Response to Referee Comment #2

Referee #2's Comments:

Dear Authors, Dear Editor,

I have previously reviewed the first version of this manuscript and I appreciate the modifications brought forward by the authors.

They:

- modified the approach by calibrating the model to the three sites and by recombining the vegetation parametrizations with the soil parametrizations into 9 scenarios, which are then averaged across soil types to yield the predictions for the three distinct forest types,
- did a separate assessment of temporally split calibration/validation periods,
- added an analysis between wet/dry years and by month,
- improved framing and explanation of the approach,
- added a comparison with independent ET flux based on MODIS data,
- reduced figures in the main manuscript to essential ones. The supplementary material (which is referred to in the discussion) remains quite extensive, but this is okay.

Clarity of language and conciseness could both still be improved to better convey the interesting findings to the reader. Also, greater care in using consistent language and terminology would improve readability: e.g. the current version still uses terms of "resilience" and "drought impact" without clearly specifying what ecosystem functions are considered. Here, a more precise, more technical language should be preferred.

Overall, I think the manuscript is suited for publication in HESS after revisions (see comments below).

Dear Editor and Reviewer #2,

We thank the reviewer for the detailed, careful, and constructive comments, which have helped us further improve the clarity, consistency, and robustness of the manuscript. We also appreciate the reviewer's recognition of the substantial revisions made in the previous round.

In response to this round of comments, we have revised the manuscript to clarify the definition and use of "ecohydrological resilience," improve the explanation of the 3 × 3 scenario matrix and parameter recombination, more explicitly discuss equifinality and uncertainty, and refine the interpretation of key figures, especially Figs. 7, 10, and 12. We also improved terminology consistency, clarified the root water uptake formulation, and addressed the suggested figure and technical corrections.

We believe these revisions make the manuscript clearer and more transparent. Below, we provide detailed point-by-point responses to all comments.

Sincerely,
Dr. Cong Jiang
on behalf of all co-authors

1) General comments:

Q1. Clarity and precision of language: "Ecohydrological resilience" is nowhere clearly defined (and similarly used expressions such as: hydrological resilience, water retention and landscape resilience, resilience of forest types, land use resilience, water supply resilience …). L.50-52 contains an attempt at definition but does not specify ecosystem functions that are considered.

Reply to Q1: Thank you for this comment. We revised the definition of ecohydrological resilience to specify the key ecohydrological functions considered in this study, including soil moisture storage, transpiration, and groundwater recharge, and checked the manuscript for consistent terminology (L. 51-54).

Q2. Fig10, L555-572, L.30-32: Figure 10 is quite difficult to understand, since it shows differences of differences, i.e. between-scenarios-differences in the wet-dry-year differences. Great care must be taken here to clearly state the findings. Maybe also refer to Figure 5b that shows the reference wet-dry-year differences.

Reply to Q2: Thank you for this comment. We revised the Fig. 10 description to more clearly explain that the figure shows scenario-based differences in wet–dry-year contrasts (L. 569-570). We also added a reference to Fig. 5b for baseline context (L. 570-572).

Q3. L.323 onwards: Please clarify how the parameters were combined in the 3x3 scenario matrix. E.g. were the 100 best vegetation parameter sets combined together with the 100 best soil parameter sets. (Does this assume they are independent of each other, i.e. no equifinality?)

Reply to Q3: Thank you for pointing this out. We revised Section 3.5 to better clarify how vegetation- and soil-related parameter sets were combined in the 3 × 3 scenario matrix (L. 328-329). We acknowledge that this recombination assumes that vegetation and soil controls can be separated for scenario analysis (L. 335-336). However, we minimized equifinality effects by using the 100 best-performing calibrated simulations and vegetation-type–specific parameter ranges, with parameter convergence assessed in Fig. S4.

Q4. Figure 7b shows that there is a clear trade-off between the transpiration and recharge across the 100 best parametrizations. The calibration was thus not able to clearly constrain the relative importance of these two fluxes with the given calibration targets. In other words, the 100 parametrizations lead to similar (equifinal) calibration targets for different, mutually compensating reasons.

The size of this effect appears quite large, e.g. for conifers ranging from 5% to 30% recharge, which is compensated by transpiration going from 55% to 35% (Fig 7b). This also appears to be the reason for the large error bars on the order of 100mm/yr in Figure 6 and Figure 8a to 8i.

Reply: Thank you for these insightful comments. We agree that Fig. 7b highlights a trade-off between transpiration and groundwater recharge across the retained parameter ensembles. However, the spread in flux partitioning reflects not only equifinality within the retained parameter sets, but also differences among the paired vegetation–soil configurations, particularly contrasting soil hydraulic properties and water-holding capacities across the three sites.

