Martina Leone
CIMA Research Foundation
Via A. Magliotto,2 17100 - Savona
Italy

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To the editor and to the reviewer,

we thank you for your suggestions and comments. Here we respond to the reports and explain the changes made to the paper to address them.

Response to report n.1

1. Although S3M Italy has been validated for snow processes, the glacier-melt component has not undergone a rigorous uncertainty assessment. The authors are suggested to quantify model uncertainty by: Providing a sensitivity analysis for key parameters (e.g., degree-day factor, radiation coefficients), Discussing potential errors due to spatial resolution mismatch between model pixels and stake measurements.

We thank the reviewer for raising this important point. We clarify that, similarly to the snow component, the glacier melt component of S3M has undergone validation in previous work. In particular, Avanzi et al. (2022a) evaluated the glacier-melt formulation of S3M against ablation stake measurements on several glaciers in Aosta Valley, showing realistic melt magnitudes.

While a full, dedicated sensitivity analysis of key melt parameters is beyond the scope of this study, we will more explicitly discuss the validation results reported in Avanzi et al. (2022a) and their implications for the robustness of this work.

Changes: In the revised manuscript, we have:

(i) Expanded the description of the S3M glacier-melt formulation in Section 3.1 to clarify the hybrid temperature-radiation approach, precipitation-phase partitioning, and key process assumptions;

(ii) Added a more explicit discussion of glacier melt validation results from Avanzi et al. (2022a), highlighting their implications for the robustness of our melt estimates;

While a dedicated sensitivity analysis of melt parameters (e.g., degree-day or radiation coefficients) is beyond the scope of this study, we now explicitly discuss the main sources of structural and parametric uncertainty and frame our conclusions accordingly (see lines 94-106).

2. The use of static glacier outlines (RGI v6.0) likely overestimates current glacierized area, especially during 2022–2023. The manuscript qualitatively acknowledges this, but a quantitative sensitivity test should be added to estimate its impact on meltwater contribution and streamflow ratios.

We agree with the reviewer that the use of static glacier outlines from RGI v6.0 likely leads to an overestimation of glacierized area, particularly during the most recent years of the analysis. Indeed, more recent regional inventories show smaller glacier extents. To quantitatively assess the impact of this assumption and strengthen the robustness of our results, we will recompute glacier - melt contribution to streamflow also using updated glacier masks derived from the most recent available regional inventories. By comparing results obtained with RGI v6.0 outlines and with updated glacier extents, we will quantify the sensitivity of glacier melt contribution and streamflow ratios to the glacier area uncertainty.

Changes: In the revised manuscript we therefore performed a quantitative sensitivity analysis using updated glacier outlines from the most recent regional inventories (2019 for Aosta Valley and 2021 for Lombardy). We recomputed glacier melt contributions to streamflow using these updated glacier masks and compared the results with those obtained using the RGI v6.0 outlines. The analysis confirms that smaller glacier extents lead to a systematic reduction in absolute glacier melt contribution values. However, the relative amplification observed during the 2022–2023 drought remains robust. For example, mean annual glacier contribution at Tavagnasco increases from 6.4% during 2011–2021 to 19.1% during 2022–2023, while at Fuentes it increases from 2.9% to 11.1%. Similar robustness is observed for peak weekly contributions. These results indicate that although absolute values depend on the assumed glacier area, the magnitude and persistence of the drought-related intensification are not driven by the choice of glacier inventory. These additions and the corresponding discussion of glacier-area uncertainty have been included in Section 5.1 of the revised manuscript (see lines 374-388).

3. Both study basins are affected by hydropower operations. Although the authors argue that regulation effects are minor, this statement would benefit from more evidence. A short discussion on how reservoir storage timing may alter glacier-melt signals at weekly scales would strengthen the argument.

We thank the reviewer for this suggestion. To better support our statement that hydropower regulation has a limited influence on the glacier melt signal during the period analysed, we added a short discussion and supporting evidence based on simulations from the Valle d'Aosta Territorial Water Plan (PTA). These simulations (Figure 1) compare observed discharge (Qobs) with modeled discharge including hydropower operations (Qhydro) and a naturalized scenario without the main hydropower plants (Qnat). The analysis shows that while differences between regulated and naturalized discharge can exceed 200% during winter low flow conditions, these differences during the main melt season (May–September), which corresponds to the period analysed in this study, remain below approximately 10%. This indicates that reservoir storage and regulation have a limited influence on glacier melt signals during the melt season.

Changes: A paragraph in the discussion has been added (see lines 396-409).

