

Biogeosciences Discuss., author comment AC2
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Reply on RC1

Aaron Smith et al.

Author comment on "Modelling temporal variability of in situ soil water and vegetation isotopes reveals ecohydrological couplings in a riparian willow plot" by Aaron Smith et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-278-AC2>, 2022

The authors thank the reviewer for their constructive comments, which the authors will incorporate into the manuscript during revision. In particular, the authors will work to clarify the modification made to ECH2O regarding plant water availability, as described in the initial response to Reviewer 1 posted on-line. In reply to RC2, the lateral contributions account for water amount, age, and signature. Regarding the KGE and AIC of distance-based mixing using measured data, the authors did not intend to suggest that there was a "head start" by either the instantaneous or distance-based mixing approach. Rather, that the model (and root distributions and water uptake) had been optimized for the simulated soil isotopes, where using simulated soil isotopes the distance-based mixing showed higher performance. While a small sample size, this could suggest that if the model were able to fully reproduce the measured soil isotopes with accompanying root update the KGE of the distance-based approach would be consistent with the AIC. The authors will additionally revise figures, clarify the calibration method as well as the interpretation of the distance-based mixing approach. Further expansion of the discussion section to incorporate suggestions of research direction as well as limitations of the presented approach.

Specific Comments: Reviewer 1

R1C1: L31: The 80-90% T/ET estimate by Jasechko et al. (2013) is often thought to be overestimated ; maybe the "updated" estimate Schlesinger & Jasechko (2014) would be more appropriate for citation.

Response to R1C1: The authors will revise the reference used.

R1C2: L36: Please considering citing the original, peer-reviewed publication by Zink et al. (2017)

Response to R1C2: The authors will revise the reference from UFZ (2021) to Zink et al.

(2017).

R1C3: L36-37: I am not sure what is meant by "beyond vegetation uptake during the growing season", please rephrase.

Response to R1C3: The authors will revise this statement.

R1C4: L43: Rather than "small or larger scales", please consider providing indicative scale (e.g. plot to stand).

Response to R1C4: The authors will revise "small or large scales" to "plot or stand."

R1C5: L65: Appropriate citations of ecohydrological modelling advances may also include Maneta et al. (2013) and Fatichi et al. (2012).

Response to R1C5: The authors will add these references.

R1C6: L81-82: The stated achievements are rather general; additionally it would preferable to have this section turned this into research questions and/or testable hypotheses (it is not clear to me what these are), to further detail the general goal described L79-80. In this process, rather than "exploring" achievements/question #2 should better state the adopted strategy regarding root-mixing development and its evaluation/rejection (see General Comments)

Response to R1C6: The authors will revise the objectives to reduce the generality and improve the clarity of the overall objectives.

R1C7: Fig. 1: In connexion with the General Comments regarding the rooting system, it would be welcome to have a visual description of the land patches neighbouring the study plots (e.g. in Fig 1b or c, as in Fig. 1c in Landgraf et al., 2021), since the main text (L90-91) only describes what is at least 20m away from the plots.

Response to R1C7: The authors will revise Fig 1 to show the surrounding landuse patches as shown in Landgraf et al. (2021).

R1C8: L117: Did the author mean "Köppen Index Cfb"?

Response to R1C8: The authors will add Cfb to the parentheses.

R1C9: L147-155: I could not find a description of how in-situ LAI measurements are carried out, although such data is presented in Fig. 5, could the authors clarify?

Response to R1C9: The authors will add the description of the LAI measurements to the materials and methods section.

R1C10: L177-183: It seems from the text that the version of code used in this study uses the SPAC module developed by Simeone et al. (2019), if so the authors should acknowledge and cite this work

Response to R1C10: The authors will add this citation to the section.

R1C11: L206-207: Is it a full mixing in the whole soil domain? Or some compartments are differentiated?

Response to R1C11: Full mixing is conducted within each soil layer (10, 30, and 60cm depths) and within the canopy and surface stored water (when applicable). The authors will clarify this in the revised manuscript.

R1C12: L217: The 100 "best" simulations have not been defined yet, please refer to Sect. 3.4.2

Response to R1C12: The authors will revise this statement to indicate that this analysis was conducted for each retained parameter set.

R1C13: L240: I do not understand the synchrony between the proposed description of rooting length and SPAC, as the latter module is mostly focused on tree mortality (roots included).