From that follow 4 suggestions:

a) I suggest to briefly mention this observation in Figure 7b in the results in 4.3.1. (in the discussion it is already mentioned in l.648-658)

Reply to Q4a: Thank you for this suggestion. We revised Section 4.3.1 to explicitly discuss the transpiration and groundwater recharge trade-off shown in Fig. 7b and clarify that the observed spread reflects both equifinality and differences among the paired vegetation–soil configurations (L. 493-494).

b) I suggest to add the (large) prediction intervals to the provided flux estimates in Fig 12,

Reply to Q4b: Thank you for this suggestion. We considered adding prediction intervals to Fig. 12; however, because Fig. 12 is intended as a summary visualization of ensemble-mean flux partitioning under long-term, dry-year, and wet-year conditions, adding large uncertainty ranges would substantially reduce readability. Prediction intervals are already shown in Figs. 6 and 8, and we expanded the discussion of uncertainty and equifinality accordingly.

c) (as discussed in l.656-658): while this has led to large error bars in figure 8, it does not necessarily invalidate the qualitative changes under the 9 scenarios. However, I am now wondering if the correlations between parameters (for soils and for vegetation) would need to be taken into account when creating the 3x3 scenario matrix. It might be worthwhile to add to the discussion that this was not considered and what limitations this entails. Would disregarding correlation inflate uncertainties? Conversely, would keeping the correlations even reduce uncertainties? (It might, since it would do so for calibration targets (e.g. soil moisture in Fig. S11 to S14 (error bars not shown)).)

Reply to Q4c: We expanded the discussion to acknowledge that parameter correlations were not explicitly preserved in the 3 × 3 scenario framework (L.667-671). This may influence, and potentially inflate, uncertainty estimates, although the qualitative scenario responses remain supported by consistent ensemble-mean patterns.

and d) Could these mutually compensating processes be illustrated concisely with a pairwise correlation analysis/plot of the parameters among the 100 best parameter sets in the supplementary material?

Reply to Q4d: We appreciate this suggestion. Parameter convergence across the retained ensembles is already illustrated in Fig. S4. To keep the Supplement concise, we did not add a full pairwise correlation analysis, but we have expanded the discussion to acknowledge this limitation more explicitly.

2) Specific comments:

Q5. L516: I am unsure about giving "soil moisture limitation" as single reason. Given that ET still increases it looks rather like more efficient interception, leading to larger Ei. Since your model modulates Tp1-3 using a combination of soil moisture limitation (e.g. STO/Smax) and subtracts canopy evaporation from atmospheric demand (i.e. Tp-Ei) it is probably a combination of both: "soil moisture limitation" and "interception evaporation". (This would also be more consistent with the paragraph L519-527).

Reply to Q5: Thank you for this helpful comment. We agree that the reduced transpiration at high LAI is not solely caused by soil moisture limitation, but also reflects increased interception evaporation and associated reductions in available transpiration demand. We revised the text accordingly to better distinguish the combined effects of soil moisture limitation and interception evaporation (L. 525-527).

Q6. L525: "even at constant LAI", I cannot follow this statement, since I do not see any results that show transpiration when canopy density is varied with constant LAI.

Reply to Q6: Thank you for pointing this out. We revised the sentence to remove the confusing reference to "constant LAI" and clarify the trade-off between interception evaporation and transpiration under high canopy density (L. 537-539).

Q7. L643-645: I could not follow this statement. Please clarify. Your model is still a big leaf model using a single set of apparent parameters instead of multiple sets for each vegetation species that interact with each other. So, I don't see how the Monte Carlo calibration would allow extension to mixed-species stands.

Reply to Q7: Thank you for this clarification. We agree that EcoPlot-iso uses an aggregated plot-scale representation and does not explicitly resolve species-specific interactions or sub-grid vegetation heterogeneity. We revised the text accordingly (L. 657-661).

Q8. L.689-691: This statement appears imprecise: there are no simulations of mixed forest presented, and water yield (which I understand as runoff) seems not sustained since it is practically zero.

Reply to Q8: Thank you for pointing this out. We revised the statement to avoid referring to mixed forests and water yield, and clarified that agroforestry supports water availability mainly through groundwater recharge (L. 708-710).

