Reviewer #1

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 critical, yet very constructive and insightful comments. Below we provide clarifications in detailed replies to all comments.

Comment:

Ponds et al. explore how a non-stationary model parameter (root zone storage capacity) will impact streamflow in humid (energy-limited) catchments under future climate scenarios. My understanding is that previous work from the coauthors has used the same model and the same data to explore the impact of future climate scenarios in the same catchments (Bouaziz et al. and Hanus et al.). Thus, this contribution's impact hinges on what we learn about nature/how the world works by exploring the implications for streamflow of changing a single parameter (S_R) in an existing hydrological model in a particular climate type.

Reply:

We realize that the description of how our study is different to previous ones has not been sufficiently clear in the original manuscript. Therefore we would like to clarify two points.

Firstly, the reviewer is right in assuming that our previous work in Hanus et al. (2021) has explored effects of a changing climate on streamflow, based using the same model in the same study catchments in the Central Alps. The critical difference in that earlier study was that it did not account for potential changes in the role of vegetation. As such the predictions of Hanus et al. (2021) were based on a stationary model parameter of root zone storage capacity S_r, as is currently still common practice in the vast majority of studies that aim to predict the climate change effects on hydrology (see also comment/reply below).

In contrast, Bouaziz et al. (2022) investigated potential effects of climate change on S_r in the Meuse basin, a hydro-climatically substantially different region in NW-Europe. While also an energy-limited basin, it is characterized by seasonal precipitation and energy (i.e. E_P) signals whose amplitudes are out-of-phase: a winter rain regime, in which with the highest flows in winter and the lowest in summer. The alpine region of the current study, instead, is a snow-dominated regime, where the amplitude of seasonal water supply, consisting of snow melt, glacier melt and rainfall is much more in-phase with the seasonal energy signal and where high flows occur in the summer and low flows in the winter months.

Secondly, while for essentially all other model parameters it is at this point problematic if not at all impossible to quantify their future changes in a meaningful way for catchment-scale applications, Bouaziz et al. (2022) further outlined a method for future estimates of the root zone storage capacity. Leveraging the potential of this method in the current manuscript, we indeed analyse the "*implications for streamflow of changing a single parameter*", which is actually the only catchment-scale (or "effective") parameter for which we can at the present do so in a systematic and plausible way.

Comment:

I am not as familiar with the large literature on process-based hydrologic models, but while reading I was left with the impression that surely this knob must have been turned in prior studies? Could the authors please make explicit the novelty of exploring the impacts of changing this parameter, and summarize previous works that have done so (or state that it has indeed never been done)?

Reply:

There are some studies that have analysed time-variable, catchment-scale model parameters *ex-post* by time dynamic calibration over multiple past periods (e.g. Wagner et al., 2003; de Vos et al., 2010; Merz et al., 2011; Stephens et al., 2019) or by linking parameters to past time-series of remote sensing data, as was for example done by Duethmann et al. (2020) who used temporal variability in remotely sensed NDVI signals to correspondingly dynamically adjust the vegetation surface resistance parameter in their model.

However, an *ex-ante* and thus forward extrapolation of catchment-scale parameters remains challenging. This is foremost due to a lack of robust mechanistic or statistical relationships between catchment characteristics, their evolution over time and model parameters as, amongst others, pointed out by Wagener (2007), Fatichi et al. (2016) or Stephens et al. (2021). In other words, even if we knew (which we rarely do) how certain catchment characteristics, for example vegetation composition, will change, we still have insufficient means to quantify how this will affect model parameters. Note, that existing spatial parameter regionalization schemes that seek to identify relationships between catchment characteristics and parameters largely rely on static catchment attributes (e.g. soil types) or past time series of some observed variables (see also above; e.g. LAI/NDVI; Samaniego et al., 2010), making them unsuitable for future estimations.

For that reason, the vast majority of studies that use catchment-scale models to analyse the effects of climate change do so by using either (a) stationary model parameters or (b) different scenarios of how catchment characteristics, for example vegetation composition will change, based on a sensitivity analysis with *ad hoc* assumptions of how these changes may affect model parameters (e.g. Bormann et al., 2007; Huisman et al., 2009; Bulygina et al., 2012; Guimberteau et al., 2017; Pechlivanidis et al., 2017; Gaur et al., 2021; Padulano et al., 2021). This lack of a systematic, time-variable adaptation of catchment-scale parameters applies not only to hydrological models but also to the vast majority of land surface schemes of climate models as recently pointed out by van Oorschot et al. (2021, 2023).