Response to R1C13: This statement was intended to describe the connection between the rooting distribution (vertical only) already present within Ech2O and the proposed lateral root distribution. The authors will remove the reference to the SPAC module to help clarify.

R1C14: Eq. (1): I am not sure how this equation was derived from Sperry et al. (2016). I am guessing it combines the cumulative root proportion provided in Eq. (6) in the above reference, the use of center-of-biomass depth, and layer depths in Ech2O-iso, but the intermediate steps to Eq. (1) escape me. In addition, I am confused so as if the beta factor here is the same beta found in Sperry et al. (2016) and its relation to the exponential factor k_{root} , also because the value of 0.995 is also found (for beta) in Sperry et al. (2016) Also, in calculating the vertical length, shouldn't one add the height-above-ground at which xylem measurement are made (here, 1 meter)?

Response to R1C15: In revision, the authors will expand on the derivation of this formula. As the reviewer noted, this equation is based on Eq. 6 from Sperry et al. (2016) (and code provided in the Sperry et al. 2016 publication); however, in the reference, all soil layers have equal biomass which is not the case in Ech2O. The equation was modified to produce biomass proportions at different depth intervals. As noted in Sperry et al. 2016, root biomass is calculated to 99.5% (in the equation here, 0.995) with $0 < \beta < 1$.

In Ech2O, k_{root} has a similar meaning to β but with different parameterised values ($0 \leq \infty$). The authors equated the translation of parameters (as indicated in the text) which was tested to ensure that equivalent values were produced before Eq1 was implemented into Ech2O.

R1C15: L246-253: This approach differs from Sperry et al. (2016), where the volume of roots is calculated in the first layer, using radial length in the first layer, and then radial in others layers is estimated by assuming that each layer has the same volume of root. It is likely not the case here because layer depth is fixed but k_{root} seems to be calibrated and differs between simulations. So I am guessing the authors used total root volume, implying that Eq. (2) uses total rooting depth (rather than d_1 as currently written) and then use Eq. (3) as a custom-made formula to reach the radial lengths in each layer?

Response to R1C15: The reviewer is correct that the volumes of roots in each layer are not equal. The authors will revise this section to better indicate that the equation was modified to account for different rooting root proportions in each of the soil layers. As with Eq1, this modification was tested prior to implementation in Ech2O to ensure that if rooting proportions were equivalent in each soil layer the root volume in each layer was also equivalent.

R1C16: L249: According Sperry et al. (2016), D should be the maximum rooting depth, not the total soil depth.

Response to R1C16: Within Ech2O, the vegetation rooting depth is maximized at the maximum soil depth. There is no additional parameterisation to reduce maximum rooting depth. The authors will clarify this in the revision.

R1C17: L252-263: While the principles of root-length-based transit times is nicely described, it is quite frustrating not to see the calculated values for the rooting length (radial, vertical, total) in the results section or elsewhere in the manuscript. This could be a supplementary figure or table, at a minimum.

Response to R1C17: The authors will add the calibrated rooting distributions (radial and vertical) to the supplementary material.

R1C18: L264-265: At first glance, this no-cavitation hypothesis seems inconsistent with the integration of the SPAC module, whose purpose is precisely to describe occurrence of cavitation using plant hydraulics. Did the authors found evidence that no cavitation occurred during the simulated growing season?

Response to R1C18: Given the relatively dry soils below the willow trees, the authors recognised the potential for water stress characteristics which is why the SPAC module was incorporated to test. Ultimately it was found that the willows were not under water stress during the simulated growing season which allowed for this approach to be used. The authors will clarify this in the revision.

R1C19: L288: Do the authors mean that the bottom depth of each layer in the model is fixed to correspond to 10, 40, 100cm, with effective layer “thickness” of 10, 30, and 70 cm, respectively? This information is provided in Table 3’s caption, but it would be handy to have it earlier in the manuscript.

Response to R1C19: The individual measurement depths are already provided in the materials and method section (L140). As the depths of each soil layer are specific to the study site presented here, the introduction of layer depths prior to the model set-up section could create confusion on the model capabilities.

R1C21: L293-295: How is the grouping done for vegetation parameters? This is quite unclear, all the more that the type of information on calibrated parameters in Table S1 is not provided for vegetation parameters, could the authors provide a similar table? In addition, the SPAC module requires further parameterization that was carefully constrained in Simeone et al. (2019), but no mention is made on this topic, nor associated parameters, in the manuscript. Overall, it seriously limits the reader’s understanding of the modelling setup used in this study.