Q9. L. 719-722: This statement appears incorrect and unneeded (and moreover it contrasts with your statement in lines 72-74). Both EcH2O(-iso) and RHESSys take into account dynamic water availability in combination with (static) root distributions to compute and partition transpiration across soil layers. Moreover, alternative tracer-aided forest water balance models such as HYDRUS-1D 2012 (Stumpp et al. 2012), HYDRUS-1D 2021 (Zhou et al. 2021), and LWFBrook90 (Bernhard et al. 2025) also simulate root water uptake from multiple soil layers. The improved rooting scheme in EcoPlot-iso developed for this study thus seems to bring EcoPlot-iso actually closer to these models.

Reply to Q9: Thank you for this clarification and for providing these helpful references. We agree that multi-layer root water uptake is already represented in models such as EcH2O, RHESSys, HYDRUS-1D, and LWFBrook90. We revised the text to clarify that the contribution of this study is the integration of a simplified depth-dependent root water uptake formulation into EcoPlot-iso, enabling transpiration demand to be partitioned across soil layers according to rooting distribution and soil moisture availability within a parsimonious conceptual framework (L. 737-739).

Q10. Figure 7: Still very confusing labelling for axes: if possible, axes labels should be put directly next to the corresponding axes tick labels. And caption (L492) could be improved as: “based on individual model simulations” => “based on the 100 best model simulations”

Reply to Q10: Thank you for the suggestions. We revised Fig. 7 by moving the axis labels from the triangle corners to the corresponding axis sides, improving their alignment with the tick labels. The caption was also corrected accordingly.

Q11. L.294: The second step is still unclear as it is written in the manuscript. It implies the “parameter space” was retained, which I interpret as being the bounding box of valid parameters and would thus require another Latin Hypercube Sampling? Based on your direct reply to reviewers, I can see that you kept the selected parameter sets and re-ran the same simulation, but this 2nd time you kept the full output. I hence suggest modifying: “parameter space” => “[N] parameter sets”. And potentially add to L290: “This step reduced the number of parameter sets from 100’000 to [N].”

Reply to Q11: Thank you for pointing this out. We revised the text to clarify that selected parameter sets, rather than the parameter space, were retained for the second simulation step. We also added a sentence to further clarify the reduction from the initial 100,000 sampled parameter sets, as suggested (L. 293-294).

Q12. L 336-343: in parentheses I suggest to explicitly list all the parameters of each type: “Forest-typespecific vegetation parameters (rE, alpha, IntSp, k)” and later on: “soil-related parameters (ks1, ks2, ks3, Smax, GWmax, Lmax, Ic, g1, g2, g3, PFscale, StoSo, gwSp, lwSP, x)”. Explicit listing of all 20 parameters should clarify the approach.

Reply to Q12: Thank you for the suggestion. We revised the text accordingly (L. 347-348). Isotope-related parameters were not included here because the forest management scenarios were evaluated using hydrological fluxes and storage outputs, rather than isotope outputs.

Q13. Supp. L109: “Root distribution factor (dimensionless)” is not correct. In your formulation beta is not dimensionless. Judging by Figure S2 it appears your betas are in (m-1) since you multiply them with [0.05m, 0.20m, 0.65m].

Reply to Q13: Thank you for pointing this out. We corrected the unit of β from dimensionless to m^{-1} in the Supplement and clarified the unit of β consistently in the manuscript.

Q14. Supp L109: Min,max values provided under calibrated range do hardly differ from the initial range. This would suggest the calibration to be non-informative for these parameters. However, Fig S4 shows indeed some reductions in the 100 behavioral simulations. Maybe rather report 5th and 95th percentiles instead of min,max values in the table?

Reply to Q14: Thank you for this suggestion. We revised Table S3 to report the 5th and 95th percentiles, instead of minimum and maximum values, together with the median of the 100 best-performing simulations. This better reflects the constrained calibrated parameter ranges.

3) Technical corrections:

Clarify language:

Q15. L.25: "rooting distribution AND stand ages" seems confusing, since in your framework you use rooting distribution as a proxy for stand age.

Reply to Q15: Thank you for pointing this out. We agree and have revised the sentence to clarify that rooting distribution was used as a proxy for stand age.

Q16. L. 30-32: This sentence remains unclear as to what is compared: 1) what is compared? dry vs wet? Or conifer vs broadleaf. 2) Moreover, what does "peak drought sensitivity" mean?

Reply to Q16: Thank you for pointing this out. We revised the sentence to clarify that the comparison refers to transpiration contrasts between conifer and broadleaf forests and the wet–dry-year contrast between these forest types. We also removed the unclear phrase "peak drought sensitivity."

Q17. L. 33, L. 99: the target of drought impacts should be clarified. Is it on groundwater recharge? On vegetation health?

