Author’s Response to Referee Comments by Giulia Mazzotti

We sincerely thank Giulia Mazzotti for her careful and constructive review of our manuscript. We greatly appreciate the time and effort devoted to the review and the thoughtful suggestions provided. We agree with the points raised and believe that addressing them will improve the clarity, presentation, and overall quality of our work. We respond to each comment point by point below and, should we be invited to submit a revised version, intend to incorporate the suggested changes accordingly.

General Assessment: The study by Seelig et al. presents hydrological modelling for 5 study areas (15 nested catchments) in the Austrian Alps and several climate change scenarios in order to characterize the impacts of on hydrological regimes, with a particular attention to the role of catchment storage characteristics. It involves an impressive amount of model simulations and analyses, with results nicely synthesized in very elaborate figures (both in the main article and the supplementary material), and will doubtlessly make a worthy contribution to the literature. The challenge lies in digesting the amount of information presented to carve out the key message of the article – I often struggled with keeping track of the storyline while jumping back and forth between text, figures, and supplementary material. I therefore have a few suggestions for revisions, some of which aim at better highlighting the novelty of the article / guiding the reader, while most others are simple requests for clarifications that I believe could improve the readability of the manuscript. Thank you for the opportunity to review this interesting work, and please don’t hesitate to reach out in case of questions (giulia.mazzotti@inrae.fr).

Response: We sincerely thank you for your positive assessment of our study and for recognizing the effort that went into the modeling and analyses. We also greatly appreciate your constructive suggestions on how to better highlight the key messages and guide the reader through the results. Your comments provide very helpful direction for improving the clarity and storyline of the manuscript. We are also particularly grateful for your kind offer to reach out with questions during the revision process, and we very much appreciate your openness and willingness to discuss any points that may arise. Below, we provide detailed point-by-point responses to your individual comments.

Major Comment 1: Are rock glaciers intended to be a focus of the study?

This was a recurring question while reading the manuscript. It first came up in the introduction (a paragraph dedicated to rock glaciers but no mention of rock glaciers in the paragraph presenting the study objectives), then again in the methods (rock glacier monitoring

and use for calibration is mentioned, but how exactly it is integrated in the rest of the model setup procedure remains unclear), and in the results and discussion section (all figures specifically relating to rock glaciers are in the supplement). I think explicitly clarifying whether this is a focus and better incorporating the paragraphs and analyses related to rock glaciers into the rest of the storyline would greatly help the narrative of the manuscript. I tried to comment on specific instances that relate to this issue in the line-to-line comments below (I apologize for redundancy)

Response: We thank you for this thoughtful suggestion. We agree that the role of rock glaciers in the study is not sufficiently clear in the current manuscript, and that making this role more explicit substantially improves the storyline.

In our study, rock glaciers serve as a proxy for alpine porous aquifers. We focus on them because they are known to provide considerable subsurface storage capacity in many high-mountain catchments (e.g., Jones et al., 2019; Hayashi, 2020), and because available inventory data across Austria (Wagner et al., 2020) allow us to systematically identify them using established regional mapping. This provides a consistent framework for cross-catchment comparison when evaluating how subsurface storage affects headwater hydrological responses.

Our broader objective is to investigate how hydrological responses evolve along the stream network within the context of the Austrian Alps, from springs and small headwater streams to downstream river sections. In this context, rock glaciers provide one type of alpine porous aquifer through which we can investigate the role of subsurface storage. They therefore serve as an important basis for our model setup, though the hydrological processes addressed by the study pertain to the wider headwater catchment.

We therefore agree that the current presentation does not sufficiently distinguish between the role of rock glaciers for our analysis and the broader, but still clearly defined, scope of the study. Should we be invited to submit a revised version, we intend to clarify this distinction throughout the manuscript, in particular by reducing the emphasis on rock glaciers as a separate topic in the Introduction and Methods and instead more clearly explaining their role within the overall study design as proxies for alpine porous aquifer storage. We also intend to add a discussion of the extent to which the insights obtained from rock glaciers may translate to other types of alpine porous aquifers. This will allow us to place our findings in the broader context of alpine subsurface storage while avoiding overstating the generality of our results.

