

Reply to referee 2

We would like to thank you for your constructive comments and feedback on this manuscript. We think that the suggested revisions based on the Referee's comments will greatly improve the article. Please find our responses (in blue) to the main points raised (shown in black) below.

General comments

1. There are many grammatical and writing errors in the current version of the manuscript. I have highlighted some of them in the specific comments section of this work, but have not listed them comprehensively. I recommend the authors conduct a close reading to improve the clarity of the writing, even in places with no scientific comments.

→ Thank you and we will go through a language check.

2. I recommend that the authors re-structure the introduction. As it stands, it reads
 - the 2022 European/Italian alps drought
 - glacier shrinking in Europe
 - knowledge/modeling gap related to glacier modeling and glacier inclusion in hydrologic models
 - the 2022 drought again
 - what this study does

I would like to see a structure that more methodically frames the impact of this study, which I see as an accounting for the presence or lack of glacier buffering of droughts in several important drought years. To me, this would start with an explanation of glacier-driven hydrological buffering, then the argument that this may be lessening due to glacier shrinking, then that modeling is required to understand how much buffering occurs (and any technological gaps that might be related), then an introduction of the domain of this study (the 2022 Alps drought).

→ Thanks and we see the overlaps mentioned. We are going to restructure it based on the new structure here provided.

3. For the ICHYMOD-TOPMELT model: this seems to be an interesting and important application of this model. The discussion of the model does not make it clear which components were developed in previous work and which components are novel in this paper. Please make this more clear, which will also highlight the novelty of this work!

→ thank you, we see the need to make our novel additions clearer. We can stress this out modifying line 83 at the beginning of section 2.2.1.

4. Albedo calibration: There should be, at minimum, a sensitivity analysis performed on the albedo calibration, and more justification of choosing a singular albedo value. The resulting albedo value should be externally validated in some way (i.e., establish that the value is a reasonable value for glacier per the literature, observations, etc.) It seems that the results are moderately dependent on this value, but it is calibrated on only two individual observations and is held to be one singular value for all times of the year in all years. If such a calibration is required, all of these should be carefully justified. The choice to calibrate on albedo and not another parameter should also be justified.

→ thank you for this comment and we see the need to clarify this important process. In particular, we would like to highlight two main points:

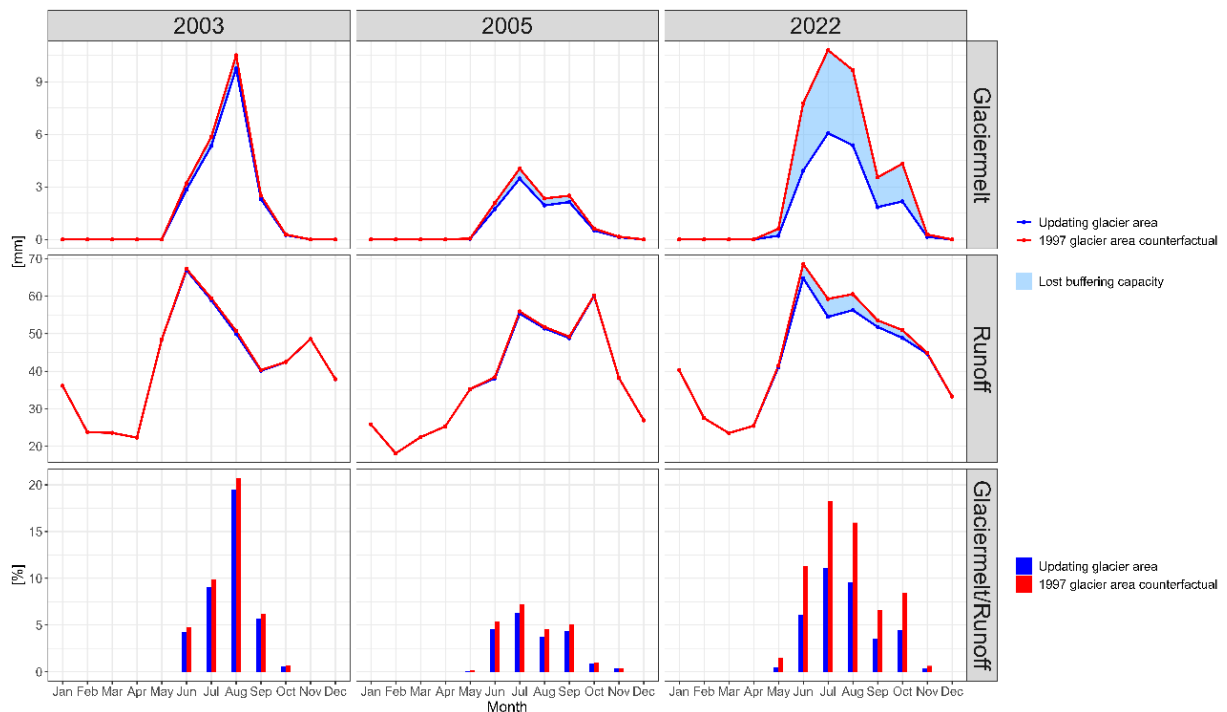
- We have already carried out a sensitivity analysis (line 184) considering different albedo values and we selected the one (i.e., 0.65) which was leading to glacier area and mass balance closest to the observed data: to the only three surveyed available glacier measurements in one case and to the Dussaillant dataset with a yearly estimate of glacier mass budget in the other (upper and lower panel in Fig. 2). We are aware that our calibration value of 0.65 is slightly higher than usual values used for ice and more similar to the values used for firn (snow one year old). However, the selected value falls within the range defined in other studies in similar areas (e.g., Richter et al., 2026) and lower than the fresh snow albedo value of 0.9 used in the model.
- The choice of calibrating only the glacier albedo parameter came from the type of model structure (Zaramella 2019). Indeed, several other parameters equally influence glacier melt dynamics, namely combined melt factor for combined radiation-temperature driven melt, rain melt factor and night melt factor, all handling ice melt in different conditions. However, these parameters are coupled for snow and ice melt by model design, effectively controlling the behavior of both storages at once. Therefore, the calibration effort focused on glacier albedo, the sole decoupled parameter in our framework. Although this parameter may vary in space and time we chose to explore a range of spatio-temporally fixed parameters since this would (i) substantially reduce the computational effort and (ii) avoid a clear risk of equifinality, given the very limited set of validation information at our disposal, compared to an almost endless set of potential space-time combinations of albedo values.

