Root sapwood cross-sectional area in the first soil layer is equal to stem sapwood cross-sectional area, as all water must be transported through the root sapwood within this soil layer. In the following soil layers downwards, root cross-sectional area decreases by the relative amount of fine roots in all soil layers above (Fig. 2)."

L 180: 'to derive a functional relation between tree height and rooting depth'?
Thank you for this suggestion. We changed it accordingly, now in line XX.

L 203: check brackets in Huang et al
Thank you for pointing this out. We changed the brackets, now in line XX.

L 233: I suggest to delete 'The new features... direction.' and say 'In LPJmL4.0-VR, PFTs can...'
Thank you for this suggestion. We changed it accordingly, now in line XX.

L 234: 'formally', I assume this should be 'formerly' or 'in previous model versions'.
Thank you very much. We changed it accordingly, now in line XX.

L 233-240: An increase of mortality rates from 3% to 7% is quite substantial and more then 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 this rate by >16%.
Thank you very much for raising this point. We agree that claiming 7% is in the boundary of 6% is overstated. The point we wanted to make was mixed up and lost by citing the study mentioning a observed maximum of 6% background mortality rate in 167 Amazon forest plots (Johnson et al., 2016) and comparing it to the maximum (not actual background) mortality of LPJmL4.0-VR. In fact observed background mortality rate and conceivable maximum mortality rate are not the same thing. The growth efficiency related mortality which is used in many DGVMs needs a maximum mortality rate, because otherwise the simulated mortality rate would rise to 100% when total respiration exceeds productivity. In the real world though plants would optimize and reallocate carbon pools under those circumstances as far as they can. Nevertheless, a mortality rate of 100% in the real world is conceivable as well. The maximum mortality rate of a forest under a hypothetical super extreme scenario (no rain for 12 month) could of course be 100%.
Accordingly, in a hypothetical model which truly captures all important mechanisms of tree survival and mortality, the maximum mortality rate could be set close to 100% as we tried to explain formerly in line 239-240. In other words this constant could eventually be obsolete. The road to such a model will be long and might never be ending, but until then, a maximum mortality rate at any value will always be necessary to achieve biomass values in acceptable ranges. We now deleted the reference of observed mortality rates of 6% in the manuscript as this number is not comparable to the maximum mortality rate set in the model. Moreover, we will describe the true purpose of a maximum mortality rate and why we changed it (as described in this answer) now in line XX.

L 239: 'We regard ...' I don't understand this sentence. And what does 'right direction'refer to? I suggest to reword.
We deleted this sentence in accordance with our response above.

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?
Thank you very much for those questions. We wanted to show that our results remain robust using various climate inputs and decided (also with regard to the amount of figures and respective manuscript text) to show this with regional rooting depth (Fig. 6) and regional evapotranspiration (corridors in Fig. 11). In connection to referee 2 requesting a substantial reduction of the manuscript size and amount of figures, it seems impracticable to produce all figures for all climate inputs. We also argue that, Fig. 6 & 11 show some proof that results for different climate inputs are very similar. There are local differences, but the big picture remains the same. As we do not want to go into detail regarding local scale differences we hope the current amount of results is enough.
We will add a sentence of clarification in the discussion in line XX.
We now clarify what climate variables were taken from the different data sets in the respective methods section in line XX-YY.

L 267: Reword to 'caused by the the presence or absence of variable rooting strategies'?
Thank you very much for this suggestion. We changed it accordingly, now in line XX.

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?

Thank you very much for this question. No there is a smooth transition between spin-ups and transient simulations regarding atmospheric CO2 content. Before the year 1840 a constant value of 278ppm is used, while after this year values are rising according to a LPJmL protocol first introduced in Sitch et al. (2003) and eventually following the Mona Loa record of Tans and Keeling (2015). We now clarify this approach in the methods section in line XX.

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?