We greatly appreciate your careful consideration of this aspect of the study and, in particular, your specific line-to-line comments on this issue. They provide valuable guidance for making the role of rock glaciers and the overall storyline much clearer and will help us improve the manuscript substantially.

References:

Jones, D. B.; Harrison, S.; Anderson, K.; Whalley, W. B. (2019): Rock glaciers and mountain hydrology. A review. Earth-Sci. Rev. 193, 66–90. DOI: 10.1016/j.earscirev.2019.04.001.

Hayashi, M. (2020): Alpine Hydrogeology. The Critical Role of Groundwater in Sourcing the Headwaters of the World. Ground Water 58 (4), 498–510. DOI: 10.1111/gwat.12965.

Wagner, T.; Pleschberger, R.; Kainz, S.; Ribis, M.; Kellerer-Pirklbauer, A.; Krainer, K.; Philippitsch, R.; Winkler, G. (2020): The first consistent inventory of rock glaciers and their hydrological catchments of the Austrian Alps. AJES 113 (1), 1–23. DOI: 10.17738/ajes.2020.0001.

Major Comment 2: Are rock glaciers representative of headwater catchments in general?

The high-altitude catchments chosen all have a high rock glacier coverage. It is unclear to what extent the conclusion drawn in this study would apply to headwater catchments without such a high rock glacier coverage as well; i.e., are the rock glacier areas here treated as a specific case or considered representative of high-alpine areas? This aspect could be discussed more explicitly (even just in the Conclusion section).

Response: We thank you for raising this important point. We agree that the extent to which our findings can be generalized to other headwater catchments should be made more explicit. We view rock glaciers as a specific, well-constrained example within the broader spectrum of alpine aquifers in headwater catchments, rather than as directly representative of all high-alpine catchments.

In our study design, we attempted to address this variability by selecting study sites and nested sub-catchments with widely varying rock glacier coverage (Table 1). However, we recognize that this aspect was not sufficiently emphasized in the manuscript.

Conceptually, our interpretation relies on the understanding that common alpine porous aquifers (e.g., talus, moraines, rock glaciers) share key hydrological functions, including high infiltration capacity, rapid response to melt/rainfall, and sustained baseflow release (e.g., Hayashi, 2020). While their storage capacity is small compared to lowland aquifers, these unconsolidated deposits often constitute the principal subsurface storage component in alpine headwaters, unless bedrock fracture or karst systems dominate (e.g., Somers and McKenzie, 2020). We intend to frame rock glaciers explicitly within this broader hydrogeological context.

In a revised manuscript, we plan to address this comment through the following revisions:

- Study Sites: Highlight the gradient of rock glacier coverage across our study catchments more clearly in Section 2.
- Discussion & Conclusions: Add a dedicated paragraph discussing how our findings apply to catchments dominated by other types of alpine porous aquifers and identify knowledge gaps and future research directions.

We greatly appreciate this suggestion, as it will help us better contextualize the results and more clearly communicate the scope and limitations of their generalization.

References:

Hayashi, M. (2020): Alpine Hydrogeology. The Critical Role of Groundwater in Sourcing the Headwaters of the World. Ground Water 58 (4), 498–510. DOI: 10.1111/gwat.12965.

Somers, L. D.; McKenzie, J. M. (2020): A review of groundwater in high mountain environments. WIREs Water 7 (6), e1475. DOI: 10.1002/wat2.1475.

Major Comment 3: Differences between catchments vs. differences along altitudinal gradients

I have the impression that the study highlights the variability in hydrological response along altitudinal gradients within the same catchments more than the variability between catchments. However, the choice between which figures are presented in the main article and which in the supplementary material somewhat contradict this. Only Figure 4 contrasts sub-catchments between the same study areas, while all other figures of the result section present results for the five study areas (which are often redundant). To interpret the results on the variability along altitudinal gradients (and the role of rock glaciers) one has to find one's way in the numerous figures of the supporting information. This massively disrupts the reading flow. This said, I do not want to suggest removing material from the supplementary material, there is definitely value in presenting the analysis results as comprehensively as it has been done in this case and I really commend the authors for this! But some figure choices in the main article could be revisited (e.g., make the existing figures slimmer by not showing all five catchments every time, and instead move some examples that now only figure in the supporting information to the main article). Again, I have tried to provide suggestions in the specific comments below, which may have caused some redundancy.