Response to R1C21: The authors will add the vegetation parameters to the table in supplementary material with a description of parameter consideration.

R1C22: L305: Have the authors looked at the additional information brought by Ic-excess? This could further help analyzing contribution from shallow/deep soil horizons, and further fractionation effects (or lack thereof) during root-stem transport.

Response to R1C22: The authors did simulate both $\delta^2\text{H}$ and $\delta^{18}\text{O}$ and did not find large differences between the variables and measurements. The authors presented only $\delta^2\text{H}$ as it was directly calibrated, $\delta^{18}\text{O}$ (or Ic-excess) did not produce notably different results, and the increased data would make the already data-rich plots more difficult to interpret. Furthermore, the analytical precision of 2H is better than 18O, and the sensitivity of 2H to fractionation is greater than 18O. In addition, the isotopic data of soil (particularly shallow soil) were part of a large quantity of data in multicriteria calibration which was used to constrain multiple water and energy fluxes beyond soil evaporation. Given the similarities in water contribution from shallow/deep soil horizons to Bayesian mixing (Landgraf et al., 2021) the authors are confident in the models capabilities to estimate the contribution with the current multicriteria calibration. It was not an objective of this manuscript to assess potential fractionation in the root-stem transport, and the transport was assumed to be non-fractionating. The authors will clarify this in the revision.

R1C23: L307: By “split calibration”, do the authors mean using a calibration period and a validation period? Or a calibration period for one step, and another period for the other step? A combination of both? Please clarify.

Response to R1C23: The authors intended that “split calibration” meant splitting the data into a calibration and validation period. The authors will clarify this in revision.

R1C24: L309-315: I am confused so as to how this step-wise calibration was performed. First, I am interpreting L309-310 as having a first step using isotopes, energy fluxes and

water balance data as a constraint, and then a second step using biomass data ; or rather, 4 steps for each data group? Please be more explicit, and possibly add this information to Table 2 as well. Secondly, since each steps use 100,000 samples, I am guessing that step $i+1$ does not use a subsampling from calibration step i ; how were the calibration steps connected? Overall, this section needs a substantial rewriting to understand how calibration was actually performed ; under which hypotheses regarding parameter space, total number of parameters, etc. Consider adding additional supplementary tables with information on calibration ranges at each step, resampling procedures, etc.

Response to R1C24: The authors will reformat this section to improve the clarity of the stepwise calibration. The stepwise calibration was conducted in two steps: first with the isotopes, energy, and water balances, and second with the biomass data. Because the LAI time-series was used in the first calibration step, the influence of biomass simulations on the first step calibration was negligible. Due to the high sensitivity of the vegetation dynamic parameters, resampling of first step calibration was utilized. The "best 100" simulations show both independent soil and vegetation parameters.

R1C25: L325-328: How was the sub-discretization done? Also, why not trying to change the thickness of the first layer so that the measurement depths fall within the model layers, not at interfaces between model layers (e.g. layer 1 could be 20cm-deep)? Adding the same red line to L1 moisture under grassland could be informative in checking for percolation ; from these figures it seems that infiltration-percolation under the grass patch is underestimated.

Response to R1C25: The sub-discretization was conducted using 1cm increments, the authors will add this descriptor during the revision. Changing the thickness of the soil layers to have the midpoint as the measurement location would result in further complications, mainly the added volume that is accessible by soil evaporation and added volume to "dampen" soil moisture response. While the added depth in layer 1 may result in increased fractionation of soil isotopes (particularly at site A), the added volume for mixing will additionally result in a "dampened" isotopic dynamic. As described in the manuscript, the discretization was not utilized in calibration, thereby the "damped" soil moisture response with a deeper soil layer would greatly impact calibration. The authors did additionally sub-discretize the grass site, but due to wetter conditions and more frequent percolation (as shown by the more dynamic moisture in layer 2) the discretized moisture at 10cm was not notably different to the average of the soil layer. Infiltration/percolation appears likely appears low due to the underestimation of the soil moisture in layer at the grass site. It should be noted that the parameterisation of soil layers is uniform, thereby multiple layers must be estimated with the same parameter set. During revision, the authors will discuss the reason for underestimation of moisture in layer 2.