Thank you very much for this question. Given the vast amount of different model test runs during model development, we can assure that results are very robust. We admit we do not fully proof this in this study. However, the 4 very similar rooting depths maps (Fig. 6) provide some proof.
Since there are no real stochastic processes in LPJmL which could lead to a path dependency of vegetation dynamics, this model behavior was expected. Therefore, it is also a standard procedure to not conduct any replicate simulations when using LPJmL. We now clarify this point in the methods section in line XX.

L 285: which method was used for re-gridding?

Thank you for pointing out this lack of information. We now name the methods, software packages and underlying studies in line XX. Regridding was conducted in R (R Core Team, 2019) using the "aggregate" function of the R-package "raster" (Hijmans, 2019), which aggregates to lower resolutions by taking the arithmetic mean excluding NAs.

L 289: check brackets in Brienen

Thank you for pointing that out. We corrected the brackets, now in line XX.

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.

We will provide a more sophisticated explanation of the respective methods section now in line XX-YY.

L 304: check bar in 'average x_bar'

Thank you for pointing out this mistake. We corrected the bar, now in line XX.

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.

We fully agree that using gridded climate products is not optimal to reproduce locally measured ET fluxes, because they can lack information of local weather events. We solely use gridded data in this study due to several reasons: 1) We want to stay consistent with the regional results of this study. As we e.g. show a simulated regional rooting depth maps (Fig. 6) and plots of underlying local tree rooting strategies (Fig. 5) as well as regional ET (Fig. 11) all based on gridded climate data, changing the climate input for simulations at local scale seemed inconsistent. 2) Even though we apply statistical metrics to compare model vs. flux agreement, our focus was not on the effects of local

weather events on ET, rather on the general climate signal, most importantly the presence or absence of a dry season and the effects on simulated rooting depth and ET and the differences between the different model versions. We fully agree that forcing the model with local climate data could in principle enhance model performance, but we never aimed for a perfect match of ET and NEE at all sites. 3) There is meteorological data available for different flux sites, but these data sets are often cluttered with gaps and are only available for a few up to 10 years only. Moreover, each site has its own limitations when it comes to model implementation. Taken together, this creates quite some problems for DGVM simulations. The LPJmL model needs continues climate data and long time series. Jumping from a spin-up simulation into repeating 5-10 very similar years can cause artifacts which should be avoided. We would be happy simulate rainforest sites with real site meteorological data, but for this we would need longer time spans than currently available. There are approaches trying to solve those problems, but they are currently beyond the scope of this study. 4) Given that we compare monthly means of simulated and measured ET at the local scale and that simulation results appear to capture seasonal signals, we are convinced that our approach is sufficient to deliver the message of this manuscript.
We will insert a clarification of why we used gridded climate input data only in the discussion in line XX.

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

Thank you very much for this remark. Unfortunately, in the data sets accessible to us, continues NEE data covering at least 2 years was only available for 3 sites. We now clarify why we compare NEE only at 3 sites in the methods sections in line XX and in the respective figure caption.

L 336: replace 'called' by ':'

Thank you very much. We changed it accordingly, now line XX.

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

Thank you for pointing that out. We changed it accordingly, now line XX.

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.

We will check the results section and will transfer all potential interpretations into 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.

Thank you for this comment. Indeed these distributions can be bimodal, indicating that very distinct rooting strategies can co-exist. We have not systematically checked all grid cells, but a clear tri-modal distribution was not observed so far. This might also need different ways of detecting them as multiple modes might be hidden in a continuous distribution. So far we observed 2 cases where distributions can clearly be bi-modal. 1) In areas with dominant evergreen tree cover and a "medium" dry season, where shallow and deep rooting evergreen sub-PFTs can co-exist. 2) Areas with a substantial dry season where the evergreen and deciduous PFT co-exist. Here the deciduous PFT shows shallower roots and the evergreen PFT deeper roots. Multi-modal distributions are highly

connected to the topic of niche segregation and has many ecological implications, which we want to avoid in this study. With regard to the comment of referee 2 to significantly shorten the manuscript, we regard this topic as beyond the scope of this study. It will definitively be in the focus of future studies.