Overall, we can modify the methodology section 2.2.1 and 2.3 as well as the limitations in section 4.1 so to reinforce the two main points reported here above and clarify the conditions used to reconstruct glacier dynamics within the lumped hydrological model.

Richter, J.N., Arndt, A., Ban, N., Gampierakis, N., Maussion, F., Prinz, R., Scheiter, M., Umlauf, N., Nicholson, L., 2026. Towards the Bayesian calibration of a glacier surface energy balance model for unmonitored glaciers. <https://doi.org/10.5194/egusphere-2025-6249>

5. Focus on justifying all claims: The major point of this work is to quantify the role of glaciers in buffering/exacerbating hydrological droughts. I see opportunities to strengthen this argument with minimal analysis related to what you currently show and to focus more on this point than on a general accounting of drought conditions (which has been done in some of the literature in this region). Specifically, with your current counterfactual runs, can you present results on how much more buffering would have been provided if the glaciers had not retreated? I think that is the point of the counterfactual, but I do not see this result communicated very clearly. For example, there are currently 2 figures on the hydroclimate of drought, but the counterfactual, which I find more impactful, is a table that receives minimal discussion. What about a timeseries of streamflow showing the observation and then the counterfactual model? The discussion mostly focuses on the difference between dynamic and static, not on the use of the 1997 glacier area counterfactual, which I see as the more impactful result!

→ Thanks for the very useful comment. We can definitely expand the text on counterfactual and replacing the current table with a dedicated figure with info on runoff, glaciermelt and the glaciermelt contribution to runoff for the three major droughts considered (attached here below). Moreover, we can expand the part on counterfactual within the discussion.



Specific:

6. Use “significant” not “significant” and always include a statistical justification of “significant” when using. Otherwise, use another word, like “meaningful,” “observable,” or “large”

→ thanks we will correct it throughout the manuscript

7. L26: droughts (plural)

→ thanks, we will change it.

8. L28: using SWE acronym without first defining it

→ thanks, we are going to define it

9. L29-30: Sentence “By contrast...” is written unclearly, please rephrase

→ thanks, here is a rephrased sentence we are going to use in the revised version: “By contrast, the significant runoff deficit in 2005 was driven by colder temperatures, which reduced glacier melt contribution to 4.85%, combined with low SWE conditions.”

10. L31: maybe “in contrast to” instead of “differently”

→ thanks we can modify it accordingly

11. L34-35: be clear over what region you make this claim

→ we think having “the upper Adige River basin” in the sentence already provides a clear reference to the area of interest. In case this is not clear enough, we are available to hear specific suggestions so to improve this sentence.

