From Snow Depth to Streamflow: Reducing Snowfall Uncertainty in Alpine Headwaters with Sentinel-1-based Snow Depth Retrievals

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1 Response to the Editor

Dear Editor,

We sincerely thank the Editor and reviewers for their careful and constructive comments. We address each of Reviewer 2's comments point by point below, outlining our planned revisions to the manuscript prior to resubmission.
We also noted the Editor's comment regarding the figures: "In the downloaded PDF file it appears that there are some boxes / parts of boxes / grey lines around Figures 1, 2, 3, 4, 5, 8 and 9 – please check / are these screenshots? Figure 5: 'Sentinel-1' instead of 'Sentine-1' in the legend)."
The figures are not screenshots; we will investigate and remove these artifacts in the revised figures, and correct "Sentine-1" to "Sentinel-1" in the Figure 5 legend.

Kind regards,
Shima Azimi on behalf of all authors

2 Reviewer 2

General Comments: The manuscript addresses an important problem and presents a potentially useful application of Sentinel-1 snow-depth assimilation for snowfall correction. However, major revisions are needed before the conclusions can be supported. The main scientific concern is that the study claims improved snowfall estimates without directly evaluating snowfall against independent snowfall, SWE, or snow-depth observations. The leave-one-out analysis evaluates total precipitation at gauge locations, while river discharge provides only indirect evidence because it is influenced by many model processes and parameters. The manuscript also needs a clearer separation between data used for assimilation, model calibration, validation, and fully independent evaluation.

Response to the General Comments: We sincerely thank the reviewer for the time and effort dedicated to carefully assessing our manuscript. We greatly appreciate the thoughtful and constructive comments, which we believe will substantially improve the quality and clarity of the paper.

We acknowledge the reviewer's comments regarding (1) the evaluation of snowfall and precipitation inputs, and (2) the need for a clearer separation between the datasets used for assimilation, model calibration, validation, and fully independent evaluation. We will revise the manuscript to clarify these aspects.

We agree that direct measurement and validation of snowfall would be the most desirable assessment of the proposed approach. However, direct snowfall observations in mountainous regions are sparse and remain challenging to obtain reliably due to well-known observational uncertainties, including wind-induced undercatch, precipitation phase uncertainty, and the limited spatial representativeness of point observations in complex terrain.

Given these limitations, we adopted a complementary evaluation framework consisting of (i) a leave-one-out analysis using independent precipitation gauges to evaluate total precipitation. Although gauge-based validation provides an independent assessment, it is also subject to uncertainties associated with precipitation measurements in mountainous regions. Therefore, we complemented this evaluation with (ii) hydrological simulations to assess the consistency of the adjusted snowfall estimates with the observed hydrological response.

In addition, although comparisons with independent SNOW-IT snow depth/SWE observations are already included in the manuscript, we will expand this analysis in the main body of the revised paper with more detailed comparisons. We believe that incorporating these independent snowpack observations provides a more robust and comprehensive evaluation of the proposed snowfall enhancement framework.

We will also clarify the distinction between the datasets used for Sentinel-1 assimilation, model calibration, validation, and fully independent evaluation throughout the revised manuscript. More detailed responses to these points are provided under the relevant specific comments.

Comment 1– Abstract *The abstract is written more like an introduction. Clearly state the research gap, methods, evaluation data, main quantitative results, and limitations. Avoid paragraphing. The claim that snowfall estimates improved should be softened unless independent snow observations are used. Also explain clearly how improvements were found in years without data assimilation.*

Response to Comment 1 We thank the reviewer for this valuable comment. We will revise the abstract into a single concise paragraph that clearly presents the research gap, methodology, evaluation framework, main results, and limitations. Given the challenges associated with direct snowfall validation in mountainous regions, we will replace the term "improved snowfall estimates" with "enhanced snowfall estimates" to better reflect that the Sentinel-1 assimilation adjusts the snowfall representation without implying direct validation of snowfall accuracy against independent observations.

We will also clarify that data assimilation and calibration of the snow model parameters are performed during 2015–2020. For the remaining years, no data assimilation/calibration are applied; instead, the posterior model parameters are transferred, and the approach is evaluated using independent years. We will include the quantitative results from these independent validation years to demonstrate the robustness and transferability of the proposed approach. These points will be clarified in the revised abstract and manuscript.