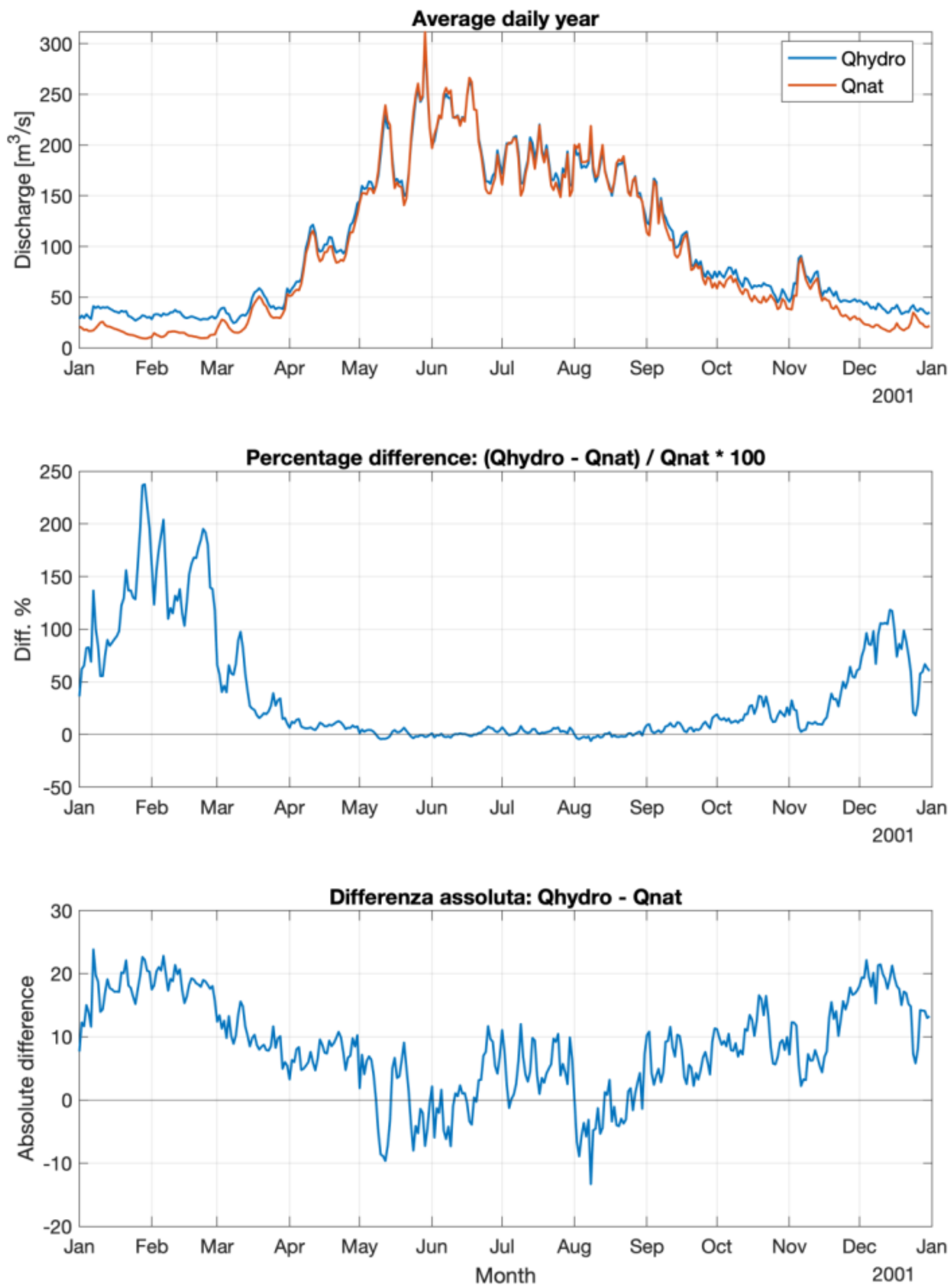

Figure 1. Comparison between regulated discharge (Qhydro) and naturalized discharge (Qnat). The upper panel shows the mean daily hydrograph from eight years of simulations. The middle panel shows the percentage difference between the two scenarios, which exceeds 200% during winter low-flow conditions. The lower panel shows the absolute difference between Qhydro and Qnat.

4. The four mechanisms (earlier onset, intensified contribution, earlier peak, prolonged season) are clearly described, but their physical interconnections remain underexplored. I recommend including a conceptual schematic illustrating the causal chain between snow drought, temperature anomalies, snow cover loss, and glacier melt timing.

We thank the reviewer for this excellent suggestion and fully agree that an illustration of the physical interconnections among the four mechanisms would strengthen the manuscript. In the revised version, we will include a conceptual schematic in the Discussion illustrating the causal chain linking snow drought conditions, temperature anomalies, reduced snow cover, and the resulting changes in glacier melt timing and contribution to streamflow, in order to synthesize the processes described in the Results and clarify their physical interdependence.

Changes: In the revised version, we added a conceptual illustration (new Figure 9) in the Discussion that synthesizes the causal chain between meteorological anomalies and glacier runoff response. The schematic highlights how temperature and precipitation anomalies lead to reduced snow water equivalent and shorter snow cover duration, which in turn expose glacier ice earlier in the season. This earlier exposure enhances ice melt and results in four characteristic responses identified in our analysis: (i) earlier onset of glacier melt, (ii) intensification of glacier melt contribution to streamflow, (iii) earlier seasonal peak in glacier melt contribution, and (iv) a prolonged melt season (see Figure 9).

5. To highlight the novelty of the results, the authors should discuss how these findings compare to similar studies in other Alpine or mountain regions. This would clarify whether the doubling/tripling of glacier contribution is typical or exceptional in a broader context.

We agree with the reviewer that placing our results in a broader alpine context would better highlight their novelty. In the revised manuscript, we will expand the Discussion by comparing our findings with previous studies from other Alpine regions, focusing on reported magnitudes of glacier contribution during drought and/or normal conditions. This comparison will allow us to assess whether the observed doubling to tripling of glacier contribution during the 2022–2023 snow drought years is typical or exceptional, and to place our results within the existing body of literature, thereby strengthening the robustness and interpretation of our conclusions.

Changes: In the revised manuscript, we expanded the Discussion to compare our findings with previous and present studies from other regions of the Alps. In particular, we now discuss studies documenting the buffering role of glacier melt during drought conditions (see lines 329-336).

6. The Discussion section would benefit from slight condensation to avoid repetition of results.

We agree with the reviewer and we will have more condensed Discussion section in the revised manuscript by reducing repetition of results and sharpening the focus on interpretation and broader implications.

Changes: In the revised manuscript we have slightly condensed the Discussion section to reduce repetition of results and improve readability.

7. Please specify the spatial resolution and vertical accuracy of the DEM used for S3M Italy, and discuss its implications for high-elevation modeling accuracy.

We thank the reviewer for this request for clarification. The digital elevation model (DEM) used in S3M Italy was originally provided by the Italian Institute for Environmental Protection and Research (ISPRA) at a spatial resolution of 20 m. For the operational implementation of S3M Italy, the original 20 m DEM was resampled to a 200 m grid using an averaging method, as described in Avanzi et al. (2023). The vertical accuracy of the DEM is therefore consistent with that of a resampled elevation dataset at 200 m resolution, and does not retain the fine scale topographic variability present in the original DEM. In the revised manuscript, we will explicitly report the DEM spatial resolution and discuss the implications of DEM smoothing for high elevation modeling accuracy.

Changes: In the revised manuscript, we have explicitly specified that the DEM used in S3M Italy was originally provided at 20 m resolution by ISPRA and subsequently resampled to 200 m using an averaging method. We have also clarified that the vertical accuracy is therefore consistent with that of a 200 m resampled DEM and does not retain fine-scale topographic variability (see lines 116-120).

Response to report n.2

Main comments:

a) Description of results. In some places I find the description of results to be a little too detailed, in terms of including many statistics in the main text which could be easily summarized more succinctly or the reader could be referred to a figure. Sometimes just shortening the text is enough, whereas in other cases improving the current figures or adding new ones would be beneficial. I think this would help keep the story clear for readers.