Response: We sincerely thank you for this very helpful and constructive comment. Your observation made us realize that the current selection and organization of figures does not sufficiently reflect the distinction between variability among catchments and variability along altitudinal gradients within the same catchments. We agree that the main text should guide the reader more effectively through the key findings, while the Supplementary Material can provide the more detailed results underlying these findings. We greatly appreciate you pointing this out, as this was not sufficiently apparent to us in the current presentation.

Should we be invited to submit a revised version, we intend to revisit the figure selection accordingly. We will consider reducing repetition across the five study areas in the main figures and bringing selected examples that are currently only presented in the Supplementary Material into the main article. The complete set of results will remain in the Supplementary Material to document the variability across catchments.

We sincerely thank you for this suggestion and for the specific guidance provided in your line-by-line comments. We agree that this will substantially improve the structure and readability of the Results section.

Major Comment 4: What are the implications of the methodological limitations of the study?

The study uses conceptual models that have limitations with regards to process representation. For example, a degree-day snow model may not accurately capture the snowpack response to rain-on-snow, and therefore, melt factors calibrated on current conditions may not appropriately represent future dynamics. Likewise, I think you are not accounting for changes in subsurface storage capacity as currently ice-containing rock glaciers become inactive? I think adding a paragraph on model limitations would be relevant (e.g. in 4.1?).

Response: We fully agree that the methodological limitations of our conceptual modelling approach and their implications for the interpretation of our results deserve more explicit consideration. We thank you for highlighting this aspect and for pointing out the potential implications for future hydrological dynamics.

In particular, we agree that the degree-day snow model has limitations in representing individual processes such as rain-on-snow events and changes in snowpack characteristics. Consequently, melt factors calibrated under current conditions may not fully capture how snowmelt dynamics could change under future climatic conditions. We also agree that our current approach does not explicitly account for potential changes in subsurface storage capacity associated with the transition of currently ice-containing rock glaciers towards relict states. This represents an inherent limitation when interpreting the longer-term hydrological implications of our results.

Should we be invited to submit a revised version, we intend to add a dedicated paragraph on these methodological limitations, as you suggest, and discuss more explicitly how they may affect the interpretation and transferability of our results. We will also clarify which aspects of the projected hydrological responses are robust within the framework of our models and which should be interpreted with greater caution. We greatly appreciate your suggestion, which will help us provide a more balanced assessment of the strengths and limitations of our approach.

Comment on L71: ‘Their distribution…’- I am not sure I understand what you are trying to say with this sentence, consider rephrasing.

Response: We thank you for pointing this out. We agree that the original sentence was ambiguous. By "their distribution", we intended to highlight the spatial occurrence of rock glaciers as a diagnostic indicator of coarse-grained subsurface storage across alpine

catchments. In the revised manuscript, we will rephrase this passage to state directly that mapped rock glaciers serve as indicators for the location and extent of shallow porous storage, without implying that mapping alone defines their detailed hydrological functioning. We appreciate your feedback, which helps us remove this ambiguity.

Comment on L84ff: Considering the importance of rock glaciers in the rest of the study, I find it somewhat surprising that they aren't mentioned in this paragraph. I recommend bringing up this aspect explicitly here, this will facilitate following the storyline later.

Response: We agree that, given the role of rock glaciers in our study, their absence from this paragraph makes the storyline less clear. We will therefore explicitly mention rock glaciers here and clarify their role as a particularly well-constrained example of alpine porous aquifers and subsurface storage within our investigation of headwater hydrology. Introducing this aspect earlier will make the connection to the subsequent analysis more transparent. We appreciate this helpful suggestion and your careful attention to the manuscript's overall storyline.

Comment on Section 2: The description of each site is very detailed and contains interesting information, but with 5 sites the details are very hard to retain. I wonder if a more concise description could better serve the paper. In practice, this could be achieved by moving the detailed information to the supplement, and instead introducing the sites all together by referring to Table 1 (no need to repeat the numbers in the text) and better highlighting how these sites differ. But this may be personal preference…

One detail on the sites: since some catchments have active rock glaciers, one is left wondering if they also have glaciers – which would impact streamflow and future scenarios. I found the answer in the supplement but I would state this explicitly in the manuscript.