Comment 2–Introduction Comments *Part 1: Major revisions are needed. Many citations are missing, the research gap is not clearly identified, and there is little connection to previous Sentinel-1 studies.*

Response to Comment 2-Part–1 We thank the reviewer for these valuable suggestions.

First, we will carefully review the existing citations throughout the Introduction and include additional and more appropriate references where necessary to strengthen the scientific context. This includes the references suggested by Reviewer 1 as well as those mentioned by Reviewer 2, thereby providing a more comprehensive overview of the current state of research.

Second, we agree that the research gap should be stated more explicitly. Existing studies assimilating Sentinel-1 snow depth observations have primarily focused on improving simulated snow states, such as snow depth and snow water equivalent. In contrast, the objective of this study is to assimilate Sentinel-1 snow depth observations to estimate snowfall correction factors, thereby improving the representation of snowfall inputs in high-elevation mountainous regions. The corrected snowfall forcing is then used to investigate how improvements in snowfall representation translate to improved streamflow simulations. This distinction and the novelty of our approach will be clearly emphasized in the revised Introduction.

Furthermore, we will expand the Introduction to discuss the limitations of conventional gauge-based snowfall correction methods. In particular, we will review the challenges associated with deriving elevation-dependent snowfall correction factors from point-scale precipitation measurements, which are subject to substantial uncertainties due to wind-induced undercatch and snow redistribution, as discussed by **?**. This discussion will better motivate the need for a complementary satellite-based framework for estimating snowfall correction factors and improving both snowpack representation and hydrological simulations in snow-dominated mountainous regions.

Following the reviewer's suggestion, we will strengthen the connection to previous Sentinel-1 studies by incorporating and discussing relevant recent work (e.g., Mirza et al. [2025], Brangers et al. [2024], Dunmire et al. [2026]) in the appropriate section of the Introduction. The revised Introduction will explicitly position our study within the existing literature and clearly highlight its novelty relative to previous Sentinel-1 assimilation studies.

Part–2: Line 23 or 24: Also cite Günther et al. (2019), "Uncertainties in Snowpack Simulations—Assessing the Impact of Model Structure, Parameter Choice, and Forcing Data Error on PointScale Energy Balance Snow Model Performance."

Response to Comment 2-Part–2 This study is relevant to support Lines 23–24 regarding uncertainties in snowpack simulations, and we will consider that.

Part–3: Lines 28-30: Cite papers showing that kriging is commonly used and discuss its limitations in mountainous terrain with sparse gauges.

Response to Comment 2-Part–3: We thank the reviewer for this valuable suggestion. We will include additional references in the Introduction demonstrating the widespread use of kriging for precipitation interpolation, such as Goovaerts [2000] and Ly et al. [2011]. We will also discuss its known limitations in mountainous regions with sparse gauge networks, including the effects of limited gauge density, unresolved orographic variability, and smoothing effects e.g., Phillips et al. [1992], Hevesi et al. [1992], Prudhomme and Reed [1999]. Furthermore, we will expand the discussion to clarify how these limitations may affect kriging-based precipitation interpolation accuracy in complex terrain.

Part–4: Line 35: Please provide a citation or evidence for this statement.

Response to Comment 2-Part–4 We will add the following reference to support the sentence in Line 35: Wood et al. [2025]

Part–5: Lines 39-47: Please acknowledge that independent evaluations have found substantial and spatially varying biases in Sentinel-1 snow depth, especially in the western United States. Cite Hoppinen et al. (2024) and Mirza et al. (2025).

Response to Comment 2-Part–5 We will acknowledge that and cite the proposed references.

Part–6: Lines 48-51: Previous work has already assimilated Sentinel-1 snow depth in the western United States and found high spatial errors. However, that work focused on snow depth rather than snowfall correction. This distinction should be used to define the research gap more clearly.

Response to Comment 2-Part–6 We agree that this point should be used to define the research gap more clearly. As described in our response to Comment 2–Part 1 (second paragraph), we will revise the Introduction to explicitly distinguish our work from previous studies that assimilated Sentinel-1 snow depth observations to improve modeled snow states (e.g., snow depth and snow water equivalent). We will clarify that the novelty of the present study lies in exploiting Sentinel-1 snow depth observations to improve the snowfall component of precipitation, thereby extending the application of these observations beyond snow state estimation. Furthermore, we will emphasize that the corrected snowfall forcing is subsequently propagated through a hydrological model to evaluate how improvements in snowfall representation translate into improved snowpack evolution and, ultimately, more accurate river discharge simulations in snow-dominated mountainous catchments.