We agree with the reviewer and will revise the Results section to improve clarity, in particular by reducing the amount of detailed numerical information in the main text, referring more explicitly to figures, and summarizing key patterns more succinctly.

Changes: In the revised manuscript, we reduced the number of detailed numerical values reported in the main text and instead summarized the key patterns while referring readers to the figures and tables for detailed statistics (see Section 4).

b) Clearer description of (some) of the study limitations as well as model uncertainty. I think there could be an improvement in the description and illustration of the comparison of the model results against glacier mass balance (see detailed comments), and potentially remote sensing datasets could be used for comparison too. It would also be useful (if possible) to quantify the effect of not evolving the glacier outlines. The authors are clear about how they estimate the glacier contribution to discharge and already mention that it is a simplification, but I think it likely leads to an overestimation of glacier contribution to discharge since many of the not included elements (groundwater and ET) result in losses and therefore would increase the calculated glacier contribution. Potentially the glacier melt as a proportion of inputs (rain, snowmelt and ice melt within the catchment) could be used as an alternative and/or other studies could be used to compare against the magnitude of the results (even if they are in a slightly different region of the Alps). This could go in the discussion and would allow your results to be seen better in context.

We thank the reviewer for this comprehensive and constructive comment. We agree that a clearer discussion of model uncertainty and study limitations would strengthen the manuscript, and we will revise the Discussion accordingly.

Validation of glacier melt estimates at the catchment scale remains challenging, particularly in the Italian Alps, where many glaciers are partially or extensively debris covered. While satellite based geodetic mass balance datasets are increasingly available, their applicability in this region is often limited by debris cover and by the difficulty of separating snow, ice, and debris signals. For this reason, we consider the ablation stake dataset used here - collected by regional glaciological services - to be the most reliable source for evaluating glacier melt in our study area. We will clarify this point more explicitly in the revised manuscript and improve the description and illustration of the comparison between modelled melt and stake measurements.

We also agree that the use of static glacier outlines is an important source of uncertainty. As suggested, we will therefore quantify glacier melt contributions using updated glacier outlines from the most recent regional inventories. This sensitivity analysis allows us to explicitly assess the impact of glacier area uncertainty on meltwater contribution and streamflow ratios.

With respect to the estimation of glacier contribution to discharge, we acknowledge that our simplified approach likely overestimates absolute contributions, since groundwater losses and evapotranspiration are not explicitly accounted for. We will emphasize this limitation more clearly where we already introduce our definition of contribution as glacier melt over streamflow. Finally, to better place our findings in context, we expand the Discussion by comparing the magnitude of glacier contributions observed here with those reported in other Alpine and mountain-region studies.

Changes: In the revised manuscript we expanded the Discussion and Limitations sections to better describe model uncertainty and study limitations.

Specifically, we improved the description and illustration of the comparison between modeled glacier melt and ablation stake measurements. We also quantified the effect of static glacier outlines by performing a sensitivity analysis using updated regional glacier inventories (Aosta Valley 2019 and Lombardy 2021) and recalculating glacier contributions to streamflow.

In addition, we clarified that our simplified estimate of glacier contribution to discharge (ratio of glacier melt to observed streamflow) does not account for evapotranspiration and groundwater losses, which may lead to a modest overestimation of absolute contributions (see section 5.1).

c) Treatment of snowmelt. It seems like the SWE (at least over glaciers) is calculated for the catchments and that the glacier melt contribution is a combination of the snow and ice melt over the glaciers, which is pretty reasonable. However, it is less clear to me how off-glacier snow is dealt with. In section 3.1 it is mentioned that SWE is calculated by S3M nationally (so I presume also off-glacier), and yet the snow contribution to discharge is not discussed later in the paper, only the on-glacier SWE variations. Maybe for this study off-glacier snow melt was not calculated? I respect that the authors may have chosen to concentrate specifically on the glacier melt contribution, but I think it either needs to be said clearly why the off-glacier snow melt component was not calculated/analysed or for it to be included alongside the glacier melt contribution.

We thank the reviewer for highlighting this lack of clarity and we apologize for the confusion in the original manuscript. In this study, we do not quantify snowmelt contributions to discharge. Our analysis focuses exclusively on ice melt water originating from glacierized areas, which is used to estimate the glacier contribution to streamflow.

While S3M Italy simulates snow water equivalent both on and off glaciers at the national scale, off-glacier snowmelt is not analysed here because attributing its contribution to discharge would again require a fully coupled hydrological model accounting for groundwater storage, evapotranspiration, and routing processes, which is beyond the scope of this work. We agree that this was not sufficiently clear in the original text.

In the revised manuscript, we will therefore clarify (i) that the glacier contribution analysed here refers to meltwater generated over glaciers only, and (ii) why off-glacier snowmelt was not included in the analysis. This clarification will be explicitly stated in the Methods and reiterated in the Discussion.

Changes: We have clarified this point in the revised manuscript. Specifically, we now state in Section 3.1 that although S3M simulates SWE across the entire domain, SWE and meteorological variables are analysed only over glacierized areas and off-glacier snowmelt contributions are not quantified in this study. We also explain at lines 103-106 that isolating the contribution of off-glacier snowmelt would require a fully coupled hydrological model including runoff routing, groundwater storage, and evapotranspiration processes, which is beyond the scope of this work.

d) Figures. I think in various places the Figures could be improved (please see comments below) and the Appendix figures (which are for Lombardy) could fit in the same figures as the Aosta ones which are in the main paper.

We agree with the reviewer and will revise the figures to improve clarity and readability. Where appropriate, figures will be updated following the detailed suggestions, and Appendix figures for Lombardy will be integrated into the main figures alongside those for Aosta Valley to facilitate comparison.

Changes: We modified the figures as suggested.

Minor comments:

Abstract: consider starting with an introduction/rationale sentence

We agree with the reviewer and will change the Abstract to include a clearer introductory sentence outlining the motivation and rationale of the study.

Changes: In the revised manuscript, an introductory sentence has been added (see line 1).

Line 10: maybe 'elevation-dependent' is more correct

This change will be implemented in the text.

Changes: This change has been implemented in the text (see line 10).

L11: Be careful to use points 1.5 (rather than 1,5) here, and throughout the manuscript

This change will be implemented in the text.

