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To the editor and to the reviewer,

we thank you for your suggestions and comments. Here is a response to the first report.

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.

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.

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 important point and we agree that hydropower operations may influence discharge timing, potentially affecting glacier melt signals at weekly resolution. In the revised manuscript, we will expand the Discussion to better address regulation effects by providing

additional supporting evidence from available literature.

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.

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.

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.

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.