Comment 3: Study Area and Datasets Comments *Part–1:Why do the authors expect Sentinel-1 to work well in this region?*

Response to Comment 3-Part–1 We thank the reviewer for this important question. In the revised manuscript, we will clarify the rationale for using Sentinel-1 snow depth observations in our study region. Our expectation that Sentinel-1 can provide useful information in the European Alps is primarily based on the validation study of Lievens et al. [2022], who evaluated the retrieval algorithm against a dense network of ground-based snow depth observations across the Alps. However, we acknowledge that Sentinel-1 retrievals are subject to uncertainties and will revise the manuscript to avoid implying that good performance is assumed a priori.

To evaluate the applicability of Sentinel-1 observations in our study area, we compared the satellite-derived snow depths with available ground-based observations prior to assimilation, obtaining RMSE values of 454.1 ± 216.4 mm in the Aosta Valley and 398.6 ± 193.6 mm in the Piemonte headwaters. These results are already reported in the manuscript; however, we will present this evaluation earlier to better establish the performance of the Sentinel-1 retrievals before assimilation.

Furthermore, to reduce the influence of retrieval uncertainties on the estimated snowfall adjustment factors, the methodology uses targeted post-storm observations, which minimize the impact of wet-snow effects and other SAR-related uncertainties. In addition, Sentinel-1 observations are spatially aggregated at the HRU scale before assimilation to provide more robust and spatially representative snow depth information for the hydrological model.

Part–2: Figure 1: Use a different color for precipitation gauges because red is difficult to see and not color-blind friendly. I could not find the hydrometric stations. Since discharge is the main evaluation dataset, these stations should be clearly shown. Use clearer colors for the study regions, increase the font size, and simplify the x-axis labels in panels b and c.

Response to Comment 3-Part–2 We fully agree with the reviewer's comments regarding Figure 1, and we will revise the figure accordingly.

Part–3: Line 87: Were the same discharge data used for both calibration and evaluation? Would this not bias the evaluation, or am I missing something? Please clearly state which years and stations were used for calibration, validation, and independent evaluation. Also, was Sentinel-1 snow depth evaluated before being assimilated?

We thank the reviewer for raising this important point. To address the different aspects of the comment clearly, we respond to each point separately below.

Were the same discharge data used for both calibration and evaluation? Would this not bias the evaluation, or am I missing something? We thank the reviewer for raising this important point. We clarify that the same discharge observations were not used for both model calibration and independent evaluation. The discharge data were separated into distinct periods for calibration and evaluation, ensuring that the performance assessment was conducted using independent observations that were not involved in the calibration process.

In the revised manuscript, we will clarify this procedure in the methodology section and will add a summary table (Table 1) describing the periods used for calibration and evaluation. This clarification makes the evaluation framework and the independence of the discharge observations more explicit.

Please clearly state which years and stations were used for calibration, validation, and independent evaluation.

We agree that the separation between calibration, validation, assimilation, and independent evaluation periods should be described more clearly. In the revised manuscript, we will provide a detailed overview of the temporal division of the study period.

As summarized in Table 1, Sentinel-1 data assimilation is performed over the 2017–2020 period to estimate the snow-related parameters, including the snowfall correction factor. Subsequently, the remaining model parameters, which are unrelated to snow processes, are calibrated using the 2015–2017 period while keeping the snow-related parameters fixed. **To avoid any ambiguity, the validation period highlighted in red will be removed, and only the independent evaluation period (2010–2014) will be retained. Consequently, the 2018–2020 period will no longer be labeled as a validation period. The corresponding descriptions and results throughout the manuscript will be updated accordingly to ensure consistency.**

Also, was Sentinel-1 snow depth evaluated before being assimilated?

The evaluation of Sentinel-1 snow depth retrievals prior to assimilation has already been addressed in our response to Comment 3–Part 1 (Paragraphs 2 and 3). Please refer to that response for further details.

Part–4: Lines 93-94: This is a good place to acknowledge that Sentinel-1 performance varies between mountain regions, such as the European Alps and the western United States. Cite Hoppinen et al. and Mirza et al.