Changes: This change has been implemented in the text (see line 11 and throughout the text).

L15: early/late season snowfalls

This change will be implemented in the text.

Changes: This change has been implemented in the text (see lines 15-16).

L24: 'negative glacier mass balances' – maybe the argument isn't quite the right way round here, since the climate warming causes the negative glacier mass balances, which are driven by the decreased snowfall

We thank the reviewer for this observation. We will better phrase to a more scientifically correct sentence.

Changes: We have revised the sentence to more accurately reflect the causal chain, clarifying that climate warming leads to reduced snowfall and snow cover, which in turn result in increasingly negative glacier mass balances and glacier shrinkage (see lines 23-26).

L35 'dry and warm conditions' do you really mean both dry and warm conditions, for snow droughts are you defining them as specifically a lack of precipitation? Or are the warm temperatures also *important?*

We thank the reviewer for this clarification. In this study, we adopt the widely used definition of snow drought as a condition of anomalously low snow water equivalent, which can arise from (i) reduced cold-season precipitation, (ii) anomalously warm temperatures causing precipitation to fall as rain rather than snow, or (iii) a combination of both processes. We will clarify this distinction in the revised manuscript and now explicitly state that warm temperatures are a key driver of snow droughts, particularly in Mediterranean and transitional alpine climates.

Changes: In the revised manuscript, we changed the sentence to explicitly define snow droughts as conditions arising from reduced snowfall, anomalously warm temperatures, or a combination of both. The revised text also clarifies the physical mechanisms linking snow drought to glacier shrinkage, including reduced snow accumulation, earlier melt onset, prolonged melt season, and enhanced ice melt (see lines 36-38).

L37 and 38: I agree that the full process from glacier melt to water supply deficit are not fully understood, but there are well known links between low snow and its impact on glacier melt and mass balance.

We thank the reviewer for this important clarification. We agree that the links between reduced snow cover and enhanced glacier melt and negative glacier mass balance are well established in the literature. Our intention was not to imply uncertainty in these cryospheric processes, but rather to highlight that the full cascade from snow deficit through glacier melt enhancement to downstream water supply impacts - particularly during compound drought and heat events - remains less well quantified. We will revise the text to make this distinction explicit.

Changes: We have revised the sentence to acknowledge that the link between reduced snow cover and enhanced glacier melt and negative mass balance is well established in the literature. The revised text now clarifies that the remaining uncertainty concerns the propagation of these cryospheric changes to downstream water supply deficits during snow droughts, rather than the glacier processes themselves (see lines 39-41).

L43: 'the contribution of glacier melt to streamflow' and 'early studies' – I would tend to say previous studies, as 'early' tends to indicate quite old studies whereas you have included those which are fairly recent

This change will be implemented in the text.

Changes: In the revised manuscript, this change has been implemented (see lines 45 and 48).

L46: 'whereas elucidating the' might be better here

This change will be implemented in the text.

Changes: In the revised manuscript, this change has been implemented (see line 49).

L48: 'little to no contribution'

This change will be implemented in the text.

Changes: In the revised manuscript, this change has been implemented (see line 51).

L49: Ayala et al is a Chilean study, it is relevant to your work, but here you are talking about Mediterranean climates. Also 'a crucial link' might be better phrasing than 'crucial hinge'.

This lines will be changed and implemented: "a crucial hinge" will be worded as "a crucial link", while we will add two citations of articles mediterranean centered: Huss, 2011 and Farinotti et al., 2012.

Changes: These changes have been implemented (see lines 53), while two citations of mediterranean centered articles have been added: Huss, 2011 and Farinotti et al., 2012 (see line 52).

L51: remove extra brackets around citation

This change will be implemented in the text.

Changes: This change has been implemented in the text (see line 55).

L58: Missing a year for the citation, here and also on line 63

This change will be implemented in the text.

Changes: The years in both citations have been added (see lines 59 and 61).

L65: '2022-2023 season' Just for clarity is this one hydrological year, or just the melt season? Maybe give dates

We thank the reviewer for pointing out this ambiguity. In the revised manuscript, we will clarify that the value of 2.8 refers to the long-term winter snow water equivalent anomaly, expressed as the March SSWEI for winter 2022–2023, as reported in Colombo et al.(2023). We will also explicitly specify the temporal reference of the "2022–2023 season" to avoid confusion between hydrological years, winter seasons, and melt seasons, and we will provide the relevant dates in the revised text.

Changes: We have revised the text to clarify to avoid confusion (see lines 68-70).

L66: Maybe rephrase for clarity, ''Both 2022 and 2023 were characterised by intense heat and exceptionally low snowfall accumulation, demonstrating..'

This change will be implemented in the text.

Changes: We have revised the sentence for clarity as suggested (see lines 70-72).

L71: Would it be correct to say '2022 and 2023 winter snow drought'? If so this is perhaps clearer.

We agree with the reviewer and will revised the text to explicitly refer to the winter snow droughts of 2022 and 2023, which provides clearer terminology and avoids ambiguity.

Changes: We revised the text to explicitly refer to the winter snow droughts of 2022 and 2023 (see line 74).

L71 to 73: Maybe stay in the same tense throughout this paragraph, this is just a style thing to consider

This change will be implemented in the text.

Changes: This change has been implemented in the text (see lines 74-79).

L79 Include also the total area of the modelled catchments, as well as the glacier area, for both regions. Include the year for this reference, if it's a website it can be when it was accessed.

This change will be implemented in the text.

Changes: The total area of the catchments has been added, alongside the glacier area, for both regions (see lines 86 and 91). Also, the informations about Aosta Valley glaciers have been updated (see line 82).

L83 0.3% - check throughout that you use points not commas as the decimal separator

This change will be implemented in the text.

Changes: This change has been implemented in the text (see lines 87 and througout the text).

Figure 1 Can you include the modelled catchment areas as well? Maybe make the hydrometric station stars larger so they are more distinguishable from stakes. In the caption, 'various inset maps' rather than 'various zooms'.

We agree with the reviewer and we will revise Figure 1 accordingly. The modelled catchment boundaries will be included in the figure, the hydrometric station symbols will be enlarged to improve their distinction from ablation stakes, and the figure caption will be revised to use "various insert maps" instead of "various zooms".