Response: We agree that the site descriptions are overly detailed for the main text and that a more concise presentation would improve readability. We will therefore move the detailed site information to the Supplementary Material and provide a concise overview in the main text, referring to Table 1 and emphasizing the differences among the five study areas.

We also agree that the presence of glaciers should be stated explicitly. We will clarify that glaciers occur only in the Ötztal Alps study area, where they cover less than 1% of the catchment area, and will briefly address their (~negligible) influence on streamflow under current and future climate scenarios.

We appreciate these helpful suggestions, which will make the site description more focused and the hydrological context clearer.

Comment on L103ff: 'Drainage percentages refer to…' this sentence confused me. Is 'the combined area of the rock glaciers and their contributing catchments' the same thing as the 'total area'? In that case, I would write it like that and not mention drainage percentages here. But if the drainage percentages in fact relate to something different than the total area, you should make it clearer.

Response: We thank you for pointing out this ambiguity. We completely agree that the wording in the original draft was confusing and failed to clearly distinguish between these two spatial metrics.

To clarify the underlying distinction:

- Total catchment area (A): The complete drainage area upstream of the catchment outlet.
- Rock-glacier-drained area (A_{RG}): The specific subset of the catchment that drains through a rock glacier prior to reaching the outlet.

Because A_{RG} is smaller than A in most cases, the percentage represents the proportion of the total catchment area influenced by rock glacier storage.

In a revised version, we will take great care to clarify these definitions and eliminate any potential for confusion. Specifically, we will define both metrics explicitly in the caption of Table 1 and remove redundant text references to ensure the description in the main text remains clear, focused, and unambiguous.

Comment on L229: It is unclear what these 'five elevation bands' refer to, since afterwards it is stated that SCF is computed per grid cell. Is each basin subdivided into 5 elevation bands? Please specify.

Response: We agree that this was not sufficiently clear. Each basin is subdivided into five elevation bands, and SCF is calculated for each grid cell and then averaged across all grid cells within each elevation band. This is necessary because of the model structure of the employed snow routine (CemaNeige). The subdivision into five bands follows the recommendations of Valéry et al. (2014) and Riboust et al. (2019) for the specific model setup used in our study. We will clarify this procedure in the manuscript to avoid ambiguity. We thank you for highlighting this aspect and helping us make the methodological description more precise.

References:

Riboust, P.; Thirel, G.; Le Moine, N.; Ribstein, P. (2019): Revisiting a Simple Degree-Day Model for Integrating Satellite Data. Implementation of Swe-Sca Hystereses. Journal of Hydrology and Hydromechanics 67 (1), 70–81. DOI: 10.2478/johh-2018-0004.

Valéry, A.; Andréassian, V.; Perrin, C. (2014): 'As simple as possible but not simpler'. What is useful in a temperature-based snow-accounting routine? Part 2 – Sensitivity analysis of the Cemaneige snow accounting routine on 380 catchments. Journal of Hydrology 517 (12), 1176–1187. DOI: 10.1016/j.jhydrol.2014.04.058.

Comment on L234: Are the projections also available on a 1x1 km grid? And over one yearly cycle, or as full time series (if the latter: for which period)?

Response: Yes, the climate projections are available on the same 1 × 1 km grid as continuous daily time series covering the period 1970–2099. We ran our hydrological models continuously across this entire period for each projection. To account for interannual variability and focus on long-term hydroclimatic trends, we subsequently aggregated the output time series into representative "near-future" and "far-future" periods.

In the revised manuscript, we will state the spatial resolution, temporal extent, and output aggregation periods explicitly in Section 3 to ensure the setup of our modeling framework is completely transparent. We thank you for asking for this clarification.

Comment on L247ff: This paragraph feels a bit out of place here – the subsequent paragraph detailing the model is a much more logical continuation. The issue is that the paragraph addresses at the same time new observational datasets (while the other observational datasets were introduced in section 3.1) and one model calibration step (while the rest of the calibration is detailed at the end of section 3.2. This leaves some open questions as to how this calibration step fits within the rest of the calibration procedure? I think the information on the rock glacier spring monitoring should be moved to 3.1, and the entire calibration procedure should be introduced at once.