Table 1: Overview of the study periods used for data assimilation, calibration and independent validation.

	Study period						
Purpose	**2010–2014**	**2015**	**2016**	**2017**	**2018**	**2019**	**2020**
Data assimilation							
Calibration							
Validation							
Independent validation							

Response to Comment 3-Part–4 We thank the reviewer for this valuable suggestion. We will add a statement at this point in the revised manuscript, such as:

"It should be noted that the performance of Sentinel-1 snow depth retrievals can vary among mountain regions due to differences in snowpack characteristics, vegetation conditions, terrain complexity, and local meteorological processes. While successful applications have been demonstrated in parts of the European Alps e.g., Lievens et al. [2022], larger uncertainties have been reported in some regions of the western United States (e.g., Hoppinen et al. [2024], Mirza et al. [2025]), highlighting the importance of considering regional conditions when applying Sentinel-1 snow depth products."

Part–5: Figure 2: Make the color bar larger. Snow depth in millimeters is difficult to read at this scale, so meters or centimeters may be clearer. Add stronger boundary lines between the Aosta and Piemonte regions. Also clarify whether glaciers are present, remove unnecessary grid lines, and explain why January-April mean snow depth is shown.

Response to Comment 3-Part–5 Thank you for the comment. To address the different parts of the comment clearly, we respond to each point separately below.

Figure 2: Make the color bar larger. Snow depth in millimeters is difficult to read at this scale, so meters or centimeters may be clearer. Add stronger boundary lines between the Aosta and Piemonte regions.

We will revise the figure accordingly, following the reviewer's recommendations as much as possible in the revised manuscript.

Also clarify whether glaciers are present, remove unnecessary grid lines

We thank the reviewer for this comment. Glaciers are present in limited areas of the study region; however, we did not apply any additional glacier masking during our preprocessing. The Sentinel-1 snow depth product used in this study was generated using auxiliary datasets, including glacier cover information from the Randolph Glacier Inventory, which was considered within the retrieval framework. Therefore, glacier-related effects are already accounted for in the Sentinel-1 product used in this study. We will clarify this point in the text and improve the corresponding figure presentation by removing unnecessary grid lines in the revised manuscript.

explain why January-April mean snow depth is shown.

Regarding the choice of the January–April period shown in the figure, this period corresponds to the main window used for assimilating Sentinel-1 snow depth observations. It was selected because it represents the dominant snow accumulation and persistence period in the study area, before substantial snowmelt begins. During this period, snowpack conditions are generally more suitable for Sentinel-1 retrievals, with predominantly dry snow conditions and limited influence of melt processes. We will clarify this rationale in the revised manuscript.

Comment 4: Methods Comments: Part–1: Please provide more details about the model setup, including spatial resolution and interpolation methods.

Response to Comment 4-Part–1 We thank the reviewer for this valuable suggestion. In the revised manuscript, we will provide additional details on the model setup, including the spatial discretization approach, the delineation of Hydrological Response Units (HRUs) using the Horton Machine toolbox, the resulting number and spatial characteristics (e.g., size and distribution) of HRUs for each catchment.

Furthermore, we will add a dedicated subsection describing the spatial interpolation method. In particular, we will provide more details on the kriging approach used to generate spatially distributed forcing data, as also requested by Reviewer 1.

Part–2: The assumption that snowfall bias is the main source of snow-depth error is too strong. Snowdepth errors may also come from snow density, rain-snow partitioning, melt, wind redistribution, vegetation, terrain, and Sentinel-1 retrieval errors. The snowfall correction factor may therefore compensate for several unrelated model errors.

Response to Comment 4-Part–2 We thank the reviewer for raising this important point. We would like to clarify that we do not assume that snowfall bias is the sole or dominant source of snow depth errors. In the data assimilation framework, we therefore did not only perturb the snowfall correction factor, but also considered other relevant snow model parameters, including melting temperature, melting factor, freezing factor, radiation factor, and snow porosity. By jointly adjusting these parameters, the assimilation framework accounts for multiple sources of uncertainty affecting the simulated snow depth evolution.

We will clarify this point in the revised manuscript to avoid the interpretation that the snowfall correction factor is assumed to compensate for all snow depth errors.