Changes: We revised Figure 1. The modelled catchment boundaries are now included in the figure, the hydrometric station symbols are larger, and the figure caption has been changed.

Section 3.1. I feel like some if this is more modelling methods rather than data. It might be better to have a specific modelling section. I also feel like it is worth being more clear that S3M is a melt model only, it does not simulate discharge. I only say this since the discharge measurements are mentioned, which somehow hints that the model simulates discharge.

We agree with the reviewer that parts of Section 3.1 describe modelling methods rather than data. In the revised manuscript, we will therefore restructure this section by introducing a dedicated subsection describing the S3M model. We will also clarify explicitly that S3M is a cryospheric melt model and does not simulate discharge.

Changes: In the revised manuscript, we restructured this section by introducing a dedicated subsection describing the S3M model. We explicitly stated that S3M is a cryospheric melt model and does not simulate discharge (see sections 3.1 and 3.2).

L102: 'second-order approximation' - this term has a very specific use, but I think you just mean that it's a reasonable simplifying assumption that glacier areas do not change and there is no debris cover.

We agree with the reviewer that the term "second-order approximation" is too specific in this context. In the revised manuscript, we will replace this expression with clearer wording, explicitly stating that the assumption of static glacier geometry and the neglect of debris cover represent reasonable simplifying assumptions for the temporal and spatial scales considered in this study.

Changes: In the revised manuscript, we replaced this expression with clearer wording (see lines 122-124).

L104: 'calibration in north-western Italy' - it would be good to give a little more detail on this, e.g. the parameters calibrated and whether they are varied spatially. Of course full details can be in related publications.

We agree with the reviewer that additional clarification on the calibration procedure would be helpful. In the revised manuscript, we will provide a brief summary of the parameters calibrated for

S3M in north-western Italy.

Changes: In the revised manuscript, we added few sentences about the model calibration (see lines 125-135).

L110: I'm not completely sure what these %s represent.

We thank the reviewer for pointing out this lack of clarity. The percentages refer to the ratio between peak snow water equivalent volume and total annual streamflow volume, computed across 102 Italian basins. We agree that this was not clearly stated in the original text. In the revised manuscript, we will clarify what these percentages represent and correct the formatting to avoid confusion.

Changes: In the revised manuscript, we clarified what these percentages represent (see lines 132-135).

L118-119: Did you consider using remote sensing datasets of glacier mass balance? E.g. Hugonnet et al. 2021 or other data specifically for Italy?

We thank the reviewer for this suggestion. We considered the use of remote-sensing–based geodetic mass-balance datasets (e.g. Hugonnet et al., 2021); however, we did not use them in this study because we considered in situ ablation stake measurements to be more suitable for validating the glacier melt component of S3M at the spatial and temporal scales of interest. In the Italian Alps, many glaciers are partially or extensively debris-covered, which can introduce substantial uncertainty in satellite derived mass-balance estimates. In contrast, ablation stakes provide direct, point-scale measurements of melt that are particularly valuable for evaluating model performance across elevation bands.

L132: 'every 500 m between 2000 m a.s.l. and 4500 m a.s.l.' would be more succinct

This change will be implemented in the text in order to simplify the reading.

Changes: This change has been implemented in the text (see line 162).

L138: Did you consider using drought indices? SPI etc.?

We thank the reviewer for this suggestion. For the scope of this study, we consider anomalies in precipitation and air temperature, together with snow water equivalent, to be sufficient to characterize the snow drought conditions relevant to glacier melt. The use of standardized drought indices (e.g. SPI) would require additional assumptions regarding accumulation periods and reference climatologies, and was therefore not pursued here. We agree that the application of drought indices could provide complementary insights and represent a valuable direction for future follow up work.

L146: 'we computed the correlation'

This change will be implemented in the revised text.

Changes: This change has been implemented (see line 177).

L149: 'discharge that is'

This change will be implemented in the revised text.

Changes: This change has been implemented in the text (see line 180).

L151: I think your simplified response to glacier contribution is ok, and reasonable given the data *you have available, but it likely overestimates glacier contributions, since the discharge has accounted for losses to groundwater/ET for all inputs. Often in more complex hydrological models ice melt at least is not lost via ET or to groundwater (although it could be), but snowmelt more often is.*

We agree with the reviewer that our simplified approach likely overestimates absolute glacier contributions to discharge, since observed streamflow integrates losses due to evapotranspiration and groundwater recharge that are not explicitly accounted for. However, we emphasize that we do not estimate glacier discharge directly. Instead, consistent with previous studies, we quantify the ratio between glacier melt and observed streamflow as a proxy for glacier contribution.

We agree that evapotranspiration and groundwater processes are important, particularly for snowmelt, and we will clarify more explicitly that the reported glacier contribution should be interpreted as a relative indicator rather than an exact partitioning of runoff components. This clarification will be added where the glacier contribution is defined and further discussed in the Discussion section.

Changes: We have clarified in the revised manuscript that glacier contribution is estimated as a ratio between modeled glacier melt and observed discharge, and should be interpreted as a relative indicator rather than an exact partitioning of runoff sources (see lines 180-186).

L156: Instead of 'concentration time' do you mean more the 'lag time' or 'travel time'

This change will be implemented in the text.

Changes: This change has been implemented (see line 191).

L163: You give the overall melt estimate biases, but the errors are still quite high at lower elevations, so I think this is worth mentioning. Ideally you could also include the mean bias overall for each region, and ideally also for each glacier (maybe figures could go in the SI).

We agree with the reviewer that the relatively larger errors at lower elevations deserve clearer discussion. In the revised manuscript, we will explicitly highlight this aspect and discuss possible reasons for the increased bias at low elevations. In addition, we will compute and report the mean melt bias aggregated by region, which provides a clearer regional scale assessment of model performance.

Changes: In the revised manuscript, we added the overall mean melt bias and RMSE for each region (Lombardy: 0.89 m w.e., RMSE 1.66 m w.e.; Aosta Valley: 0.39 m w.e., RMSE 1.15 m w.e.) (see lines 197-200).

Figure 2: It would be a good idea to include the glacier hypsometry and debris cover by elevation bins, then it's possible to see how important the glaciers are in each bin and where the debris cover is. Also add a horizontal line at 0 bias. Is the standard deviation of the comparisons? It might be useful just to see the actual modelled and measured mass balances, as then you can show the uncertainty of the measurements and model.