Response: Thank you for pointing this out. We fully agree with your assessment and will follow your suggested reorganization. Should we be invited to submit a revised version, we will move the rock glacier spring monitoring to Section 3.1 and present the complete calibration procedure together at the end of Section 3.2. We appreciate this helpful suggestion, which will make the structure of the Methods section more coherent.

Comment on L249: 'the model parameters' – which ones?

Response: Thank you for noting this unclear reference. We realize that "the model parameters" is introduced too early and is therefore unclear at this point in the manuscript. We intended to refer to all θ_1–θ_4 parameters of the snow model and all x_1–x_4 parameters of the hydrological model, which are transferred to ungauged rock glacier catchments using a first-

approximation transfer approach based on the assumption of hydrogeological similarity among rock glaciers within a given study area. We will clarify this explicitly when introducing the parameter transfer and define the parameters before referring to them.

More broadly, we recognize that this assumption is implicit in the overall study design, given our use of rock glaciers as proxies for alpine porous subsurface storage. As noted in our responses to your Major Comments, we agree that this conceptual basis and its boundaries must be made more transparent. In a revised manuscript, we will state this assumption explicitly in the Methods and thoroughly discuss its implications and limitations in the Discussion.

Comment on Figure 2: Could be moved to the supplement. Instead, Figure S10 would be useful in the main article.

Response: We agree and will follow your suggestion, should we be invited to submit a revised version. Thank you!

Comment on L279: Does this mean that two KGE values are computed and weighted (Table A1 reports only one value)? Or are you using something else for the snow-covered area? Please specify. Note: the title

Response: Yes, two KGE' values are computed and weighted in the calibration, with the resulting weighted average reported in Table A1. We will revise Table A1 to also report the individual KGE' values.

This calibration approach follows the methodology recommended by Riboust et al. (2019) for this specific model setup, which has been shown to improve overall model robustness. We appreciate this comment, as making this weighting explicit will improve the clarity of our methodology.

Reference:

Riboust, P.; Thirel, G.; Le Moine, N.; Ribstein, P. (2019): Revisiting a Simple Degree-Day Model for Integrating Satellite Data. Implementation of Swe-Sca Hystereses. Journal of Hydrology and Hydromechanics 67 (1), 70–81. DOI: 10.2478/johh-2018-0004.

Comment on Figures S11 etc: These figures are great and contain a lot of very valuable information. I think though that the information on what line colors correspond to is missing. Suggest to include legends in all the subpanels of Figure S11 (for the others it's obvious that the same apply)

Response: We agree and apologize for this oversight on our part and will add the corresponding legends to all subpanels of Figure S11, with the same convention applying to the other figures. We appreciate you drawing our attention to this omission.

Comment on Appendix A: I really appreciated the effort in making the parameter calibration accessible to the reader; the only thing that remains unclear to me is how the rock glacier springs data went into this, and since it seems like a considerable effort it would be worth specifying.

Response: We appreciate this observation and agree that the role of the rock glacier spring data in the calibration procedure should be explained more clearly. The springs and downstream rivers were modelled within the same modelling framework, following an approach that has been successfully applied across a range of alpine spring and river systems (e.g., Nepal et al., 2017; Wagner et al., 2016, 2021; Sezen et al., 2019; Ollivier et al., 2020; Bertolotti et al., 2026; Masten et al., 2026). This consistency was an important consideration in our choice of model structure.

A substantial effort was devoted to monitoring the rock glacier springs, and we agree that their contribution to the calibration deserves to be made more explicit. Should we be invited to submit a revised version, we will clarify these aspects and the underlying rationale. We sincerely thank you for highlighting this aspect and for recognizing the value of the spring monitoring effort.

References:

Bertolotti, G.; Vremec, M.; Seelig, S.; Krainer, K.; Wagner, T.; Fischer, A.; Winkler, G. (2026): Characterization of recharge components for delineation of a high-alpine rock glacier spring catchment, Ötztal Alps (northern Italy). Hydrogeol J 12 (10), 48. DOI: 10.1007/s10040-025-03005-y.

Masten, M.; Seelig, M.; Seelig, S.; Vremec, M.; Wagner, T.; Obwegs, M.; Hausleber, M.; Eybl, J.; Winkler, G. (2026): Climate change impacts on alpine springs in Austria. Shifts in discharge seasonality and water availability. Journal of Hydrology 679, 136337. DOI: 10.1016/j.jhydrol.2026.136337.