The methodology outlined by Bouaziz et al. (2022), argues that parametric formulations of the Budyko framework (e.g. Tixeront, 1964) constitute the rare case of a robust semi-empirical relationship, strongly supported by mechanistic reasoning (e.g. Porporato et al., 2004; Gentine et al., 2012), that can facilitate the estimation of future changes to a catchment-scale model parameter, i.e. the root zone storage capacity.

The novelty of our study is that it is the first analysis to systematically estimate future adaptations of catchment-scale root zone storage capacity as model parameter based on that methodology and to quantify the cascading effect thereof on future predictions of streamflow in an alpine environment.

We will further clarify this in the revised manuscript.

Comment:

Many previous works have explored the interactions between root zone storage capacity, aridity, and water partitioning (e.g., Porporato et al., 2004, and references cited therein). These studies have emphasized the distinct dynamics that are likely to occur under arid vs. humid regimes. I think that the restriction of this contribution to a narrow range of aridity (humid, energy-limited environments) limits the scope of the findings and usefulness of the study, and suggest that the impact of the paper would be significantly greater if a diversity of climate regimes were explored.

Reply:

We completely agree that the iconic work of Porporato et al. (2004) and subsequent studies provide insightful descriptions of the differences between different types of environments. While we agree that an exploration of the issue across a wide spectrum of environments can provide a wider perspective, this would be an entirely different analysis, which would necessarily come at the price of much less detail. The Alps being the major source of water for much of central Europe, we deliberately chose to confine our study to this regional scale. This allowed us to analyse climate effects on hydrology in a more comprehensive way and from different aspects, including seasonal water supply as well as timing and magnitudes of extremes (i.e. floods and low flows), all of which play different and sometimes even contrasting roles for developing efficient future water resources management strategies in the region.

We will explain our choice in more detail in the revised manuscript.

Comment:

The paper is very long and complicated for what it is - the exploration of how a parameter changed in a model compares to previously published work with the same model. I would suggest that the authors consider ways to simplify and shorten the work where possible.

Reply:

We agree that some sections in the manuscript are lengthy and unnecessarily convoluted. We will shorten these parts and make the manuscript more easily readable.

However, we also want to reiterate that this study does not only analyse "*how a parameter changed in a model compares to previously published work*". Instead it is less about how the parameter changed (p.16 in the original manuscript) but rather about the consequences of that change (p.17-26). Using the outline of Bouaziz et al. (2022), the study is, to our knowledge, the first one to systematically infer future changes of a model parameter based on a robust relationship (i.e. the Budyko framework) and to quantify the effects thereof on future streamflow.

We will further clarify that in the revised manuscript.

Comment:

The study adopts the assumption that roots will be able to grow as much or as little as needed to obtain water to overcome droughts of a particular recurrence interval, without consideration to how substrate may limit rooting. The studied include glaciers (and presumably, large expanses of *exposed, relatively fresh bedrock, whose area grows under the future climate scenarios in which warming has resulted in glacier retreat). It is not clear that this assumption is realistic for the study catchments.*

Reply:

Two of the study catchments indeed include glaciers, accounting for 1.5 and 18% of the respective catchment areas (Table 1 in the original manuscript). The glaciers are represented in the model HRU referred to as “Bare Rock/Sparsely vegetated” (Supplementary Material Fig.S1). The past and estimated future glacier retreat, as described in Section 2.2.1 in the original manuscript, is accounted for by changing areal fractions over time as described by variable A_{Gl} [-] (Supplementary Material Tab.S1). The fraction of the HRU “Bare Rock/Sparsely vegetated” that is covered by glacier (A_{Gl}) is assumed to have a perennial snow pack that allows continuous melt on days with temperatures exceeding the threshold temperature T_{Thres}, thereby assuming a *de facto* “infinite” snow water storage. The fraction not covered by glacier ($1-A_{Gl}$) can only generate snow melt as long as a seasonal/transient snow pack is present.