We thank the reviewer for these helpful suggestions regarding Figure 2. We agree that adding a horizontal reference line at zero bias and clarifying that the shaded area represents the standard deviation of the model–observation differences would improve the interpretability of the figure, and we will implement these changes in the revised manuscript.

We also considered the inclusion of debris cover information by elevation bins. However, to our knowledge, no high resolution, regionally consistent debris cover dataset is currently available for the Italian Alps. Existing global debris cover products are relatively coarse and may introduce additional uncertainty or inconsistency when compared with the point scale ablation stake measurements used here. For this reason, we prefer not to include global debris cover datasets in this analysis.

To provide additional context on the representativeness of the elevation bins, we will instead include an additional panel showing glacier hypsometry by elevation. This will allow readers to better assess the relative importance of different elevation ranges in the comparison while keeping Figure 2 focused on the model-stake validation.

Changes: In the revised figure, we have added a horizontal reference line at zero bias to improve interpretability and we have included a new panel showing glacier hypsometry by elevation. We have also clarified in the figure caption that the shaded area represents the standard deviation of the model–observation differences (see Figure 2 and the relative caption).

L171: Careful with commas in the temperature values here and below. Also, I would tend to only give air temperatures to 1 decimal point, since temperatures are rarely measured so accurately. Please also include the time period that the average was taken over for comparison.

We agree with the reviewer and will revise the manuscript accordingly. Temperature values will be reported with one decimal point, commas will be corrected, and the time period over which the reference averages were computed will be added to the text.

Changes: Commas have been replaced with points and temperature values now have only one decimal point (see throughout the text).

L172-173: This text on the temperature differences is not so efficient, these values would be better as a graph of anomalies v elevation, you could have a line per month.

We agree with the reviewer and will revise the manuscript by synthesizing the description of temperature differences and adding a new figure showing air temperature anomalies vs elevation.

Changes: We simplified the text and added a table with mean seasonal air temperatures in order for this paragraph to be more efficient (see lines 209-216 and table 1).

L175-177: Are all these temperature comparisons for June and July? Again, I think a figure would be better.

We agree with the reviewer that the wording in this section is unclear. The temperature comparisons refer to mean summer (June–July–August) air temperatures, not to individual months. We will revise the text to state this explicitly and improve clarity.

Changes: We simplified the text and added a table with mean seasonal air temperatures in order for this paragraph to be more efficient (see lines 209-216 and table 1).

Figure 3: Consider if weekly averages might be clearer to interpret, mainly for the left hand panels.

We thank the reviewer for this suggestion. We considered using weekly averages; however, we chose to retain daily values in Figure 3 because shorter-term temperature variability is essential for capturing heatwave events, which play a key role in the study period. Aggregating to weekly averages would smooth these extremes and reduce the ability of the figure to highlight the timing and intensity of heatwaves relevant to our analysis. For this reason, we would like to keep the current temporal resolution while improving the clarity of the figure where possible.

Changes: As we stated before, we kept the figure unchanged.

L186: Write out 2023 if you want to start a sentence with it.

This change will be implemented in the text.

Changes: This change has been implemented in the text (see line 218).

L193-209: In general this paragraph could be more concise. It might work better to give the % differences in rainfall, rather than the comparison of the exact amounts, I think this might be clearer.

We agree with the reviewer and will revise this paragraph to improve conciseness by emphasizing percentage differences in precipitation rather than detailed absolute amounts.

Changes: This change has been implemented in the text (see lines 222-233).

L205: 'largely on average' – 'close to average' might be better

This change has been implemented in the text.

Changes: This change has been implemented in the text ad the section has been revised.

L210-213: The idea that winter and summer were the warmest seasons of 2022 is a bit confusing, as are the stats given here. Look at the mean per season anomaly (maybe per 3 month season) and then give the one season with the largest anomaly.

We agree with the reviewer that the original wording is confusing. In the revised manuscript, we will rephrase this section by explicitly referring to seasonal mean temperature anomalies computed over three month seasons.

Changes: In the revised manuscript, we rephrased this section.

L214: It is mentioned that in 2023 the only clear difference compared to 2022 was that spring was noticeably cooler than the other seasons, but in 2022 the anomalies were much higher and the patterns between the seasons were different.

We agree with the reviewer that the original wording did not clearly distinguish the seasonal anomaly patterns between 2022 and 2023. In the revised manuscript, we will rephrase this section to explicitly compare seasonal mean temperature anomalies in the two years, clarifying that 2022 was characterized by consistently high anomalies across multiple seasons, whereas in 2023 spring exhibited markedly lower anomalies compared to the other seasons.

Changes: In the revised manuscript, this section has been rephrased (see lines 234-242).

L231: What exactly do you mean here about melt out dates? The start of the date of any melt or the exposure of ice? The term 'melt out' is more used for instance when an ablation stake melts out as its no-longer in the ice, which is not what you mean I think.

We thank the reviewer for this clarification. We agree that the term "melt-out" can be ambiguous and is commonly used to describe the complete melt-out of ablation stakes or full ice exposure. In this study, we refer instead to the timing of seasonal snow cover depletion on glacier surfaces, not to complete ice melt out. We will revise the text accordingly to explicitly define this timing as the end of snow cover on glaciers and the onset of ice exposure, thereby avoiding potential confusion.

Changes: In the revised manuscript, the term "melt-out" has been replaced with a better phrasing to avoid confusion (see line 252-255).

L234-241: You could simplify the statistics given here for clarity, for instance give the range of the % change and the elevation pattern, e.g. the deficit ranged between -71% and -56%, decreasing with altitude. Since the actual values are in figures, you can afford for simplicity to simplify the values in the text.

We agree with the reviewer and will revise this section to improve clarity by simplifying the presentation of statistics in the text.

Changes: In the revised manuscript, this section has been simplified (see lines 258-261).

L247: 'More in details' is a bit odd phrasing, maybe 'In detail..' or 'Specifically..'

This change will be implemented in the revised text.

Changes: This change has been implemented in the revised text (see line 267).

Figure 6: and paragraph L247-252, it might help understanding if you make clear again which place links to which river since in the text you give the outlet towns and in the Figure caption the rivers, just so the reader doesn't need to check back to the map again. It might also be useful in Figure 6 to add horizontal lines of the long term average, so the anomalies are really obvious. Also a comment in relation to this figure is that the discharges remain lower than normal, despite the increased glacier melt – does that mean that the glaciers are not able to fully compensate for the drought conditions?