Part–3: Line 185: Define HRUs and explain how they are created. Also explain how Sentinel-1 pixels are averaged within each HRU and whether forest, glacier, radar shadow, layover, or steep terrain pixels are removed.

Response to Comment 4-Part–3 Thank you for the comment. We answer the question in split parts:

Define HRUs and explain how they are created.
As mentioned in Comment 4–Part 1, we will dedicate a subsection describing the model setup, including how the HRUs are delineated using the Horton Machine toolbox.

Also explain how Sentinel-1 pixels are averaged within each HRU and whether forest, glacier, radar shadow, layover, or steep terrain pixels are removed.

We thank the reviewer for this important suggestion. We will clarify the Sentinel-1 preprocessing and aggregation procedure in the revised manuscript. The Sentinel-1 snow depth product used in this study was developed by [Lievens et al., 2022], and the main SAR-related corrections and quality filtering were performed during the generation of this product rather than independently in this study. These procedures include terrain correction and flattening using Shuttle Radar Topography Mission (SRTM) data, exclusion of observations with local incidence angles greater than 70° to reduce radar shadow effects, and consideration of auxiliary information on snow cover, glacier extent, and forest cover. Furthermore, the 100 m grid cell size of the processed product was selected to reduce the speckle noise inherent to radar observations and to mitigate the impacts of geometric distortions within the complex topography of the Alps, including radar foreshortening, layover, shadowing, and inaccuracies in geo-referencing.

Therefore, no additional masking of forested or glacierized areas was applied during our preprocessing. In our study, the valid Sentinel-1 pixels within each HRU were used and aggregated using a mean to derive HRU-scale snow depth estimates for assimilation into the hydrological model. This spatial aggregation reduces pixel-level variability and the speckle noise inherent to SAR observations, while providing snow depth estimates at a spatial scale consistent with the hydrological model representation.

We will add a clearer description of the Sentinel-1 product characteristics, the applied quality filtering, and the HRU-scale aggregation procedure in the revised manuscript. We will also clarify that SAR-related corrections and quality filtering were performed during the generation of the Sentinel-1 snow depth product by [Lievens et al., 2022] and were not independently applied in this study.

Part–4: Line 191: What are the exact assimilation dates? Please add a table or figure showing how many Sentinel-1 observations were used in each year. Also explain how storm events are identified, why only post-storm observations are used, whether a wet-snow mask is applied, and why assimilation begins in January rather than earlier in the accumulation season.

Response to Comment 4-Part–4 Thanks for the reviewer's comment. We separately answer different parts of the question:

What are the exact assimilation dates? Please add a table or figure showing how many Sentinel-1 observations were used in each year.

We thank the reviewer for this helpful suggestion. The assimilation dates were selected based on the proposed storm-event detection method, which is explained in the following part of this comment. Also, we will add a figure summarizing the number of Sentinel-1 observations assimilated in each year. We agree that this information will improve transparency regarding the temporal resolution of the Sentinel-1 observations used in this study, and we will add the details to the revised manuscript.

Also explain how storm events are identified, why only post-storm observations are used, whether a wet-snow mask is applied,

We will clarify the procedure used to identify assimilation dates in the revised manuscript. Snow accumulation events were selected using a Sentinel-1 snow depth change detection approach. The change in snow depth was evaluated by comparing each observation with preceding and subsequent observations in the Sentinel-1 time series to identify transitions from stable or decreasing snow depth conditions to subsequent increases. These increases were considered snow accumulation events.

The first available Sentinel-1 observation after each detected accumulation event was selected as the post-storm observation for assimilation. This timing captures newly accumulated snow conditions, ensuring sufficient snow accumulation for Sentinel-1 detection while minimizing the potential influence of snowmelt and wet-snow effects on Sentinel-1 retrievals (see Figure 1).

A specific wet-snow mask was not applied, however, assimilation dates were selected during the cold season (January–April), when dry snow conditions generally dominate in the study area. We will add further details on this selection procedure and summarize the number of Sentinel-1 observations used in each year in the revised manuscript.

and why assimilation begins in January rather than earlier in the accumulation season.