12. L35: is the dependence showing a decreasing trend or are precipitation and snowmelt?
Rephrase to make this clearer

→ we meant that snowmelt is showing a decreasing trend. We can modify the sentence into “Glacier retreat is weakening the contribution of glaciers to summer flows, increasing the upper Adige River basin’s dependence on precipitation and snowmelt, the latter of which is also showing a decreasing trend”.

13. L51: grammar corrections to “39% of European glacier [is this count? mass? Area?] was lost”

→ thanks for pointing it out and we can add reference to the “glacier mass” in the sentence so to make it clearer.

14. L53: “recession” singular

→ thanks, we can modify it.

15. L56: grammar corrections

→ we can modify it as follow: “Yet, understanding glaciers’ contributions to buffering river streamflow during droughts remains limited and poses a major barrier to improving present and future water management.”

16. L73: This is the second portion of the introduction that introduces the 2022 drought – please restructure the introduction to have this information in one place

→ thanks, we can do it following comment n.2

17. L75: clarify if the Adige River Basin is part of the Italian Alps (and therefore an example of the broader context you are describing). Refer to figure 1 here

→ Thanks for pointing it, we will clarify that the upper Adige River Basin is part of the Italian Alps and add reference to Figure 1.

18. L111: give the streamflow gauges a density too? #/length of river?

→ we can add the value of density of (1 per 23 km of river) for the streamflow gauges for consistency

19. L112: citation or link, not hyperlink

→ we can add link in brackets: (<https://meteo.provincia.bz.it/it/home>)

20. Figure 1: the lake and glacier colors are extremely similar and the lakes and glaciers are quite small so it is difficult to tell them apart

→ thanks and we can make the glacier color brighter.

21. L140: should this be “lower, fixed albedo” ? -- how would a high and fixed albedo enhance absorption and accelerate melt?

→ thank you, this is a typo and we are going to change “higher” into “lower”.

22. L157: why is $n=3$?

→ The parameter $n = 3$ is the exponent of Glen's flow law, which governs the non-linear deformation of ice (Glen, 1955). It is the standard value adopted in glacier modelling and follows the formulation of Farinotti et al. (2009) and Huss and Farinotti (2012). We can clarify it within section 2.2.1.

Glen, J.W., 1955. The Creep of Polycrystalline Ice. Proc. R. Soc. Lond. Ser. Math. Phys. Sci. 228, 519–538

Farinotti, D., Huss, M., Bauder, A., Funk, M., Truffer, M., 2009. A method to estimate the ice volume and icethickness distribution of alpine <https://doi.org/10.3189/002214309788816759>

Huss, M., Farinotti, D., 2012. Distributed ice thickness and volume of all glaciers around the globe. J. Geophys. Res. Earth Surf. 117. <https://doi.org/10.1029/2012JF002523>

23. L184: is the glacier albedo calibration a standard practice in this modeling? To me, this seems like a huge range of potential albedos that could have a massive impact on the results – what measures were taken to ensure that the calibrated albedo matches real albedo of the glaciers at this time (especially given the fact that we have spaceborne albedo estimates for many of these regions during this timeline).

→ We agree with the reviewer that glacier albedo can vary significantly. As stated in reply to comment n.4, during the calibration we chose to explore a range of spatio-temporally fixed parameters since this would (i) substantially reduce the computational effort and (ii) avoid a clear risk of equifinality, given the very limited set of validation information at our disposal, compared to an almost endless set of potential space-time combinations of albedo values. Therefore, the goal of the calibration was to derive one lumped albedo value within the range defined in other studies in similar areas (e.g., Richter et al., 2026) that allowed to closely reconstruct the observed glacier dynamics (area and mass). Following up the reply in comment n.4, we can modify the methodology section 2.2.1 and 2.3 as well as the limitations in section 4.1 so to reinforce the main points reported and clarify the conditions used to reconstruct glacier dynamics within the lumped hydrological model.

24. Figure 2: What is the time interval for plotting the area of the glacier? If it is relatively coarse, use markers to show when the data points appear. If it is very fine, I am surprised to not see seasonality in glacier area – can you explain why this does not appear? Would there not also be a moderate amount of seasonality in overall albedo?

→ we considered yearly time interval since the update of the glacier extension works at a yearly scale. We are going to add it to the figure.