R1C26: L330: Another obvious isotopic feature is the much higher ~week-scale variability in 10cm isotopic at site A (Fig 3a) as compared to site B (Fig 3b) . This is reasonably differentiated in the simulations cells although 1. simulations at site A are too dampened and 2. there an unrealistic depletion in October at site B. While the former is briefly mentioned in Sect 4.3, I suggest to add these descriptions here and discuss them further on in the Discussion.

Response to R1C26: As described in **Response to R1C25**, part of the reason for more dampened isotopic compositions at Site A is in the mixing within the soil depth. A secondary

part, particularly in day-to-day variability is soil-vapour interactions. Modelled isotopic variability is dependent on infiltration and soil evaporation only. Thereby, variability in soil isotopes, particularly depletion when no infiltration occurs, cannot be estimated by the model. The model estimates the averaged variability. While there is an “unrealistic” depletion of soil isotopes in October at Site B, this depletion occurs notably at site A due to depleted precipitation isotopes. The difference between sites is likely due to differences in mixing. The authors will add additional descriptions of these characteristics to the discussion during revision.

R1C27: Figure 3: Are isotopic datasets daily-averaged in this figure? If so, it should be stated somewhere in the main text.

Response to R1C27: The isotope datasets were daily averaged for visual purposes. The authors will state the averaging in the figure caption.

R1C28: L344: The model description states that there are two thermal layers in Ech2O-iso (without providing the depth of each), can the authors briefly describe how they extrapolated the modelled soil temperatures at three depths?

Response to R1C28: The soil temperatures at different depths were estimated using the same linear damping formulation used to estimate soil temperature at the bottom of the thermal layers (Maneta et al., 2013). Different depths were estimated by using soil depth of different layers in the formulation following the estimation of surface temperature and thereby are not accounted in the energy balance. The authors will add this description to the manuscript.

R1C29: L345: Although the scales in Fig. 4 (Site B, latent & sensible heat fluxes) are quite squeezed (please consider expanding them), it is apparent that latent heat is overestimated throughout the growing season.

Response to R1C29: The authors will modify the figure to better show the results.

R1C30: Figure 4: How was modelled grass transpiration converted into sapflow? It would be informative to see the transpiration rate (mm/d) in the second row, perhaps using a secondary y-axis on the right?

Response to R1C30: The authors will revise the y axis label for the grass to indicate that this is the volumetric water utilized by the grass. The authors will add a secondary y-axis to show transpiration rates.

R1C31: L358-364: Could the authors precise which MODIS LAI product was used? These products usually have a much larger spatial resolution (500m-1km) than the modelled domain of this study. Can the authors develop on their methodology and assumptions made to distinguish willow and grass patches?

Response to R1C31: The MODIS dataset used was MOD15A2, the authors will add this

product description to the manuscript during the revision. As the reviewer mentions the large (500m) resolution does not provide a clear division of vegetation types. The area surrounding the large (500m) area surrounding the site encompasses a greater majority of leafy tree vegetation, resulting in LAI from MODIS representing LAI of willows more than the grass. As downscaling this information is complex, the authors scaled the dynamics (as indicated in Fig 5) for the grass site using LAI measurements from other studies to provide a maximum and minimum LAI range. A similar procedure was conducted for nearby studies (e.g. Smith et al., 2021). The authors will add this description during the revision.

R1C32: L370: A reference to Table 3 would be useful.

Response to R1C32: The authors will add a reference to Table 3.

R1C33: Table 3: This table shows a lot of information. It might be much more reader-friendly if transformed to a multi-panel plot, either using bar or points with errorbar, e.g. keeping the row and column organization with facets and a color code for time periods. In addition, the third grouped-row (RU-L*) might be more intuitive if instead of layer number, depth ranges were used (e.g. RU[0-10cm]).

Response to R1C33: During the revision the authors will consider translating this table into a figure, though we are mindful that the paper already has a number of complex multi-panel plots and some readers will prefer numerical detail. The authors will change RU-L* to RU[depth] to better aid the readers.

R1C34: Figure 6: My understanding is that soil isotopes are measured in-situ at three depths, as reported in Fig. 3; why then are there not 3 solid lines in the diurnal plots, instead of 1 (panels a) and c)) or none (panels b) and d)), and why is the solid (measurement) line flat, as if there no high-frequency information? Additionally, given the high-frequency dynamics, readability would be improved by making this figure wider, e.g. having Willow 1 and Willow 2 panels on top of each other.

