The study by Smith et al. (bg-2021-278) presents a novel combination of in situ temporally high-resolution measurements of micrometeorological variables, water fluxes, stores and stable isotopes in soil and xylem together with a process-based modelling approach, in order to identify the dynamics of water partitioning under 2 willow trees and a neighboring grass patch over a growing season. The increased perspective on soil-plant water dynamics brought by this intensive monitoring, further presented in another manuscript (Landgraf et al., 2021) is used a for a multi-data calibration and evaluation of the ecohydrological outputs provided by the EcH2O-iso model. The authors use this baseline to then evaluate a new conceptualization of water uptake and transport along a vertically-and-laterally-distributed root profile, in order to understand the relation between soil and xylem water dynamics and signatures.

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.

General points raised:

i.) I want to apologize for accusing the authors of 'cheating'. Probably, this came over too strong. I was simply wondering if it is completely justified to cite another paper that is under review. However, since HESSD is a citable source, this should not be a problem in this case.

ii.) Lack of replication: I thought about this again, and I think this should not be the concern for the modeling part of the study, i.e., the present manuscript. Rather, it is a concern that applies to Landgraf et al. 2021, where the experimental part is presented. I still do think that this is a serious concern. Replication is simply and inarguably a key principle of ecological and ecohydrological field research (not only, but especially those disciplines).

iii.), Dual-isotope space: I still believe that providing both stable water isotopes would greatly have benefited the study. Having that said, I do not expect the authors to incorporate it into this study as the manuscript is already complex and rich in information. The advantage of utilizing both isotopes (e.g. in dual-isotope plots) has been shown beneficial and used in hundreds of studies. It helps to understand evaporative conditions and interpret the degree of fractionation a particular sample/tree/species is subjected to by providing one simple measure. I also believe it would be highly beneficial in terms of model evaluation (measured vs. simulated lc-excess, for instance).

iv.) Complexity of the model: The model is, without doubt, incredibly powerful and for the first time I feel that all important features of the critical zone are represented sufficiently. It is also very cool to see the in situ isotope measurements do provide benefits for modeling purposes. Hence, it has great opportunities for future applications and I applaud the authors for the developing this highly useful model. However, I think it is also extremely data demanding and not simple to parameterize by any means. When applying the model, it needs to be carefully decided if cost and benefit are in balance. When reading the manuscript, I frequently asked myself: 'Are there optional components in the model, or is all this data always needed?' In other words, what are the minimum data requirements? Working in remote areas (mainly forests) since many years, I was wondering for instance, how rooting distributions would be incorporated in systems with more heterogeneous root systems and deeper soils. Can the rooting distributions be calibrated? I know this is not

necessary to answer in this manuscript, but there are practical questions me (and probably many other readers) might have.

v.) This is a thought and not a criticism: In isotope science, a few per mil can make a huge difference. With all the uncertainties involved into measuring (more so for in situ, but also for destructive sampling) and modeling (and that was the background of my question on uncertainty), I am honestly not sure, how well we can describe natural systems currently. The community is very active at present, but – in my opinion – we need to be critical and honest with ourselves in this regard. In the revised version, the authors have included a number of statements touching on this issue, which is much appreciated.

I congratulate all the authors for the study and wish them all the best for future applications.

Matthias Beyer

- Abstract, l.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.
- 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
- Introduction: I very much like the objectives as per R1
- 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)
- L.224/225: (suggestion to add) and no fractionation during root water uptake?
- L.280: 'estimate xylem δ2H' – is this a typo, or do the authors mean only δ2H and not δ18O is estimated? → 'estimated xylem isotope values'?
- 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.
- L.357: typo: the unit of soil moisture content should be m3/m3
- L.366-367: 'soil water isotopes' should read δ2H, I think? Because in table S2 I only see δ2H
- L.679: replace 'deuterium' by 'δ2H values'?