The assimilation period starts in January because we aimed to ensure that

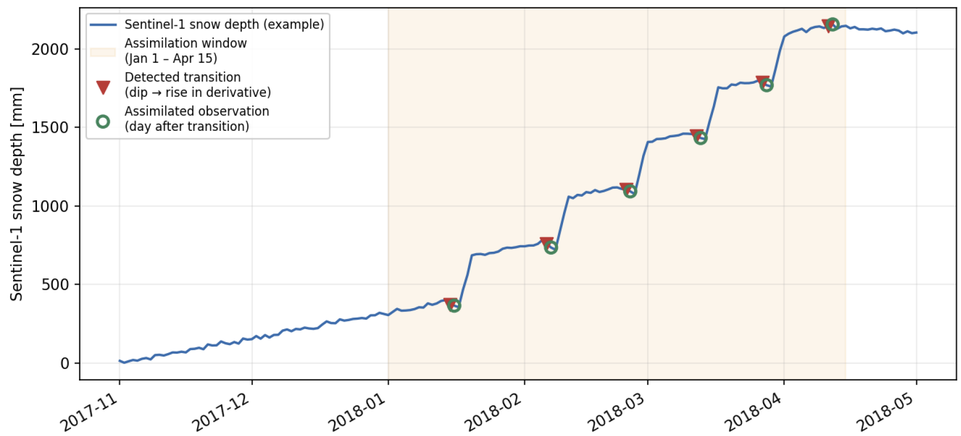

Figure 1: the method of detecting storms and selecting Sentinel-1 snow depth data for assimilation into the model (the figure is a scheme)

a more continuous and established snowpack was present before assimilating Sentinel-1 snow depth observations. This choice reduces uncertainties associated with early-season conditions, such as shallow snowpacks, and focuses the assimilation on the main accumulation period when Sentinel-1 observations can more effectively constrain snowfall-related parameters.

Comment 5: Results Part–1: Result section needs major revisions. It should clearly focus on the finding, not explaining the methods. Evaluations and validation should be clearly defined before starting the result section.

Response to Comment 5-Part–1 Thanks for the reviewer's comment. In the revised manuscript, we will reorganize this section by moving methodological details to the Methods section and adding a clear description of the evaluation strategies before presenting the corresponding results.

Part–2: Please include uncertainty ranges in the major results. Since the method produces an ensemble posterior, presenting only deterministic estimates removes important information about uncertainty.

Response to Comment 5-Part–2 We thank the reviewer for this important suggestion. In the revised manuscript, we will include posterior uncertainty ranges for the main results, including snow depth and correction factor distributions.

Part–3: Figure 5: Please calculate and report CRPS to evaluate the full ensemble distribution. From the figure, many ensemble members appear to fall well below the Sentinel-1 observations, and the ensemble may not adequately represent the observation uncertainty. There may also be particle degeneracy, with only a small number of ensemble members receiving meaningful posterior weights. Please report the effective sample size or another diagnostic of particle degeneracy and consider regenerating or expanding the ensemble to provide a wider and more representative range of possible snow states.

Response to Comment 5-Part–3 In the revised manuscript, we will calculate and report the Continuous Ranked Probability Score (CRPS) to evaluate the agreement between the posterior ensemble distribution and Sentinel-1 observations. We will also report the effective sample size (N_{eff}) of the posterior particle weights as a diagnostic of weight concentration and potential particle degeneracy.

We note that the assimilation framework used in this study is offline, based on a fixed ensemble of model simulations and observations, rather than a sequential particle-filter implementation with repeated forecast–analysis cycles. Therefore, severe particle impoverishment is not expected in the same manner as in sequential applications. Nevertheless, we agree that reporting (N_{eff}) provides a useful assessment of the posterior ensemble representation. If substantial weight degeneracy is identified, we will revise the ensemble generation such as by increasing the ensemble size to improve the representation of possible snow states.

Part–4: Also, Sentinel-1 snow depth appears to show limited interannual variability and nearly the same seasonal pattern each year. Please discuss whether this is expected or may be related to the retrieval method or spatial averaging. Please exclude 2021 from the figure if it is not used in the analysis. Finally, add posterior uncertainty ranges, such as the 5th–95th or 25th–75th percentiles, rather than showing only the posterior mean.

Response to Comment 5-Part–4 We thank the reviewer for this valuable comment. The similar seasonal patterns shown in Figure 5 may partly result from the spatial averaging of Sentinel-1 observations over the HRUs. However, this behavior is specific to the presented HRU and is not expected to be representative of the entire study region. Snow depth evolution and interannual variability can differ among HRUs depending on elevation, climate conditions, and local snow processes.

