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).

1) General comments:

- 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.
- 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.
- 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?)
- 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.

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

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?

2) Specific comments:

- 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).
- 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.
- 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.
- 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.
- 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.
- 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"
- 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]."
- L 336-343: in parentheses I suggest to explicitly list all the parameters of each type: "Forest-type-specific 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.
- 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].
- 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?

3) Technical corrections:

Clarify language:

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

- 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?
- L. 33, L. 99: the target of drought impacts should be clarified. Is it on groundwater recharge? On vegetation health?
- L. 50-52: the definition of ecohydrological resilience remains unclear, what kind of functions? Runoff generation? Groundwater recharge? Others?
- 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:

- L.108, L.152: soils brown => brown earth?
- L.131: superfluous “and”
- L254: 2004 => 2024
- L.304: shortter => shorter
- L352: Here, you could clarify that not only LAI is changed but also the vegetation parameters.
- L.399, L429: KGE => mKGE and add “modified”
- L425: 20-30cm => 10-30cm
- L465: Given the competition between transpiration and recharge: “In contrast” => “Accordingly”. Since the two results appear consistent and not contrasting.
- L516: “Fig 8c” => should probably be “Fig 8i”
- L569: “deep root, particularly” => “deep root (Fig. 10e), particularly”
- L641: “represent” => “represented”
- Supp L109: alpha initial range for CF seems wrong.
- Fig 12b: if possible, remove the frame around the white box hiding the broadleaf tree.

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