Reviewer #2

Upfront we would like to express our sincere gratitude to the reviewer for the time and effort invested in reading our manuscript. We highly appreciate the very constructive and insightful comments. Below we provide clarifications in detailed replies to all comments.

<u>Comment:</u>

The thematic of parameter stationarity in climate impact studies is an important topic when aiming at generating suitable future water resources projections. The authors explore here a framework to ingrate possible evolution of root zone storage capacity (S_R) in hydrological modelling experiments. The focus on alpine catchments in Austria and focus on impacts on streamflow response to changing soils. I like the topic and the way the authors present it. The findings represent a sample on the topic that might help future developments in this direction.

I list here below some thoughts and issues to be addressed.

<u>Reply:</u>

We thank the reviewer for the positive feedback on our study and for recognizing the importance of parameter stationarity in climate impact studies, particularly for alpine catchments.

<u>Comment:</u>

30 – 109: When accepting this review, I wondered if the authors would make a comment on Gao et al. (2023). I witnessed an interesting discussion on that between a hydrologist and a soil scientist. This was rather interesting, and I really wish the authors make their thoughts on it.

<u>Reply:</u>

Thank you for pointing out the discussion surrounding Gao et al. (2023). The debate over whether soil characteristics—such as texture, porosity, and hydraulic conductivity—serve as primary determinants of water flow and moisture availability, or if, conversely, water movement is predominantly influenced by ecosystems and their responses to climatic factors, as suggested by Gao and colleagues, is indeed highly relevant in this context. We will include this in the introduction and discussion section of final manuscript to further underline the importance of ecosystem interactions for the partitioning of water, rather than soil properties alone.

<u>Comment:</u>

69 – 84: The introduction on optimality is very well formulated. At line 534 you compare your findings to the ones presented in Speich et al. (2018). In the paper you cite optimality approaches are used and discussed. You could consider adding this study also in the introduction.

Reply:

Agreed. We will include this in the final manuscript.

Comment:

103 – 105: Staying with Speich et al research, they published in 2020 a study with transient evolution of root zone storage capacities in an alpine catchment according to different scenarios (Fig 9 in Speich et al., 2020). This might be relevant for you. Also, in Speich et al. simulations with and without dynamic S_R are presented and discussed.

Reply:

Thank you for pointing out the research of Speich et al. (2020). Their study, investigating interactions between hydrological and forest dynamics under climate change scenarios in alpine catchments, is indeed relevant for our analysis. We will incorporate this reference the final manuscript.

Comment:

*113-115***:** *The authors focus on alpine catchments with shallow soils. Is this more than an opportunistic choice stemming from the Hanus et al. (2021) study? I agree that there are the places where possibly most of soil genesis might occur, but is S_R not more dynamic in low-land areas? More in general, I see the trade-off in terms of "duplication" between this section 2 and section 2 in Hanus et al. (2021) as unproblematic.*

Reply:

The reviewer is correct in noting that our previous work (Hanus et al., 2021) investigated the effects of a changing climate on streamflow using the same model and study catchments in the Central Alps. However, the critical distinction in this study is the incorporation of potential changes in the role of vegetation, which was not considered in the earlier work. To allow comparability, the choice of catchments was thus guided by Hanus et al. (2021).

Soil genesis rates were not a decision criterion as previous work, including Gao et al. (2023), referred to by the reviewer above, but also many others (e.g. McCormick et al., 2021), demonstrate that subsurface water volumes accessible to vegetation are to the first order controlled by the extent of root systems rather than soil depth in many parts of the world.

We also acknowledge the reviewer's point that the catchments in this study are primarily cool and energy-limited. The S_R parameter may indeed exhibit more dynamic behaviour in lowland areas, as shown by Bouaziz (2022) in the Meuse basin. This basin, located in NW-Europe, represents a hydro-climatically distinct region with an out-of-phase relationship between seasonal precipitation and energy signals. It experiences a winter rainfall regime, with peak flows in winter and low flows in summer.

In contrast, the alpine catchments in our current study feature a snow-dominated regime where the seasonal water supply, driven by snowmelt, glacier melt, and rainfall, is more in-phase with the energy signal, resulting in peak flows in summer and low flows in winter.

These contrasting characteristics, along with the vulnerability of snow-dominated alpine catchments to climate change, underscore our motivation for extending Bouaziz's methodology to this region. We will further emphasize this rationale in the revised manuscript.

Comment:

Table 1: Is there any soil information to be included here?