We agree with the reviewer on all points. In the revised manuscript, we will clarify explicitly the correspondence between outlet locations and river names both in the text and in the Figure 6 caption, so that readers will not need to refer back to the map. We will also add horizontal lines indicating the long-term mean discharge to Figure 6, which makes the magnitude of the anomalies more immediately apparent.

Finally, we will expand the discussion associated with Figure 6 to explicitly address the fact that discharge remains below average despite enhanced glacier melt, clarifying that glacier melt was not sufficient to fully compensate for the concurrent precipitation deficit and reduced snowmelt during the drought conditions.

Changes: In the revised manuscript, we clarified the correspondence between outlet locations and river names both in the text and in the Figure 6 caption. We also added dashed lines indicating the long-term mean discharge to Figure 6. We also expanded the discussion on the glacier melt, which was not sufficient to fully compensate for drought conditions.

L254: 'This spur in' reads a bit strange, maybe 'This increase in'

This change will be implemented in the manuscript.

Changes:This change has been implemented in the revised text (see line 277).

Figure 7: it would be useful to have titles on the graphs with the places they represent. Also why are you showing only to week 39? There is still melt in both catchments. In the Figure 7 caption use 'upper' and 'lower' to indicate the plots, or (better) add letters to each panel. In fact this is a general comment, and adding letters to your figure panels throughout would be a good idea.

We agree with the reviewer that the clarity of Figure 7 can be improved. In the revised manuscript, we will add titles to each panel indicating the corresponding locations, and we will introduce panel labels as letters to better distinguish between plots. Panel lettering will also be applied consistently throughout the manuscript, as suggested.

Regarding the temporal extent, Figure 7 is shown up to week 39, which corresponds to the end of the hydrological year. Although some melt may still occur beyond this period, we limit the analysis to the hydrological year for consistency with the definition of annual metrics used throughout the manuscript. We will clarify this explicitly in the figure caption.

Changes: In the revised manuscript, we added titles to each panel indicating the corresponding locations, and we introduced panel labels as letters. Regarding the temporal extent, we clarified this by writing "hydrological year" in the caption.

L267: 'stood out clearly' is a little colloquial, plus I think the main point is that the glacier melt occurred earlier than usual. Maybe rephrase.

This sentence will be rephrased in order to be less colloquial and to highlight the main point.

Changes: This sentence has been rephrased (see line 289-290).

L271: Due to the importance of rainfall in interpreting the patterns it would be useful to include the precipitation patterns on the plots. I understand you might not want them to become too cluttered, but they could be small sub-plots underneath or light coloured bars. This would help the interpretation. You could also instead include the rainfall contribution to streamflow if you have this.

We thank the reviewer for this suggestion and agree that precipitation plays an important role in interpreting the observed discharge patterns. We considered adding precipitation information to the figure; however, including additional panels or bars would substantially increase this figure complexity and reduce readability, particularly given the focus of the figure on glacier melt signals. For this reason, we choose to keep the figure simple and focused. We instead can clarify the role of precipitation more explicitly in the text to support interpretation.

Changes: In the revised manuscript, we clarified the role of precipitation more explicitly in the text to support interpretation (see lines 289-295).

L286: 'growing to 63 m3s-1'

We will correct the unit measures in the revised manuscript.

Changes: Unit measures have been changed throughout the manuscript (see line 310).

L288: I do think your study does show these mechanisms of the impact of glacier melt on impacting droughts in mountain regions, but I don't know if the mechanisms themselves are so novel (as you point out with the previous research mentioned later in the discussion). However what is clear is the particularly significant role of glacier melt in these recent drought events, maybe this can be highlighted more here at the start of the discussion?

We agree with the reviewer. In the revised manuscript, we will therefore reframe the beginning of the Discussion to emphasize more clearly the significance and magnitude of glacier melt during these extreme conditions, before discussing the underlying mechanisms in relation to existing literature.

Changes: This change has been implemented (see lines 312-319).

L299: ''previous studies (Huss and Hock, 2018…" just to add a space

This change will be implemented in the manuscript.

Changes: This change has been implemented (see line 328).

L300: Maybe add some more studies on the point about glacier melt intensification during droughts

This change will be implemented in the manuscript.

Changes: This change has been implemented (see line 327-336).

Figure 8: To help the last point in the discussion it might be useful to add the average long term Ta in the top panel, and average long term Pr in the middle panel. It would also be useful to show some breakdown of the rain/snow partitioning, if you have this information.

We agree with the reviewer that adding long-term reference values would improve the interpretability of Figure 8. In the revised manuscript, we will add the long-term average air temperature to the top panel and the long-term average precipitation to the middle panel.

We also considered including a rain-snow partitioning; however, this information is not available for the analysis presented here. For this reason, we do not include a rain-snow breakdown in the figure, but we clarify this limitation in the text.

Changes: In the revised figure, the long term averages to Figure 8 have been added.

L303: 'kept at a significant level' – could be clearer here – do you mean it remained significantly higher than the long term mean?

We agree with the reviewer that this wording was unclear. In the revised manuscript, we will clarify that "kept at a significant level" refers to glacier melt contributions remaining significantly higher than the long-term mean throughout the melt season.

Changes: In the revised manuscript, this change has been implemented (see lines 332-333).

L313: Be careful here, the winter snow drought itself was not the cause of the warm and dry summer (excepting some link in the atmospheric conditions) but rather it resulted in reduced snow cover of the glaciers. I also understand your point that the melt would have been more radiation driven, but the link here is the albedo, which I would guess would be darker given earlier ice exposure. It might be worth to mention the albedo link here so its clear.

We agree with the reviewer and thank them for this important clarification. In the revised manuscript, we will explicitly mention that the winter snow drought did not cause the warm and dry summer conditions, but rather resulted in reduced snow cover on glacier surfaces. We will also clarify that the enhanced melt was primarily radiation driven due to earlier ice exposure and reduced surface albedo.

Changes: We have revised the text to clarify that the winter snow drought did not cause the warm and dry summer conditions, but instead resulted in reduced snow cover on glacier surfaces at the beginning of the melt season. We now explicitly describe the albedo mechanism, explaining that earlier ice exposure lowered surface albedo, increased radiation absorption, and enhanced radiation-driven melt (see lines 337-349).