We will indicate the drier and wetter site directly in Fig. 5 as suggested.

L 406: Fig 11 (not 9f?)

Thank you for pointing out this mistake. We changed the text accordingly, now in line XX.

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

This short paragraph is a rough qualitative description of our simulation results of mean rooting depth in relation to climate variables. It is not a description of results from other studies. We now clarify this by inserting the word "simulation" in line XX.

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

Thank you very much for this suggestion. We exchanged ':' with '.' As suggested, now in line XX.

L 453: what exactly does 'reversely' mean?

Thank you very much for pointing out this unclear formulation. We reformulated this paragraph into "At STM K77 (Fig. 9f) local circumstances show the influence of variable rooting strategies on ET in a different way. This former rainforest site was converted to pasture before Eddy covariance measurements began. This local land-use at STM K77 is not representative for the respective 0.5° grid cell, and all 3 LPJmL model versions simulate dominant natural forest instead of pasture." now in line XX.

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

Thank you for pointing that out. We changed the heading of this section into "Distribution of plant functional types" now in line XX.

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

Thank you for pointing that out. We decided to change it into "uncertainties are" now in line XX.

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?

Thank you for this question. In a recent review Fearnside (2018) found that information on belowground biomass of trees in the Amazon region is still very sparse. Available empirical data for 3 sites showed a range of about 15.2 – 33.4 % and a mean of 23.7 % of tree biomass below-ground. No data seems to be available for the western Amazon. Especially in this area we simulate high AGB and low BGB. Here a ratio of 20/(350+20) yields 5.4% of total biomass below-ground. While we are not in the position to validate these values we agree that they might be too low. According to our approach, more root biomass might not be necessary for water uptake and conduction in those

regions, but more root biomass might very well be necessary for the statics of trees. Structures ensuring a tree's stability are neglected by the LPJmL model. The implementation of tree statics would most likely increase the belowground biomass in regions which currently show a very low percentage of total biomass allocated to roots. Nevertheless those structures would be necessary for all sub-PFTs and the overall results presented in our manuscript would most likely not change significantly. We now critically discuss our findings of belowground biomass in this context in line XX-YY.

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

Thank you for pointing that out. We changed the wording to "PFT distribution", now in line XX.

L 565: 'where' instead of 'were'

Thank you for pointing out this mistake. We changed the word accordingly, now in line XX.

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.

Thank you for this comment. We agree that fire is an important driver forming the vegetation distribution especially outside tropical evergreen forests. For the current version of LPJmL4.0-VR we used the most simplistic fire module available in LPJmL (GlobFirm, Thonicke et al., 2001), which calculates a fire return interval and burned area based on litter moisture only. A PFT dependent parameter for the fraction of killed individuals then determines the burned biomass. Fire-vegetation-feedbacks are therefore existent, but very simplistic. Future studies will incorporate LPJmL's recently updated and much more complex SPITFIRE module (Drüke et al., 2019) and enable to investigate those fire-vegetation feedbacks in a more comprehensive way.

Even though C3 and C4 grasses are explicitly simulated as PFTs in LPJmL4.0, LPJmL4.0 and therefore also LPJmL4.0-VR currently underrepresents the occurrence of grass. This is mainly due to the fact that grass PFTs compete with tree PFTs for area. In that way grass abundance is often highly underestimated when tree PFTs are present. This in turn has the effect that grass-fire feedbacks which naturally stabilize grasslands by reducing tree cover, are not simulated as desired even with a better fire module. Current ongoing developments of LPJmL aim at allowing grass PFTs to grow under any tree PFT canopy. Here, grass PFTs would mainly be affected by the light reduction of trees. In that way grass fire feedbacks could transform areas currently dominated by deciduous forest to become savanna-like vegetation types. We now show grass PFT FPC in a new supplementary figure (Fig. SX). With regard to shortening the paper as suggested by referee 2 and our new research question we briefly discuss the implications of low grass PFT cover for our results in line XX-YY.