25. L206: why SWE at the end of June? This seems to be an unusual choice, given that significant melt occurs earlier in the spring. (April and May)

→ Thanks for this observation. We considered the end-of-June SWE since it represents the remaining snowpack just before the main summer melt season (July–September). We chose this metric as a proxy for the snow available to contribute to summer runoff; in alpine catchments, the snowpack by late June is critical for sustaining flows into the dry months. We can support our choice by citing the work from Penna et al., (2017), who investigated a similar contribution in a nearby area and showed that this period captures the snow influence on summer hydrology.

Penna, D., Engel, M., Bertoldi, G., Comiti, F., 2017. Towards a tracer-based conceptualization of meltwater dynamics and streamflow response in a glacierized catchment. *Hydrol. Earth Syst. Sci.* 21, 23–41. <https://doi.org/10.5194/hess-21-23-2017>

26. L230-233: the mean includes the years with positive change (unless you excluded them from the mean?) I think you mean here that qualitatively, there is minimal deviance from the long-term linear trend in any individual year (overall the signal is monotonic and linear), but that there are a few exceptions.

→ yes, we included also positive changes and computed a mean value to highlight the monotonic negative trend. In the last 30 years, a positive glacier mass budget happened for only very few years and glaciers.

27. Fig 3 caption: clarify in the caption that the increase is specifically the increase when making the glacier model dynamic.

→ thanks and we can clarify this out.

28. Fig 3: If there are 65 subbasins in this study, why are most of them not included in this figure?

→ thank you for pointing this out. We performed hydrological simulations considering 65 hydrological response units (stated in lines 175-176) and we parametrized the hydrological kernel of the hydrological model at 16 stream gauges (lines 182-183). For this reason, Figure 3 only reports the 16 subbasins.

29. L266-269: cite for “previously identified” Also, this is discussion/conclusions, not results

→ thank you and we can move it to discussion while adding explicit reference to the section of the manuscript we are referring to.

30. L285-286, 292: 0.47 and 0.55 are not “strong” correlations – they are moderate

→ thanks and we correct it.

31. Section 3.3: this is an interesting section on the hydroclimate drivers of melt, but it is not clear how this is related to the overall message of the paper, which is on modeling the contribution of glaciers to buffering (or not) droughts. For example, some of these topics are explored for the same years and regions in Brunner et al., 2023 (see fig. 4). It is also somewhat unclear from this section (and the corresponding methods section 2.5) which of the values used are from the model and which are from observations/reanalysis.

→ thank you for raising your point. The analysis of hydroclimate drivers contributes to a better understanding on how past major droughts unfolded and the buffering role of glaciers. We reported this in lines 86-90 of the introduction and we can strengthen this key message also at the beginning of section 2.5. Moreover, differently than the study of Brunner et al., 2023, our analysis of hydroclimatic drivers provides insights both at a temporal and spatially explicit way on a set of variables without assumptions on the choice of classification classes so to shed light on temporal and spatial dynamics and their significant role in the selected mountainous area.

32. The following section on counterfactuals is much more related, and, in my opinion, more novel.

→ thanks and we agree there is high novelty in the counterfactual analysis, and we will expand with the new Figure proposed in comment n.5.

33. Figure 5: please change the color scale to something more intuitive on all graphs. As currently presented, the same color can have a different meaning (and different unit!) about the same variable in different columns. Further, some rows have diverging colormaps for both the value and anomaly while others do not, regardless of whether or not the quantity is diverging. I recommend picking non-diverging colormaps for each quantity, and then a different, maybe even standard between all variables, colormap for the anomalies. Also, improve the coherence of meaning between variables. For example, white currently means a high amount of runoff, a low amount of glaciermelt, and a medium temperature.

→ thank you for this comment. We will try using different color palette for different variables as well as to use non-divergent color palette for climatology (columns on the left) and divergent color palette for anomalies. However, after having already tried several combinations for the current version, we foresee that not all inconsistencies will be solved due to the high amount of information captured in one figure.

34. Discussion: avoid repeating the results directly, but rather reference them (by figure, table, or section) and then explain the meaning. There should be more citations throughout this section as well, whenever comparing to a prior publication.

→ thank you and we can modify the discussion section, adding more literature, considerations on the implications of our contrafactual analysis on the “glacier peak contribution” and our modelling approach limitations.

35. L384: reference where this is shown in the results

→ we can add reference to figure 5

36. L435: Suggest completing the public release concurrently with this paper. Available by request does not satisfy open science guidelines, if this is what you are pursuing.

→ thanks for pointing this out, however not all the used code is available yet and hence we will keep the availability of sharing data but removing the one on code so to avoid confusion and be clear.