In absence of significant vegetation, this bare rock HRU is characterized by very low root zone storage capacities $S_{R,bare} < 10$ mm that also include surface/rock interception. Ranges and best parameter values (3000 per catchment) per HRU are displayed respectively in Figure 1 below.

Please note that for the bare storage capacity parameter, no adjustments are made from the calibration, which is why the subscript remains $S_{R,cal,bare}$. In contrast, the other parameters are recalculated using the memory method, represented as $S_{R,clim,....}$

We will clarify all of the above in the revised manuscript.

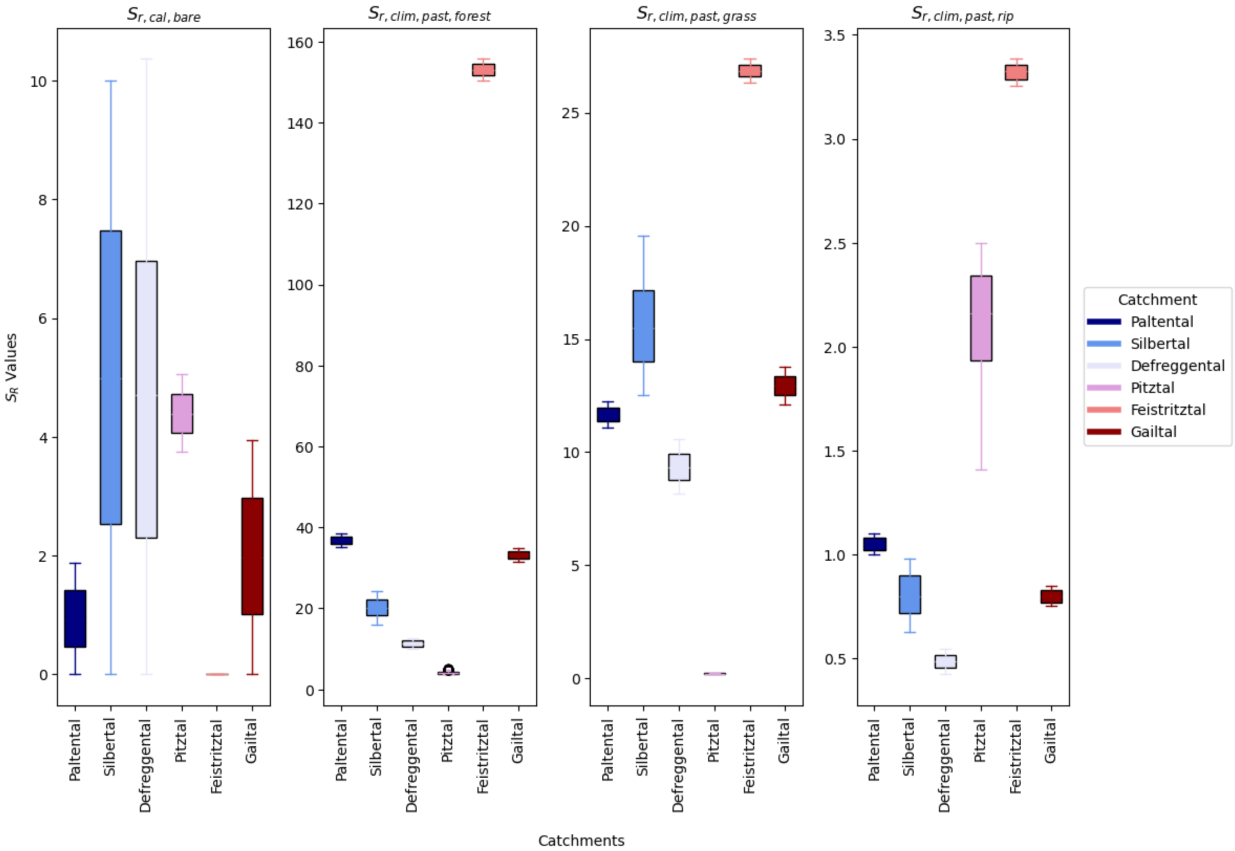

Figure 1- Overview of 3000 best parameter ranges by HRU by catchment. Please note: axis vary by HRU.