With regard to 1) the simplistic fire module used, 2) the underrepresentation of the grass PFTs, 3) our new research question, 4) the comment of referee 2 to shorten the manuscript, and 5) that we do not assess the distribution or stability of potential natural vegetation (i.e. without land-use), we regard the topic of grass-tree coexistence as beyond the scope of this study. Nevertheless, we will

discuss the aforementioned shortcomings of LPJmL4.0 and their implications in the discussion now in line XX.

L 606: the extent of evergreen forest has not been presented, but rather the extent of the evergreen PFT and the deciduous PFT.

Thank you for pointing that out. We now changed the wording "biome" into "PFT" throughout the text.

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).

Thank you for this remark. We may have missed explaining this result in the manuscript. Fig. 12 g) and h) show that the standard LPJmL4.0 model simulates a rather similar dominance of the evergreen and the deciduous tree PFT in the Amazon region (an almost 50/50 dominance of both PFTs in this region). This model behavior can be explained by the fact that LPJmL4.0 is not capable of simulating a true competitive exclusion over time. PFT establishment rates are not coupled to PFT performance and are in fact equal for all PFTs for every time step (even though the overall establishment rate can vary, but for all tree PFTs in the same way). In Fig. 12 e) and f) we see clear dominance patterns of the evergreen and deciduous tree PFT even though they vastly deviate from the evaluation data (a-b) as well. The underlying model LPJmL4.0-VR-base (just as LPJmL4.0-VR) does simulate a performance dependent PFT establishment as described in the methods section formerly in line 214-228. Therefore, the dominance pattern of evergreen and deciduous tree PFTs in Fig. 12e) and f) can be explained by competitive exclusion. A similar pattern would also be expected in standard LPJmL4.0 when a performance dependent PFT establishment would be implemented. The reason why standard LPJmL4.0's evergreen PFT (Fig. 12g) (and also the deciduous PFT(Fig. 12h)) show a similar extent towards the Southern and Eastern border of the Amazon region compared to the extent of the evergreen PFT in LPJmL4.0-VR (Fig. 12c) is simply because of the human land-use of 2001-2010 applied to all model versions used in our study. The similar extent marks the border of the arc of deforestation in this region. We will clarify these coherences in the main manuscript or the supplement depending on our decision of whether Fig. 12 remains in the main manuscript.

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.

Thank you very much for this suggestion. While we regard some of the PFTs used in LPJmL as representatives of biomes, (e.g. the "tropical broadleaved evergreen tree", as the representative of the biome "tropical rainforest") we agree not to use the word "biome" in the manuscript. When it comes to accounting for what factors actually determine a biome the scientific community follows different definitions. Therefore, we also want to avoid classifying simulated vegetation in yet a new way as suggested. Moreover any classification would lead to a loss of information regarding the results of simulated geographical PFT distribution, especially for those familiar with PFTs and DGVMs. We rather follow the other suggestion and now better clarify our results now in line XX-YY and avoid the word "biome" throughout the text.

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

Thank you very much. We changed it accordingly, now in line XX.

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 also 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.

Thank you very much for pointing out that missing connection. We will discuss our results and studies mentioned in the introduction in the context of our new research question. As mentioned above in our answer to the comment regarding L565, grass-tree coexistence is hardly possible to realistically simulate with the current status of the LPJmL model. Yet we will discuss the rooting niche separation hypothesis in the discussion as suggested. With respect to referee 2 requesting a significant shortening of the manuscript we see ourselves forced to keep this discussion rather short.

L 1083: Figure S3 not provided.

We are very sorry for this mistake. We now provide Fig. S3.

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?

Thank you for your interest. First tests of global scale simulations look very promising, but of course have to be evaluated in detail. The principles found in this study seem to apply to other regions as well. The model runs stable on a global scale and currently needs about 2-4 times longer when including 10 sub-PFTs for all 8 natural PFTs in LPJmL.

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