Reply:

We thank the reviewer for the for the suggestion. We will update Table 1 with underpinning soil information (e.g. soil texture fractions) in line with the SoilGrids250m dataset.

Comment:

Figure 2: is a well-designed graphical abstract of the envisaged methodology. The indication of the colour schemes used is also very useful here. At the size presented in the submitted manuscript I don't see the reason for keeping the text at such small fonts. A couple of more points everywhere would ease the reader.

Reply:

Agreed. We will update this in the final manuscript.

Comment:

148 ff: Considering the large number of assumptions that are declared in the 5 steps of the methodology, wouldn't be useful to introduce some very basic benchmarks such as prescribed increase or decrease of S_R between the current and future time slice?

Reply:

We thank the reviewer for the for this suggestion. We indeed agree with the reviewer that adding a sensitivity analysis would add value to our analysis. To show the sensitivity of the model towards the S_R parameter, we will perform a targeted sensitivity analysis. Using the calibrated parameter sets and all other parameters held constant, we will test 15 distinct values for a catchment average S_R (i.e., 50, 100, 150, 200, 250, 300, 350,...750), to introduce a high-contrast range in S_R values. To maintain consistency, the S_R values for the four landscape classes (i.e. vegetation types) will be scaled so that the catchment average S_R remains unchanged. Using this "manipulated" parameter set we will rerun the model and assess the model's sensitivity to varying levels of S_R under present conditions.

Comment:

206 – 215: When you introduce the concept of supply and demand limited systems (also know as water and energy limited systems), wouldn't be beneficial to elaborate on the blue and green water paradox in the Alps? Cfr. Mastrotheodoros et al (2020).

Reply:

We agree with the reviewer that addressing the Green-Blue Water Paradox would enhance the narrative by providing a tangible explanation of supply and demand-limited systems in the Alpine context. We will incorporate this in the final manuscript.

Comment:

Figure 3: This Figure is presented in the Methodology but has also some results. As ω is fixed, we see no scatter in how the six basins respond to future climate. Have you done this exercise with past data (e.g. 1961-1990 vs 1991-2020) to see if such projections are realistic? (e.g. if ω is constant).

Reply:

Thank you for the suggestion. Several recent studies, have demonstrated that, while ω cannot be assumed to be strictly constant over time, its temporal fluctuations with climatic variability are very minor in the vast majority of regions world-wide (e.g. Ibrahim et al., 2024; Tempel et al., 2024; Wang et al., 2024). Based on the results of these studies, we have therefore here not explicitly analyzed the validity of a fixed omega in a changing climate.. We will clarify that in the revised manuscript.

Comment:

*247 – 248**:** How did you partition for different vegetation types? Or did you just miss to refer to S2 also here?*

Reply:

We agree with the reviewer that this is not well explained in this section of the manuscript.

The results of previous studies (e.g., Wang-Erlandsson et al., 2016) suggest that different vegetation types adapt their root zones to bridge droughts of specific return periods. For instance, riparian vegetation, grasslands, and forests are assumed to adapt to withstand droughts with return periods of 2, 2, and 20 years, respectively. This approach allows us to distinguish between vegetation types based on their respective drought resilience.

As the Sr values from the water balance method represent the average catchment root zone storage capacity, we scale these values according to the proportional coverage of each vegetation type (i.e. HRU) in the catchment. We will clarify this in the final manuscript.

Comment:

251: When you speak of observed and modelled past, I think you mean "past obtained from simulations with observed data" and "past obtained with data from GCM/RCM". If I am wrong, please explain me if I am correct, please state it clearly somewhere.

Reply:

We thank the reviewer for pointing out this ambiguity. The reviewer is correct in his interpretation. When we refer to "observed past," we mean past conditions derived from simulations using observed climate data. On the other hand, "modelled past" refers to historical conditions generated using General Circulation Models (GCMs) or Regional Climate Models (RCMs). We will revise the manuscript to clearly define these terms to avoid any confusion.

Comment:

Figure 4: *The violins show especially in RCP85 a clustering of the outcomes in to two families. Does this relate to specific GMCs or RMCs?*

Reply:

We thank the reviewer for this valuable observation. It is indeed plausible that the observed clustering in outcomes is related to the variations among the RCMs used in the study. Although the analysis includes 14 RCMs, these models originate from only 5 distinct GCMs, which likely introduces a degree of homogeneity among RCMs derived from the same GCM. Consequently, the differences within RCMs generated from a single GCM are expected to be less pronounced than those between RCMs derived from different GCM families. This may be attributable to factors such as the GCMs' distinct underlying assumptions, resolution, or sensitivity to climate forcing. To investigate this hypothesis, we will identify the specific GCMs and RCMs associated with each cluster and analyse their characteristics to clarify the sources of clustering. We appreciate this suggestion and will incorporate any relevant findings into the revised manuscript.