In the revised manuscript, we will provide additional figures for other parts of the study region to demonstrate the spatial variability of Sentinel-1 snow depth patterns and provide a more comprehensive assessment. We will also remove 2021 from the figure since it is not included in the analysis and add posterior uncertainty ranges to better represent the variability of the assimilation results.

Part–5: Line 213-215: It belongs to method section

Response to Comment 5-Part–5 Thanks for the comment. We agree, and it will be moved to the method section.

Part–6: Line 27-218: The increase in snowfall correction factor with elevation may result from several factors other than underestimated snowfall including Sentinel-1 retrieval bias, snow-density errors, and spatial averaging. Please acknowledge and support statement with further evaluation.

Response to Comment 5-Part–6 We thank the reviewer for this comment. We agree that the increase in the snowfall correction factor with elevation may result from several sources of uncertainty, including Sentinel-1 retrieval biases, snow-density uncertainties, and spatial averaging effects, in addition to potential underestimation of snowfall by the model. These uncertainties are partly represented in our data assimilation framework through the joint perturbation of snow-related parameters and the snowfall adjustment factor (α_s). We will revise the corresponding discussion in the manuscript to acknowledge these possible contributions and avoid attributing the elevation-dependent increase solely to underestimated snowfall.

Part–7: Line 221-222: It belongs to method section.
Response to Comment 5-Part–7 We thank the reviewer. We agree and will relocate this description to the Methods section in the revised manuscript.

Part–8: Line 240: The leave one out framework should be discussed in method section not in results.
Response to Comment 5-Part–8 We thank the reviewer for this comment. We agree and will move the description of the leave one out framework to the Methods section. We will also remove other methodological details from the Results and Discussion sections where appropriate.

Part–9: Figure 8: Please test whether the difference between kriging and data-assimilation results is statistically significant. Also show how many gauges improve and how many become worse after correction, rather than presenting only the overall improvement.

Response to Comment 5-Part–9 In the revised manuscript, we will perform an appropriate statistical significance test and report the number of gauges where the corrected results improve or deteriorate compared with the kriging-based estimates.

Part–10: IT-SNOW comparison: Please explain whether IT-SNOW is independent of the meteorological observations, model inputs, or assumptions used in this study. Correlation alone is not enough because two datasets can be highly correlated while having substantial bias. Please also report RMSE, bias, and differences in seasonal magnitude. Importantly, Please don't bring new information directly in result section which has not been defined in method or earlier sections.

Response to Comment 5-Part–10 Thanks for the comment. The IT-SNOW product is independent of the methodology adopted in this study and is used as an independent dataset for comparison. To make this clearer, we will add a brief description of the IT-SNOW dataset in the main body of the manuscript, and we will move the current results from the Appendix to the main body of the manuscript.

We agree that correlation alone is not sufficient to assess the consistency between two datasets. Therefore, in the revised manuscript, we will complement the correlation analysis with additional metrics, including RMSE.

Part–11: Figure 9 is very hard to read.

Response to Comment 5-Part–11 We will revise Figure 9 by increasing font sizes and improving colors to enhance readability.

Part–12: Did you account for area impact on these results?

Response to Comment 5-Part–12 We thank the reviewer for this question. The reported river discharge corresponds to the outlet of each drainage basin and therefore inherently integrates the contributing upstream area. We will clarify this point in the revised manuscript.

Comment 6: Conclusion Part–1:The conclusions overstate the evidence by saying that snowfall estimates are more accurate. The leave-one-out test evaluates total precipitation at gauges, while the discharge analysis provides only indirect evidence. Please distinguish clearly between improved precipitation interpolation, altered snowfall estimates, and improved discharge simulation.

Response to Comment 6 We thank the reviewer for the comment. Since snowfall evaluation in this study is conducted indirectly through comparisons with independent total precipitation observations and hydrological response, we agree that the original wording may imply a direct validation for the snowfall estimates. Accordingly, we will revise the conclusions to reflect the indirect nature of the evaluation. We will replace the term ”snowfall correction” with ”enhanced snowfall representation” and explicitly mention the limitations of direct snowfall validation in mountainous regions.

We will also clarify the distinction between improved precipitation, altered snowfall estimates, and improved discharge simulation.

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