Nepal, S.; Chen, J.; Penton, D. J.; Neumann, L. E.; Zheng, H.; Wahid, S. (2017): Spatial GR4J conceptualization of the Tamor glaciated alpine catchment in Eastern Nepal. Evaluation of GR4JSG against streamflow and MODIS snow extent. Hydrol. Process. 31 (1), 51–68. DOI: 10.1002/hyp.10962.

Ollivier, C.; Mazzilli, N.; Olioso, A.; Chalikakis, K.; Carrière, S. D.; Danquigny, C.; Emblanch, C. (2020): Karst recharge-discharge semi distributed model to assess spatial variability of flows. The Science of the total environment 703, 134368. DOI: 10.1016/j.scitotenv.2019.134368.

Sezen, C.; Bezak, N.; Bai, Y.; Šraj, M. (2019): Hydrological modelling of karst catchment using lumped conceptual and data mining models. Journal of Hydrology 576 (10), 98–110. DOI: 10.1016/j.jhydrol.2019.06.036.

Wagner, T.; Pauritsch, M.; Winkler, G. (2016): Impact of relict rock glaciers on spring and stream flow of alpine watersheds. Examples of the Niedere Tauern Range, Eastern Alps (Austria). AJES 109 (1), 84–98. DOI: 10.17738/ajes.2016.0006.

Wagner, T.; Seelig, S.; Krainer, K.; Winkler, G. (2021): Storage-discharge characteristics of an active rock glacier catchment in the Innere Ölgrube, Austrian Alps. Hydrol. Process. 35 (5), e14210. DOI: 10.1002/hyp.14210.

Comment on Figure 3: In the seasonal plots, I struggle to distinguish between the saturated and faded colors for the area representing the uncertainty (i.e. I can't really make out which uncertainty range corresponds to which line) – consider using different colors instead?

Response: We agree and will adjust the color scheme to make the uncertainty ranges more clearly distinguishable from the corresponding lines. We appreciate this helpful suggestion and will address it in the revised figures.

Comment on L364: 'As evapotranspiration increases' – Is this a hypothesis, or does the data show that? I know it's not a focus of the study, but given how comprehensive the rest of the analysis is, you could consider providing these results as well if that doesn't require too much effort. I could see that being a useful reference for future studies because the subject is somewhat underexplored. * I found the data in the supplementary Figure S36. I suggest pointing the reader at this figure here already.

Response: We agree that the wording should clarify that the increase in evapotranspiration is a model result. While reference evapotranspiration is an input, actual evapotranspiration is simulated based on soil water availability. We will clarify this in the main text. We appreciate your helpful suggestion, which will make this aspect of the analysis clearer.

Comment on L370ff: This paragraph is key to the paper, but I am not sure that the key message comes across. It reads as if all the nested catchments behave similarly (more buffering of CC signal downstream than upstream) – while I understand they were chosen them to represent varying subsurface storage properties (not only from headwater to lowlands, but also between the headwaters of the different catchments). I therefore wonder: is the key message that the same CC impact is observed in the five catchments DESPITE their different

properties, or are there some differences in CC impacts that can be explained by different catchment characteristics? I feel this could be elaborated on more explicitly.

Moreover, is a bit confusing that the role of rock glaciers is addressed based on Figure 4, while the results specific to the rock glacier portion of the catchment are only shown in the Supplementary Material.

Response: We greatly appreciate this comment and agree that we have presented the results in a confusing way. The key message is that we consistently observe the same overall nested pattern, with stronger buffering of the climate-change signal downstream than upstream. However, the magnitude and timing of this dampening varies with catchment characteristics. Thus, the overall trend is consistent across the five study areas, while its strength differs among catchments. The point we tried to make here is how this buffering changes along the upstream-downstream pathway and how this interacts with catchment characteristics and changes in snow storage. We therefore intend to substantially revise this section to make this message much clearer.

We also agree that the current design of Figure 4 is not sufficiently transparent. Figures 4a and 4c show predictions for rock glacier springs (a relict rock glacier and an ice-containing rock glacier, respectively), while Figures 4b and 4d show the corresponding downstream rivers within the same study areas. We will revise the figure and its description to make these relationships explicit.