Comment:

Figure 5: I like This plot, which speaks a lot for plausibility of your approach, as you get decreasing S_R (and chance to survive) for forest in high altitude basins, while grassland seems to have more chance to survive. Is this an independent achievement from your procedure, or does this follow one of several constraints you defined? (lines 269-271)

Reply:

We thank the reviewer for the insightful comment. We agree that this aspect was not thoroughly explained in the current manuscript, and we appreciate the opportunity to clarify further.

The observed decrease in $S_{R,cllim}$ for forests, relative to the greater resilience observed in grasslands, is attributed to our modeling approach rather than to predefined constraints. The initial constraints were used solely for model calibration, affecting $S_{R,cal}$ parameter. After calibration, $S_{R,cal}$ was replaced by $S_{R,clim}$, which is derived using the water balance method. This method incorporates distinct return periods for different vegetation types and accounts for their areal distribution. Consequently, the results presented in Figure 5 are driven primarily by energy limitations within the catchments, which restrict root expansion, rather than by the initial constraints. We will clarify this in the revised manuscript.

Comment:

Figure 7: *Did I miss a discussion on the cause of larger spread of "Modelled S_R_,clim" in the Pitztal? Glacier I guess?*

Reply:

We thank the reviewer for the insightful observation. From figure 7 it is evident that the Pitztal exhibits the largest spread in modeled streamflow simulations, both when using $S_{R,cal}$ and $S_{r,clim}$ parameters.

We agree that the larger spread observed is likely linked to the glacier presence in the Pitztal. In glacier-fed catchments, meltwater contributions have a strong influence on streamflow, and these contributions depend on factors such as temperature, seasonal fluctuations, and glacier dynamics. Modeling these complex dynamics introduces additional variability. In this study, glacial area changes are represented through linear interpolation of observed outlines from 1997 to 2006, with extrapolation to estimate glacier areas up to 2015. Melt is subsequently modeled using a degree-day method (Hanus, 2021). Including these additional parameters in the model inherently adds variability, contributing to a broader spread in both $S_{r,clim}$ and $S_{r,cal}$ simulations.

The difference in spread between the $S_{r,clim}$ and $S_{r,cal}$ model runs can be further explained by the significant narrowing down of the S_R parameter range from calibration-based values to climate-based ones (Figure S5). The significantly lower $S_{R,clim}$ values represent a reduced capacity of the soil's to retain water. In glacier-dominated catchments, where snow and ice melt are primary sources of streamflow, this reduced retention capacity means that meltwater rapidly translates into runoff without sufficient soil storage. This lack of buffering amplifies fluctuations in streamflow, leading to larger extremes and increased variability between different model runs. Thus, the limited soil storage capacity in $S_{R,clim}$ heightens the model's sensitivity to climatic variations, causing greater discrepancies in streamflow simulations.

We agree that we have not highlighted this aspect sufficiently in the current manuscript and will revise this in the next iteration.

Comment:

The whole analysis of signatures is solid and follows very closely the Hanus et al. (2021) pattern. I see here potential for shortening the paper. Instead of replicating all these analyses, I would prefer you explore the sensitity of S_R with more simple benchmarks as I suggest for lines 148 ff.

Reply:

We acknowledge that some sections of the manuscript are overly lengthy and complex. We will condense these parts to enhance readability, while retaining key insights from Hanus et al. to maintain the manuscript's coherence and self-sufficiency.

Additionally, we will implement the sensitivity analysis, as described above, to further strengthen the study's robustness.

Comment:

539 – 552: I like this “Broader implication section”. As far as the catchment selection part is concerned, it is a pity that the study only concentrated on the Hanus et al. basins, all of them being humid and energy limited. So this counts also as limitation

Reply:

Agreed. We will include this in the revised manuscript.

Comment:

Final considerations: I like the organization and focus of this study. Having focus and analysed strongly connected with Hanus et al. (2021) is in first sight a good idea, but when looking at the results and analyses. I really considered a missed chance not including water limited catchments here. I fear that a follow-up study with addition of such basins would also be “jeopardized” by this “in between study”. My honest request is here major revision with addition of ~6 water limited basins. Another option would the to keep it in this form but adding some simple S_R scenarios to test the sensitivity against your sophisticated approach.