<u>*Comment:*</u>

There is a strong coupling between the omega value in the Budyko framework and the root-zone water storage capacity, which has been explored extensively in the literature (including by the references cited in this contribution). It was unclear to me how the assumption of a static omega under a future climate did not result in a circular or forced outcome when then determining how plants would 'resize' their root zone water storage capacities. I may have missed something fundamental - but by forcing the omega value to be the same, under a warmer (more arid) future, the future water partitioning is being forced as well under the Budyko framework. It is therefore unclear how what was being studied (the impact on streamflow under a future climate with a different storage capacity) was independent (not baked into) the methodology that forced streamflow to behave a certain way (according to Budyko, with a fixed omega value). Alternatively, if the outcome for streamflow is not already predetermined by assuming the fixed omega value, then another issue appears: how is the storage capacity allowed to independently evolve (if, as already established by previous studies, it is strongly coupled to the aridity index and evaporative index). In other words, if omega is a function of storage capacity, and you fix omega, how are you exploring a dynamic storage capacity?

Reply:

We completely agree with the reviewer on the strong coupling. Assuming validity of the Budyko framework, we know that long-term mean aridity is a first order control on long-term mean E_A/P (and thus on E_A and Q). Forcing a fixed omega does therefore indeed force long-term mean future water partitioning. However, while this holds for long-term averages, there is much weaker coupling at the shorter time scales that are the focus of our study. Little (if anything) about stream flow dynamics and associated the magnitudes and occurrences of floods and low flows can thus be inferred from that partitioning.

We similarly agree that omega is a function of root zone storage capacity. However, it is not *exclusively* a function of root zone storage capacity (and thus of E_A) but also of other factors, such as vegetation water use efficiency (e.g. Gentine et al., 2012). Thus following Bouaziz et al. (2022), the explicit assumption here is that fixing omega is equivalent to keeping these other factors constant, while increased E_A in more arid future conditions is sustained by increased root zone storage capacities.

We realize that we have not described and discussed this in sufficient detail in the original manuscript. We will add more detailed information and discuss the implications of the assumption in the revised manuscript.

Comment:

There is a crucial methodologic step that is described in one sentence but without sufficient detail to understand what was actually being done: "By implementing the long-term evaporative indices in the water balance equation, one Sr,clim,past and 28 estimates of Sr,clim,f ut are derived for each vegetation type". Please elaborate.

Reply:

We agree, that this was not well explained in the original manuscript.

The procedure follows several distinct steps:

Estimate past root zone storage capacity S_R

1. (1) Use observed past long-term means of time series of precipitation P and streamflow Q to estimate past long-term mean transpiration E_R (Eqs. 1-2). Note, that the total evaporative fluxes are combination of transpiration and interception evaporation, $E_A=E_R+E_I$
2. (2) Estimate time-series of past daily $E_R(t)$ by temporally redistributing the mean E_R according to the time series of daily E_P (or vice versa: rescale $E_P(t)$ so that the mean of the rescaled time series equals to mean E_R; Eq.3)
3. (3) Compute daily time series of storage deficits as $S_{R,D}(t) = \sum\left(P_E(t) - E_R(t)\right)$ and determine the highest deficit for each year as $S_{R,D,yr} = max\left(\left|S_{R,D}(t)\right|\right)$ (Eqs.4-5)
4. (4) Fit a GEV distribution to the set of highest annual deficits $S_{R,D,yr}$ and estimate the root zone storage capacity $S_{R,clim,past}$ as maximum annual deficit over a specific time interval (or return period; here 20 yrs for forest, 2 yrs for grassland)

To estimate future root zone storage capacity S_R, estimates of future transpiration E_R are needed. As no observations of future streamflow are available, the following procedure is adopted:

(5) Determine the past long-term mean catchment position defined by E_P/P and E_A/P in the Budyko framework and fit the associated parameter ω (Eq.6).
(6) With projected long-term mean future P, E_P and thus E_P/P and assuming a constant ω, long-term mean future E_A/P and thus E_A are estimated for each of the 28 future climate scenarios.
(7) With his long-term mean future estimate of P and E_A steps (1)-(4) are repeated to estimate $S_{R,clim,fut}$ for each of the 28 climate scenarios.

As pointed out by the reviewer, the assumption of a constant ω may not be fully warranted, due to the mutual interactions of aridity, seasonality and root zone storage capacity.