Thank you again for helping us identify a clearer and more logical way to structure this key part of the Results. Should we be invited to submit a revised manuscript, we will restructure this section and update the figure accordingly.

Comment on Figures 6, 7, 8: If I understand the caption correctly, the three grey boxplots within each subplot should be the same? There are some cases in which they don't seem to be (e.g. third row, third column), which is confusing. If they are the same, I suggest plotting them only once to avoid confusion and leave more space for the other boxes.

Response: Thank you for pointing this out. The three grey boxplots are expected to be similar, but not identical, because the individual climate projections also differ during the historical period. Each projection (including into the past) results from a different model run and is bias-corrected individually, leading to differences in their representation of the historical period. Comparing all projections against the same actually observed historical data would therefore introduce an additional bias. These differences need to be retained when comparing historical and future periods for each individual projection (e.g., Chimani et al., 2020; Haslinger et al., 2023).

We acknowledge that this aspect and its implications were not sufficiently explained in the manuscript. In the revised version, we will clarify and discuss the associated uncertainty. Specifically, we will compare model outputs driven by different climate scenarios against actual observed historical data to quantify this baseline uncertainty more rigorously, as

differences between historical simulations contribute to the spread of projected changes. We thank you for raising this point, which will help us make this aspect of our projection framework more transparent.

References:

Chimani, B.; Matulla, C.; Hiebl, J.; Schellander-Gorgas, T.; Maraun, D.; Mendlik, T.; Eitzinger, J.; Kubu, G.; Thaler, S. (2020): Compilation of a guideline providing comprehensive information on freely available climate change data and facilitating their efficient retrieval. Climate Services 19 (46), 100179. DOI: 10.1016/j.cliser.2020.100179.

Haslinger, K.; Schöner, W.; Abermann, J.; Laaha, G.; Andre, K.; Olefs, M.; Koch, R. (2023): Apparent contradiction in the projected climatic water balance for Austria. Wetter conditions on average versus higher probability of meteorological droughts. Natural Hazards and Earth System Sciences 23 (8), 2749–2768. DOI: 10.5194/nhess-23-2749-2023.

Comment on L411ff: This paragraph is interesting and it's a pity that the figures supporting this conclusion are all in the Supplementary Material. Consider moving e.g. one of the panels from Figure S37 to the main article so one can follow the reasoning without having to jump back and forth between article and SM…

Response: We agree and will follow your suggestion by moving a representative panel from Figure S37 to the main article. Thank you!

Comment on L466-467: Figures S39-S43 include 24 panels each – this is another place where the reader may get lost, consider presenting an example in the main article so that looking at the SM is not a necessity.

Response: We agree and will follow your suggestion. Thank you for this excellent suggestion!

Comment on Section 4.4: Same comment as above: multiple paragraphs refer to Figures S44-48 – including some of the information presented there in the main article (and more specifically pointing to the relevant subplots) would really help the reading flow.

Response: We agree and will incorporate examples into the main article, with clearer references to the corresponding subplots. We are grateful for this constructive suggestion and believe it will strengthen the presentation.

Comment on L565: What do you mean by ‘static baseline assumptions’?

Response: We agree that “static baseline assumptions” is too broad and vague and potentially misleading. We also acknowledge that the paragraph as a whole could be more specific in describing the limitations of our current modelling approach (as highlighted e.g. in Major Comment 4). We intended to highlight that our modelling does not account for the temporal evolution of catchment properties, such as permafrost thaw and associated changes in ground ice and subsurface storage, sediment redistribution, or changing land cover. As discussed in our responses to the comments above, we will revise this passage accordingly, should we be invited to submit a revised version. Thank you for raising this point, which will help us sharpen this aspect and address the limitations of our current study more clearly.

We would like to thank you again for your careful, constructive, and very helpful review of our manuscript. Your comments have helped us identify several aspects that require clearer explanation and presentation, and we believe that addressing these points will substantially improve the manuscript. We greatly appreciate the time and effort you have devoted to providing such detailed and thoughtful feedback, and we hope to have the opportunity to submit a revised version that addresses your suggestions accordingly.

Kind regards,

Simon Seelig

On behalf of all authors