Reply:

We thank the reviewer for the insightful and constructive feedback! In principle, we agree that including water-limited catchments could enhance the analysis. However, we deliberately chose to perform this study in cool, energy-limited, to explore the method as proposed by Bouaziz et al. (2022) in a different climate. To address the reviewers’ concern, we will add a sensitivity analysis using a high-contrast range of S_R values to compare with our current results. This will allow us simultaneously address concerns regarding model sensitivity, while preserving the focus on the effect of future changes in root zone storage capacity on streamflow.

References:

Bouaziz, L. J. E., Aalbers, E. E., Weerts, A. H., Hegnauer, M., Buiteveld, H., Lammersen, R., Stam, J., Sprokkereef, E., Savenije, H. H. G., & Hrachowitz, M. (2022). Ecosystem adaptation to climate change: The sensitivity of hydrological predictions to time-dynamic model parameters. *Hydrology and Earth System Sciences*, *26*(5), 1295–1318. https://doi.org/10.5194/hess-26-1295-2022

Gao, H., Fenicia, F., & Savenije, H. H. G. (2023). HESS Opinions: Are soils overrated in hydrology? *Hydrology and Earth System Sciences*, *27*(14), 2607–2620. https://doi.org/10.5194/hess-27-2607-2023

Hanus, S., Hrachowitz, M., Zekollari, H., Schoups, G., Vizcaino, M., & Kaitna, R. (2021). Future changes in annual, seasonal and monthly runoff signatures in contrasting Alpine catchments in Austria. *Hydrology and Earth System Sciences*, *25*(6), 3429–3453. https://doi.org/10.5194/hess-25-3429-2021

Ibrahim, M., Coenders-Gerrits, M., Van Der Ent, R., & Hrachowitz, M. (n.d.). *Catchments do not strictly follow Budyko curves over multiple decades but deviations are minor and predictable*. https://doi.org/10.5194/hess-2024-120

Mastrotheodoros, T., Pappas, C., Molnar, P., Burlando, P., Manoli, G., Parajka, J., Rigon, R., Szeles, B., Bottazzi, M., Hadjidoukas, P., & Fatichi, S. (2020). More green and less blue water in the Alps during warmer summers. *Nature Climate Change*, *10*(2), 155–161. https://doi.org/10.1038/s41558-019-0676-5

McCormick, E. L., Dralle, D. N., Hahm, W. J., Tune, A. K., Schmidt, L. M., Chadwick, K. D., & Rempe, D. M. (2021). Widespread woody plant use of water stored in bedrock. *Nature*, *597*(7875), 225–229. https://doi.org/10.1038/s41586-021-03761-3

Speich, M. J. R., Lischke, H., & Zappa, M. (2018). Testing an optimality-based model of rooting zone water storage capacity in temperate forests. *Hydrology and Earth System Sciences*, *22*(7), 4097–4124. https://doi.org/10.5194/hess-22-4097-2018

Speich, M. J. R., Zappa, M., Scherstjanoi, M., & Lischke, H. (2020). FORests and HYdrology under Climate Change in Switzerland v1.0: A spatially distributed model combining hydrology and forest dynamics. *Geoscientific Model Development*, *13*(2), 537–564. https://doi.org/10.5194/gmd-13-537-2020

Tempel, N., Bouaziz, L., Taormina, R., Van Noppen, E., Stam, J., Sprokkereef, E., & Hrachowitz, M. (n.d.). *Vegetation Response to Climatic Variability: Implications for Root Zone Storage and Streamflow Predictions*. https://doi.org/10.5194/egusphere-2024-115

Wang, S., Hrachowitz, M., & Schoups, G. (n.d.). *Multi-decadal fluctuations in root zone storage capacity through vegetation adaptation to hydro-climatic variability has minor effects on the hydrological response in the Neckar basin, Germany*. https://doi.org/10.5194/hess-2024-62

Wang-Erlandsson, L., Bastiaanssen, W. G. M., Gao, H., Jägermeyr, J., Senay, G. B., Van Dijk, A. I. J. M., Guerschman, J. P., Keys, P. W., Gordon, L. J., & Savenije, H. H. G. (2016). Global root zone storage capacity from satellite-based evaporation. *Hydrology and Earth System Sciences*, *20*(4), 1459–1481. https://doi.org/10.5194/hess-20-1459-2016