Response to R1C34: The authors thank the reviewer for identifying this error. The soil legend should indicate measured soil is dashed-line and solid line is simulated soil. The model does not estimate sub-daily variability in soil isotopes. All three soil depths are present on the plot; however, the simulations are all non-diurnal. This will be clarified in revision. The authors will additionally revise the figure as suggested to improve the readability.

R1C35: L412: A reference to Fig 3a (in addition to Fig 6a & b) would be helpful.

Response to R1C35: The authors will add a reference to Figure 3 during the revision.

R1C36: Table 4: I am assuming the values between brackets give KGE variability among best runs? If so, why isn't the same number given for AIC? Consider using a plot rather than a table (although less critical than for Table 3).

Response to R1C36: The values in brackets for KGE (as indicated in the caption) are for subdaily variability. Evaluation of only subdaily variability for AIC was too short for significant testing which is why they are not shown. A plot of this data would be more difficult to depict without many subpanels because the scale of AIC changes for different time-steps. There are already a significant number of plots within the manuscript and supplementary material.

R1C37: L440-449: In my view this labelling by “contributing month of the year to current store/flux” rather provides a very nice perspective, equally important and intuitive as the “time elapsed since arrival” reported above ; it directly replies to the question “what precipitation period is most important for plant water use?” ; I would suggest to move key Fig. S3 to the main text.

Response to R1C37: The authors will transfer the fractionation contribution to the supplementary material and move the monthly water contribution to Figure 7.

R1C38: Fig. 7: “Time in xylem” (panel g) is somewhat misleading, as the transit time considered integrate transit along root and xylem? Besides, my impression was that transit length (and thus time) in the xylem was neglected when computing $v(i)$ in Eq. (1) (see related comments above)?

Response to R1C38: The authors will revise the y-label to “time from root-uptake to 1m above stem base”. Eq. (1) provides the vertical distance of biomass and Eq. (3) provides the radial distance. As indicated in Fig. 2, this translates to the distribution of transit times as a function of root length. Longer rooting lengths produce a longer transit time.

R1C39: L450: “an increase of zero days” seems somewhat odd, maybe rephrase: “Since instantaneous(?) mixing equates xylem water age to that of where water is taken up (reaches 1m instantly), transit times along the root-xylem system are only shown...”.

Response to R1C39: The authors will revise the statement as suggested by the reviewer.

R1C40: L479-496: The underestimation in modelled willow transpiration (or rather, th sapflow, see a comment above) at the end of the growing season is quite interesting, as perhaps not as “minor” as stated here ; the model-data discrepancy exceeds the dispersion among best runs. That would deserve further discussion, as the current ones somewhat circumvent the issue with more general considerations. Besides, the concomitant overestimation of modelled L1 moisture (and possibly L2’s, and thus percolation, Fig. 3a) suggests that it’s not necessarily due to missed contributions from adjacent cells or a short-term reliance on deeper stores (which would have been interesting as a drought-protection process!), but merely that there is something wrong with evaporative demand when the energy balance is computed ; is it something due modelled energy fluxes and/or to forcings? In other words, is a process being missed?

Response to R1C40: The authors will add further discussion of the late growing season period to improve the explanation of measured and modelled results. The authors indicated that the discrepancy of simulated to measured sapflow was minor as when considering all sapflow measurements (4 measurements in 2 willow trees), the simulation bounds overlap the measurement bounds. As these overlap, there is limited definitive

evidence that processes are missing from the model.

R1C41: L480-481: Is this sentence suggesting that EcH2O-iso account for off-cell contribution to calculate root water uptake? And associated transit times?

Response to R1C41: The modifications made to EcH2O-iso calculate the proportion of vegetation water utilized within and outside of the cell of the vegetation. Thereby where water was used from outside the cell, water age and isotopes were additionally considered. Where model domain was exceeded, moisture and isotopes were assumed to be equivalent to the grass as it surrounds the model domain. Mixing in the transit times consider water source (within v. outside cell). The authors will revise the methods section to clarify this modification.

R1C42: L492-494: From the 'slight decrease' I am wondering if the authors meant "was under stress"? Besides, it could be informative to further have the absolute biomass in each compartment (in addition to biomass allocation) reported somewhere, perhaps as time series over the growing season, to check if the potential decay rates exceed (or not) allocation, and where.

Response to R1C42: The decrease in root biomass production (relative to leaf and stem) during the growing season is consistent with a willow not under water stress. The vegetation allocates more biomass production to leaves and stems. If the vegetation were under water stress, root biomass production would increase as the vegetation "searches" for water. The authors will add the change in biomass ($\text{gC}^{-1}\text{m}^{-2}$) to the supplementary material.

