

Response to Reviewer 1

General Comments

I thank the authors for their revision of the manuscript, which satisfactorily address most of the comments. I find the paper clearer and even more enjoyable to read.

There are, however, a few minor points that still need clarification, as detailed below. After this, I consider this paper as fit for publication in HESS.

Response to Reviewer 1 General Comments

The authors thank the reviewer for their perceptive and helpful comments throughout the revision. The comments have greatly aided in improving the clarity and demonstrating the significance of the study.

Specific Comments

R1C1: L266-267: Contrary to what the authors state in the revised manuscript, and in their response to R1C16, the rooting depth in Ech2O is not set equal by default to total domain depth, at least in the root water uptake calculation one can find on http://bitbucket.igb-berlin.de:7990/users/ech2o/repos/ech2o_iso/browse/Hydro/SolveCanopyFluxes.cpp ; there, rooting depth it is the exact depth at which the cumulative fraction (from the surface) reaches 95%, see l. 248.

Response to R1C1: As the reviewer has shown, Ech2O calculates a rooting depth of 95% of the roots. However, this 95% is still based on using the whole domain for estimating total root growth. The relatively small impact of total rooting depth on the root fraction in different soil layers results in further negligible differences between rooting depth and total model domain for water use allocation. Lastly, for the calculations used in this manuscript (and inconsistency with the *kroot* distribution in Ech2O) it is most accurate for L266-267 to describe the use of D as the total soil depth as that is what was used.

R1C2: L329-332: Maybe mention that “LAI calibration” refers to calibration Step 2 in the next paragraph.

Response to R1C2: The authors have included a reference to calibration step 2 for the LAI calibration,

R1C3: L341-347: I thank the authors for reworking this section, yet some aspects still need clarification: First, it is still not clear to me how the information from Step 1 was used in Step 2; were the 100,000 latin hypercube samples in Step 2 based on the parameter range from Step 1? Some other connection? ◦ It is quite surprising that in Step 2 none of the vegetation parameter related to allocation and growth were calibrated, contrary to earlier work by the same research group with the same model (Douinot et al., 2019). One expect that some degree of sensitivity for leaf area index and basal area simulations to these parameters. Can the authors justify this choice, perhaps using some sensitivity analysis or providing known literature values? ◦ Which “temporal dynamics of LAI” were used as inputs in Step 1? The same of as the calibration datasets of Step 2? ◦ How were the “infeasible parameter combination” assessed?

Response to R1C3: We have now clarified the calibration. Parameter sets (complete sets) were randomly resampled and merged with Latin hypercube sampling of vegetation biomass parameters. In doing so, soil parameter sets were not affected. We mistakenly left out the biomass parameters from the included

vegetation parameters in the previous revision. They have been included in the revised supplementary material. The authors have further clarified that MODIS LAI dynamics were used as inputs for calibration Step 1, and how infeasible parameter combinations were evaluated. (L341-347).

R1C4: L426: Puzzled by this somewhat strong allocation rule, I went on checking the grass allocation calculation in the available Ech2O-iso code (http://bitbucket.igb-berlin.de:7990/users/ech2o/repos/ech2o_iso/browse/Forest/GrowGrass.cpp) and the paper it draws from (Lozano-Parra et al., 2014). There, I could not find an obvious reason why the biomass allocation (total and root/leaf fractions) should be calculated as constant. Unless the LAI and/or NPP was kept constant in the simulations? Could the authors bring some precisions?

Response to R1C4: We thank the reviewer for identifying this error. Model output of the grass allocation was erroneously output as a single value, the authors have revised the statement and updated the table to reflect the seasonal cycle.

R1C5: Eq. (1): root proportions should all be in lower capitals.

Response to R1C5: Revised (Eq1).

R1C6: L357: "m3 m-3" instead of "m2/m2"

Response to R1C6: Revised (L357)

R1C7: Table S1: I am guessing the "aspect ratio" parameter the "root aspect ratio"? Please clarify.

Response to R1C6: Revised

Response to Matthias Beyer

General Comments

I thank the authors for providing a comprehensive and careful revised version of the manuscript that addresses many of the comments from version 1. I will first comment on the general points and then provide a few more comments that might help. Having that said, I recommend the manuscript for publication in HESS after addressing those very minor comments.

Response to General Comments

The authors thank Matthias for his insightful comments as these are important merit for future modelling in ecohydrology. The comments throughout the revision have been helpful in clarifying the manuscript and expanding the wider reach of the study.

Specific Comments

R2C1: Abstract, 1.17-18: I think a core strength of the model is to provide E/T separations for different root plant types/ or plots (and that in a high temporal resolution). I think it would be interesting here to have the (overall) quantitative numbers for E/T or ET for willow and grass at the study site.

Response to R2C1: The authors have added some of the ET partitions to the abstract to improve the context of water use at the site. (L17-18).

R2C2: L. 21: perhaps ‘root water uptake’ (and subsequently simply RWU) instead of ‘root-uptake’? I think the former is more commonly used, but it is up to the authors to decide

Response to R2C2: Revised (L21).

R2C3: Introduction: I very much like the objectives as per R1

Response to R2C3: Thank you.

R2C4: L.165: ‘unrealistically enriched isotopic compositions’ – this is interesting. Is this known to be the case when wounding occurs? (this is a question out of interest, because I do not know)

Response to R2C4: In our case the sudden marked enrichment and dissipation of enrichment ($\gg 20$ per mille above long term values) was coincided with the observed wounding and healing of the tree following installation. This may not be as severe for all installations or in all trees but needs further evaluation/exploration.

R2C5: L.224/225: (suggestion to add) and no fractionation during root water uptake?

Response to R2C5: Revised (L224)

R2C6: L.280: ‘estimate xylem $\delta^2\text{H}$ ’ – is this a typo, or do the authors mean only $\delta^2\text{H}$ and not $\delta^{18}\text{O}$ is estimated? → ‘estimated xylem isotope values’?

Response to R2C6: Revised (L280)

R2C7: L.345: I honestly do not know what ‘100,000 Latin Hypercube Sample parameter sets’ are. But I am not an everyday modeler as well. I still believe that a very brief explanation would benefit the non-modeler reader.

Response to R2C7: The authors have revised the statement to more clearly describe the parameter generation. The revision more clearly indicates that Latin Hypercube sampling is a method to acquire the parameter sets.

R2C8: L.357: typo: the unit of soil moisture content should be m^3/m^3

Response to R2C8: Revised (L357)

R2C9: L.366-367: ‘soil water isotopes’ should read $\delta^2\text{H}$, I think? Because in table S2 I only see $\delta^2\text{H}$

Response to R2C9: The authors have revised to indicate that the MAE presented is for $\delta^2\text{H}$. (L366)

R2C10: L.679: replace ‘deuterium’ by ‘ $\delta^2\text{H}$ values’?

Response to R2C10: Revised. (L679)