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Discussions

# *Interactive comment on* "The importance of tree demography and root water uptake for modelling the carbon and water cycles of Amazonia" *by* Emilie Joetzjer et al.

**Anonymous Referee #2**

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The manuscript by Joetzjer et al presents a modeling study that makes a few relatively small changes to ORCHIDEE-CAN (CAN) and evaluates these changes against site-level and regional gridded data from the Amazon. The two (as far as I can tell) changes made to CAN are a recruitment function and a function that allows flexibility in root water uptake. Specifically, root-zone matric potential is calculated in CAN using a weighted mean where weights are based on root biomass in a soil layer, while the modifications (labeled CAN-RS) base the weights on the maximum amount of water that can be absorbed in a soil layer. Model evaluation of biomass accumulation following biomass removal shows that CAN and CAN-RS simulate improved above-ground biomass vs age since disturbance. Site-level daily and monthly fluxes (NEE, GPP, LE)



were simulated equally by the original model version (labeled TRUNK) and CAN-RS. CAN-RS primarily improves upon failures in CAN compared with TRUNK. TRUNK and CAN-RS held similar biases in simulating regional fluxes, albeit TRRUNk was lightly less biased at simulating ET while CAN-RS was perhapos slightly better at simulating spatial variability in mean annual GPP.

Overall this paper seems like a fairly small, incremental development to ORCHIDEE with little gain in model skill compared with TRUNK. The addition of the recruitment model and the change to root water uptake seem unrelated. While nothing is really incorrect methodologically, it's quite a long paper to wade through and I'm left wondering what has been learned. I disagree that the claim in the title: "importance of tree demography and root water uptake for modelling the carbon and water cycle of the Amazon" has been demonstrated with the current set of evaluations presented in the manuscript. Furthermore the paper lacks clear objectives and a number of the conclusions have not been demonstrated by the study. The study would benefit from a clear set of objectives (usually laid out in the final paragraph of the introduction). The study conclusions then need to be tied to these objectives. The conclusions on ln 21 pp 22 to ln 4 pp 23 are not based on the results of the study but are more "future directions" that come with very little background. These would be better placed in the discussion, with more surrounding discussion that gives a justification for the future work, or deleted.

To improve the chance of this study being cited I suggest focusing in on the few defined objectives, explain the model differences, and do a better job of demonstrating the improvements. The improvement in above-ground biomass simulation following disturbance is clear, though both CAN and CAN-RS are equivalent. But it is unclear to me what has caused this improvement. Is it that tree mortality is better simulated by CAN, or is it that carbon allocation to wood is better simulated? An analysis that can parse these two possible processes would be good. With regards to the improvement in the "seasonality of GPP and LE" (ln 18 pp 20), I'm not convinced. Do the authors mean CAN-RS compared with CAN or TRUNK? This improvement needs to be demonstrated clearly.

Nutrients were alluded to in the discussion. ORCHIDEE has a version with nutrients enabled (Goll et al. 2017), why was that version not used for the current developments? Perhaps more to the point, the Amazon specific soil property maps developed by Marthews et al. (2014) were mentioned but were not used in this study. Why not? Tropical clay soils have very different hydraulic properties compared with temperate clay soils and the use of the USDA soil classification system seems like the wrong choice for this study.

Framing ORCHIDEE-CAN as a second generation DGVM is not entirely correct. All of the models described by Fisher et al. (2018) represent vertical competition for light allowing PFTs to compete. Each cohort has separate GPP and NPP. If I understand correctly, CAN and CAN-RS does not allow PFTs to compete, cohorts of a PFT share the same GPP, and parameterization is with map of PFT or species' distributions, which is not a property of a second generation DGVM.

In summary: Define a clear set of objectives A better link needs to be made between the root water uptake modification and the recruitment model modification/addition. The model evaluation ought to be in the context of the objectives with a clear explanation of why the various instances of the model differ (e.g. ABG$\sim$year after disturbance, is it mortality or allocation?). A better case needs to be made for the improvements gained from the root water uptake modification. This is an interesting modification, I'm surprised that it doesn't perform better than TRUNK in most cases. Why not? Unless there is solid justification why Marthews soil properties were not used in this study, Marthews should be used. A more focused discussion and conclusion centered around the objectives is needed. I think the manuscript would benefit from an attempt at editing and consolidation to reduce the length and number of figures.

Minor comments:

ln 31 pp 1, the model reproducing fluxes as well as the original model is not really an



advance

ln 35-36, pp 1 this claim is not supported by the data in the paper

ln 2 pp 2, the amazon is already experiencing longer dry seasons in some places

ln 28 pp 2, suggest changing "density of root tissue" to "density of roots"

ln 5 pp 3, This study does not explore "relative contributions of . . . ," that would require a variance decomposition or similar methodologically

ln 30 pp 3, APAR does not decrease exponentially with LAI, rephrase

ln 4 pp 4, If I understand Naudts et al. (2015) the canopy structure is a statistical representation of a 3D canopy, not an explicit one. This distinction should be made clear

ln 10 pp 4, what does "this" refer to

ln 23 pp 4, Are these parameter values from Naudts et al. (2015), or were the changed based on Briennen for this study? I think more generally a table that makes clear the key differences between TRUNK, CAN, and CAN-RS would be useful. This could also include a brief description of processes important to interpreting the results, e.g. how LAI is calculated.

ln 24 pp 4, Could you add a brief description about how the number of individual (N) is simulated? Presumably if N exceeds Nmax then N is reduced to Nmax and this is self thinning. Are there other mortality processes? How does TRUNK simulate mortality?

ln 1-2 pp 5, It doesn't? How are LAI and canopy gaps related in CAN?

ln 20 pp 5, why don't you use the original notation from van Genuchten? It's helpful to use consistent notation.

ln 8 pp 6, while I understand the need to distinguish the different instances of the model I wouldn't describe the minor changes here as different versions of the model.

ln 25 pp 7, Where are these met data from? Can you cite a source or dataset?

Table 1 pp8, why was spin-up $CO_2$ set to 370 ppm, this seems like a strange equilibrium condition.

Table 1 pp8, Am I right in thinking M34 is also described in the literature as K34? This site already has several names, I think it might be better to stick with K34.

ln 5 pp 11, suggest rephrasing "equal correlation extends ..."

Figure 3 pp 11, Why not also have Taylor plots of monthly data?

Figure 4 & 5 I don't find the diel cycles per month that useful or easy to read. I suggest focusing in on just a few key ones that demonstrate the model differences. Can uncertainty be added to the observations?

ln 3-4 pp 17, Can you be sure of this statement, how do you know?

Figure 10, Bias seems worst in CAN-RS

Figure 14, Why is TRUNK not shown?

ln 18-20 pp 19, How are kcmaint and CUE related, from the description it seems CUE = 1 – kcmaint.

ln 20 pp 22, TRUNK doesn't model diameter size distribution

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