R1C43: L514-521: I assume this part of the discussion will be substantially revised (see General Comments)

Response to R1C43: As described in the **Response to General Comments**, clarifications in the modifications to the EcH2O-iso model reduce the limitations of the approach. During revision, the authors will clarify the significance of utilizing the approach as well as the limitations of the approach.

R1C44: L536-539: If the measurement uncertainty is known, it would be highly informative to add it as error bars on any related plots presented in this manuscript. Actually, it should be common practice, helping to temper interpretations where inferred dynamics are commensurate with uncertainties.

Response to R1C45: Error bars related to vegetation isotopes are already presented in Fig. 6, which highlights the wide ranges of xylem isotopes. During revision, the authors will add error bars to the sapflow, and soil moisture, temperature and isotopes to better present the datasets.

R1C45: L546-553: This issue could be explored with the different tree storage mixing types presented in Knighton et al. (2020), it could help the current discussion and open

avenues for further development?

Response to R1C45: The authors agree that a combination of tree storage mixing and root-stem transit mixing presents open avenues for further development. Given the periodic nature of the additional contribution, a modification to the tree water mixing (storage and energy-based) would likely refine model estimations. The authors will add this to the discussion during revision.

R1C46: L552: Are the authors referring to measured basal diameter?

Response to R1C46: The authors will revise this during revision.

R1C47: L561-563: Maybe further precise “across Switzerland” after “Allen et al. (2019)” and “in the study region” before “(Miguez-Macho and Fan, 2021)”?

Response to R1C47: The authors will revise this during revision.

R1C48: L575-578: This is also potentially due to the fact that other studies considered root-to-shoot transit times (Meinzer et al., 2006) while this study “stops” at 1m height.

Response to R1C48: While this will have some influence on the transit time, the velocity in the xylem is relatively fast, which even during the late growing season would only add ~3 days on average to the transit time to the leaf. Vegetation species properties contribute to greater changes to the transit time than measurement height in this study. The authors will indicate the measurement height difference in the revision for transparency.

R1C49: L579: Essential or indispensable?

Response to R1C49: The authors will revise this during revision.

R1C50: L580-591: Again, it is quite surprising not to see any references to Knighton et al. (2020), a study the authors contributed to, and which precisely studies this issue of tree water storage and mixing.

Response to R1C50: This section was intended to directly discuss the impact of cell storage release as a contribution to root-stem transport mechanisms. It was not the intention to downplay the significance of the results of the work conducted by Knighton et al. (2020) which is acknowledged elsewhere in the manuscript. As with **Response to R1C45**, the authors will add further discussion of potential advancements with reference to the work conducted by Knighton et al. (2020), though we are also sensitive to potential accusations of overuse of self-citation.

R1C51: L589: Menekes et al. (2021) and Benettin et al. (2021) are recent studies on

this topic, albeit in semi-controlled conditions.

Response to R1C51: The authors will add these references to the discussion during revision.

R1C52: Conclusion: Having the Conclusion framed as Summary (L596-608) seems a bit redundant with the abstract and the main text. Rather, discussing high-level limitations, insights and potential avenues would more efficient.

Response to R1C52: The authors will revise the conclusions during revision to reduce "summary" of results.

R1C53: Figure S1: The channel is not represented in Fig 1b, and the similar color code for snowpack/channel may confuse the reader).

Response to R1C54: The authors will remove "channel" from the legend in Fig S1.

R1C54: Code availability: The statement is somewhat incomplete, as the post-processing model to compute root geometry and associated transit times does not seem to be on the referenced repository.

Response to R1C54: Upon publication, the authors will update the bitbucket code to better reflect the code version used in this manuscript.

Data availability: Again, this statement is misleading, first because "open-access" is incompatible with password-protection. Secondly, not all the data used in this manuscript is archived in the provided link ; only sapflow, stem variation and in-situ isotopes data are listed, while neither eddy-covariance energy fluxes, micrometeorological measurements, in situ LAI, and soil moisture can be found. I would strongly encourage to have all datasets published, or at a minimum have them listed along with their open-access metadata on FRED so that potential users can make informed queries to the curators.

Response to R1C55: Upon publication, the authors will update the data available on FRED to better reflect the data used within this manuscript.