L315: Do you mean late winter snowfall? Just to be clear as it influences the discussion, maybe give the month and the amount of snowfall more than normal. Although I agree that summer snowfalls could be important for ice melt rates, the mechanisms as described here could be clearer, what is the effect of these snowfalls on the magnitude of melt rates and how does this compare to normal years?

We thank the reviewer for this clarification. We agree that the wording is ambiguous. In the revised manuscript, we will clarify that we refer to late-winter to spring snowfall events, and we will explicitly report the relevant timing and the associated snowfall anomalies. We will also revise the discussion to better explain the role of these snowfall events, clarifying how they temporarily increased surface albedo and reduced melt rates relative to snow free conditions.

Changes: This change has been implemented in the manuscript (see lines 350-360).

L322 to 324: Can you expand on this point about compound droughts and heatwaves and how they relate to cascading cryosphere-hydrological droughts? Do you mean this is more of an autumn snow drought and/or more to do with autumn heat waves? I feel like there are some details missing here, it is because the lack of autumn snow means that glacier melt can become high as soon as there is a period of higher autumn temperatures? Or more because air temperatures are particularly high and the surface type is less important?

We agree with the reviewer that the description of compound and cascading processes require further clarification. In the revised manuscript, we will expand this discussion to explicitly describe how compound droughts and heatwaves interact with cryospheric conditions to produce cascading hydrological impacts.

We will clarify that the mechanism is not limited to autumn snow droughts alone, but rather arises from the combination of reduced snow accumulation and anomalously warm temperatures, which can occur in late winter, spring, summer, or autumn. Reduced or absent snow cover exposes glaciers earlier or later in the year, lowering surface albedo and allowing periods of elevated air temperature, particularly heatwaves, to trigger enhanced glacier melt. In this context, both the lack of insulating snow cover and high air temperatures are important, with surface type (snow and ice) playing a key role by modulating radiative energy absorption.

Changes: In the revised manuscript, we expanded the discussion to explicitly describe how precedent snow deficits and autumn heat anomalies interact to produce compound drought–heatwave conditions. We clarified that reduced snow cover increases glacier sensitivity to temperature anomalies through albedo feedbacks, and we explicitly described the cascading pathway from snow deficit to enhanced glacier melt and downstream hydrological impacts (see lines 360-360).

L330: I would tend to avoid the term 'second-order' in this case, rather likely glacier evolution within a relatively short period would likely only influence melt outputs by a small amount relative to the magnitude of melt variability due to climate conditions

We agree with the reviewer and will revise the wording to avoid the term "second-order."

Changes: We revised this in the manuscript (see lines 362-363).

L333-334: Maybe give some examples of possible models, e.g. a DETI melt model (Carenzo et al., 2016), and some examples of glacier evolution models which would be suitable (e.g. Huss et al., 2010, Jouvet and Cordonnier, 2023); Carenzo et al. (2016) An enhanced temperature index model for debris-covered glaciers accounting for thickness effect, Advances in Water Research, 94, 457–469, http://dx.doi.org/10.1016/j.advwatres.2016.05.001; Huss et al. (2010) Future high-mountain hydrology: a new parameterization of glacier retreat, Hydrol. Earth Syst. Sci., 14, 815–829, doi:10.5194/hess-14-815-2010; Jouvet and Cordonnier (2023) Ice-flow model emulator based on physics-informed deep learning, Journal of Glaciology, 69(273), 13-26.

We will mention these works and we will give some examples of possible models.

Changes: These changes have been implemented in the revised text (see lines 369-373).

L340 and 341: – Are the glacier area changes really precise to 0.01%? Consider the precision of values given, they should reflect the precision of the measurements (please also check this throughout the manuscript).

We agree with the reviewer that the reported precision was overstated. In the revised manuscript, we will adjust the numerical precision of glacier area changes and we will also check and revise numerical precision consistently throughout the manuscript.

Changes: These changes have been implemented in the revised text (see lines 379-380).

L345: I am not sure if it is possible given your model set-up, but it would be a good test to do a run with the minimum glacier areas at the end of the period. You could therefore quantify the effect of the glacier area change on the overall results. Its ok to not evolve the glacier areas if this is not possible in the model, but knowing the extreme case would give you confidence that the effect is small enough as to not influence your results. I don't think the last point validates your results directly, maybe remove this last clause.

We agree with the reviewer that this could be useful. In the revised manuscript, we will therefore compute glacier melt contributions using updated glacier areas representative of the end of the study period, and we will compare these results with those obtained using the static RGI v6.0 outlines. This analysis will allow us to quantify the impact of glacier-area uncertainty on the estimated glacier-melt contribution. We also revise the text to avoid implying that this test directly validates the results, and instead use it to assess the robustness of our conclusions.

Changes: In the revised manuscript, we computed glacier melt contributions to streamflow using updated regional glacier inventories (Aosta Valley, 2019; Lombardy, 2021) to assess the impact of glacier area changes relative to the static RGI v 6.0 outlines. As expected, using smaller glacier extents lowers absolute glacier contribution values. However, the relative amplification during the 2022–2023 snow-drought years remains robust. In fact, mean annual glacier contribution increased from 6.4 % (2011–2021) to 19.1 % (2022–2023) in Tavagnasco (triple), and from 2.9 % to 11.1 % in Fuentes (quadruple). Peak weekly contributions show similar robustness, with approximately twofold to fourfold amplification relative to baseline median peaks. These results confirm that while absolute values are sensitive to glacier-area assumptions, the magnitude of the drought related intensification is not driven by the choice of glacier inventory (see lines 374-388).

L359: 'clear if one'

This change will be implemented in the manuscript.

Changes: This change has been implemented in the revised text (see line 407).

L360: 'more than doubled from. . . '

This change will be implemented in the manuscript.

Changes: This change has been implemented in the revised text (see line 4084).

Appendix: I think you could probably fit these figures in the main paper, for instance include A3 next to Figure 5, A2 could go in panels under the current Figure 4 and A1 could fit next to Figure 3.

We will revise the manuscript accordingly. The Appendix figures will be reorganized and integrated into the main figures where appropriate to facilitate comparison and improve the flow of the results.

Changes: This change has been implemented in the revised text accordingly to the suggestions.