However, and to put this into perspective, the vast majority of studies up to now use the past $S_{r,clim,past}$ for any future climate impact analysis. This entails the strong assumption of a jump in ω, which may be associated to various factors such as changes in water use efficiency or vegetation density. For example, under more arid conditions (i.e. higher E_P/P), vegetation then transpires the same fraction of precipitation (i.e. E_A/P) than under previous more humid conditions. In that example, there is no fractional increase in transpiration, because vegetation, for example, does not need more water under more arid conditions ("water use efficiency") or there is less vegetation because individual plants died as they could not satisfy increased transpiration requirements due to insufficiently large root systems ("root zone storage capacity"). Yet, this is largely not what we are seeing in the Budyko framework, where long-term mean E_A/P generally increases with increases in long-term means of E_P/P. Although over time catchments do not strictly follow their specific curves defined by ω (e.g. Reaver et al., 2022), several recent studies have shown that the deviations from their curves (and thus changes in ω) over time remain very limited (e.g. Ibrahim et al., 2024; Tempel et al., 2024; Wang et al., 2024)

We will clarify this in the revised manuscript.

Comment:

(6) The model employed has so many free parameters and processes and things involved (different land cover classes behaving differently, e.g.), that the suspicion arises of whether we can expect to actually isolate the desired impact on streamflow of the term of interest (S_R). Can the authors convince the reader that the other numerous features of the model are not drowning out a signal?

Reply:

Indeed, elevated degrees of freedom, related to high numbers of parameters, do pose a challenge to distil meaningful signals in models. In a deliberate decision to limit this problem, the model was, adopting a multi-objective strategy, simultaneously calibrated to 8 distinct objective functions, thereby forcing the model to simultaneously reproduce 8 complementary signatures of the hydrological response. This approach is very effective in reducing false positives (i.e. parameter sets falsely accepted as feasible; e.g. Gupta et al., 2008; Efstratiadis and Koutsoyiannis, 2010; Hrachowitz et al., 2014) and the risk of "getting the right answers for the wrong reasons" (Kirchner, 2006).

Overall, the model showed good skill to simultaneously reproduce all 8 signatures in all study catchments as shown in Figure S6 in the Supplementary Material.

We will clarify that in the revised manuscript.

Comment:

Line 59 - missing reference. also on line 251

Reply:

This will be corrected.

Comment:

Line 80-81. It is important to emphasize that deficit-based approaches can only constrain (i.e., provide a lower bound or minimum estimate) on S_R. For example, the deficit may be quite large in a dry year, and small in the following year if it is particularly rainy. That doesn't mean that the root zone changed size over the span of one year. Only that a certain amount was detected.

Reply:

This is indeed a delicate issue that we will explain in more detail. The reviewer is correct in stating that for any individual year the annual storage deficit is a mere lower bound of the necessary root zone storage capacity. For that reason we estimate the root zone storage capacity for higher dry spell return periods (i.e. 20 years for forest) as has been shown to be suitable for many different environments (e.g. Wang-Erlandsson et al., 2016). Evidence from optimality-based studies provide further evidence that vegetation does not dimension its root-systems much larger than that so that instead it can balance below-ground resource investment with above-ground growth that is needed in competition for light (e.g. Schymanski et al., 2008; Guswa, 2008).

Comment:

There seems to be a lot of description concern about the impact of interception, followed by a decision to assign transpiration to be equal to all of ET. The paper could be simplified here.

Reply:

Agreed. We will adjust that in the revised manuscript.

Comment:

Eq. 3 effectively forces the system to be energy limited by scaling ET with PET. Is it not the case that water limitation ever occurs at any time of the year?

Reply:

This is a very sharp observation. The reviewer is right, that here we have assumed energy limitation. We think this simplifying assumption is justified in the snow/glacier-melt dominated regime of the Alps, where early summer melt but also abundant summer rain coincide with energy supply, i.e. water and energy supply are in-phase, so that during times of largest atmospheric water demand also most water is available. This reduces the occurrence of water-limited conditions.

Comment:

Eqs. 4 and 5 – why cast the deficit as negative? Confusing and a departure from most of the rest of the literature (e.g., Wang Erlandsson et al. 2016)

Reply:

Agreed. We will adjust that in the revised manuscript.

Comment:

Eq. 5 Is this equation correct? Or should it just be the minimum of the annual values – not their summation.

Reply:

Agreed. This will be corrected.

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