Reply to Q17: Thank you for this comment. We revised the corresponding text to clarify that "drought impacts" refers to impacts on water partitioning and key ecohydrological functions, particularly soil water availability and groundwater recharge.

Q18. L. 50-52: the definition of ecohydrological resilience remains unclear, what kind of functions? Runoff generation? Groundwater recharge? Others?

Reply to Q18: Thank you for this comment. We revised the definition to specify that ecohydrological resilience refers to the ability to maintain key ecohydrological functions, including soil moisture storage, transpiration, and groundwater recharge, under drought stress.

Q19. L. 217: please try to use consistent language to refer to the soil layers. L.217 uses shallow/middle/deep; L223/Fig2: upper/lower/deeper; L400: shallow/lower/deep; L403: surface/intermediate/deeper; Table3/Fig6: upper/lower/deep; Fig4: surface/lower/deeper; (eq1-3 use STO, GW, SDeep) Others:

Reply to Q19: Thank you for pointing this out. We revised the manuscript, figures, tables, and equations to consistently refer to the three soil layers as upper, middle, and deep soil layers, with corresponding storage variables S_U, S_M and S_D.

Q20. L.108, L.152: soils brown => brown earth?

Reply to Q20: Corrected.

Q21. L.131: superfluous "and"

Reply to Q21: Corrected.

Q22. L254: 2004 => 2024

Reply to Q22: Corrected.

Q23. L.304: shortter => shorter

Reply to Q23: Corrected.

Q24. L352: Here, you could clarify that not only LAI is changed but also the vegetation parameters.

Reply to Q24: Thank you for this comment. We revised the sentence to clarify that both LAI and vegetation parameters were varied among forest types.

Q25. L.399, L429: KGE => mKGE and add "modified"
Reply to Q25: Corrected and checked throughout the manuscript to consistently use modified Kling–Gupta Efficiency (mKGE).

Q26. L425: 20-30cm => 10-30cm
Reply to Q26: Corrected.

Q27. L465: Given the competition between transpiration and recharge: "In contrast" => "Accordingly". Since the two results appear consistent and not contrasting.
Reply to Q27: Corrected.

Q28. L516: "Fig 8c" => should probably be "Fig 8i"
Reply to Q28: Corrected.

Q29. L569: "deep root, particularly" => "deep root (Fig. 10e), particularly"
Reply to Q29: Thank you for pointing this out. We checked the figure panel references, but the suggested Fig. 10e reference was not applicable.

Q30. L641: "represent" => "represented"
Reply to Q30: Corrected.

Q31. Supp L109: alpha initial range for CF seems wrong.
Reply to Q31: Checked and corrected.

Q32. Fig 12b: if possible, remove the frame around the white box hiding the broadleaf tree.
Reply to Q32: Thank you for the suggestion. The frame around the white box in Fig. 12b was removed.

Tague, C. L., & Band, L. E. (2004). RHESSys: Regional Hydro-Ecologic Simulation System—An Object-Oriented Approach to Spatially Distributed Modeling of Carbon, Water, and Nutrient Cycling. *Earth Interactions*, *8*(19), 1–42. https://doi.org/10.1175/1087-3562(2004)8%253C1:RRHSSO%253E2.0.CO;2

Stumpp, C., Stichler, W., Kandolf, M., & Šimůnek, J. (2012). Effects of Land Cover and Fertilization Method on Water Flow and Solute Transport in Five Lysimeters: A Long-Term Study Using Stable Water Isotopes. *Vadose Zone Journal*, *11*(1). https://doi.org/10.2136/vzj2011.0075

Kuppel, S., Tetzlaff, D., Maneta, M. P., & Soulsby, C. (2018). EcH$_2$O-iso 1.0: Water isotopes and age tracking in a process-based, distributed ecohydrological model. *Geoscientific Model Development*, *11*(7), 3045–3069. https://doi.org/10.5194/gmd-11-3045-2018

Zhou, T., Šimůnek, J., & Braud, I. (2021). Adapting HYDRUS-1D to simulate the transport of soil water isotopes with evaporation fractionation. *Environmental Modelling & Software*, *143*, 105118. https://doi.org/10.1016/j.envsoft.2021.105118

Bernhard, F., Knighton, J., Seeger, S., Waldner, P., & Meusburger, K. (2025). LWFBrook90.jl—Including Stable Water Isotopes in a Soil Vegetation Atmosphere Transport Model to Constrain Vertical Root Water Uptake Dynamics. *Journal of Advances in Modeling Earth Systems*, *17*(11), e2024MS004445. https://doi.org/10.1029/2024MS004445