Review paper egusphere-2025-2157

Assessing the Impact of Earth Observation Data-Driven Calibration of the Melting Coefficient on the LISFLOOD Snow Module

By Premier et al.

We thank the Anonymous Reviewer for providing valuable feedback. We believe that the manuscript can be improved by considering his suggestions and by clarifying several critical aspects that were not previously well explained. Below, we provide our point-by-point responses, highlighted in red.

General Comments

This study investigates the calibration of the snowmelt coefficient in the LISFLOOD hydrological model using Earth Observation (EO)-derived snow cover data. The authors propose two EO-based calibration methods and assess their impact on snow cover fraction (SCF), snow water equivalent (SWE), and discharge simulations across nine European river basins. The manuscript contributes to ongoing efforts to integrate remotely sensed data into large-scale hydrological modeling.

We thank the Reviewer for recognizing the topic importance.

However, several methodological ambiguities, design inconsistencies, and literature gaps limit the manuscript’s clarity, reproducibility, and broader relevance. The introduction focuses heavily on LISFLOOD while neglecting to situate the work within the substantial body of existing literature on EO-based snow calibration and assimilation techniques, many of which have long addressed multi-objective calibration using snow and streamflow data. The authors should better articulate the novelty of their approach beyond its application to LISFLOOD.

We thank the Reviewer for the thoughtful comments and for highlighting areas where the manuscript could benefit from improved clarity and contextualization. We acknowledge that the introduction currently focuses predominantly on LISFLOOD, and we agree that it could be expanded in a revised version of this manuscript. However, as explained in the following responses, the emphasis on LISFLOOD reflects the practical scope of our study—since it is the model that is relevant within our operational context.

We believe that the main novelty of this work lies in assessing the effects of a more consistent representation of a specific LISFLOOD component—the snow module—on hydrological response. To this end, we calibrated the snowmelt coefficient using EO data and re-ran the model using the EO-derived snowmelt coefficient, with all other settings consistent with the EFAS setup. However, the same approach can be applied to any other hydrological model.

This approach allowed us to directly evaluate the behavior of the snow module. Our analysis shows that adjusting the snowmelt coefficient to better represent the snow cover did not necessarily require recalibrating the other parameters. While the original model was already accurate for snow cover when considering the entire basin, this adjustment provided a more accurate agreement at the pixel scale when evaluating SCF. Crucially, these improvements in SCF did not alter the resulting streamflow.

We acknowledge that we did not develop an entirely new methodology, as many of the techniques used are established in the literature. However, we combined these techniques in a novel way to evaluate the LISFLOOD snow module differently from previous studies (e.g., Thirel et al., 2012; Pistocchi et al., 2017). Our approach includes a novel calibration technique made possible by the use of a newly developed, gap-filled high-resolution snow cover time series, which is expected to have higher accuracy than commonly used gap-filled SCA products. This is largely due to the integration of high-resolution data, which we elaborate on later in the answers.

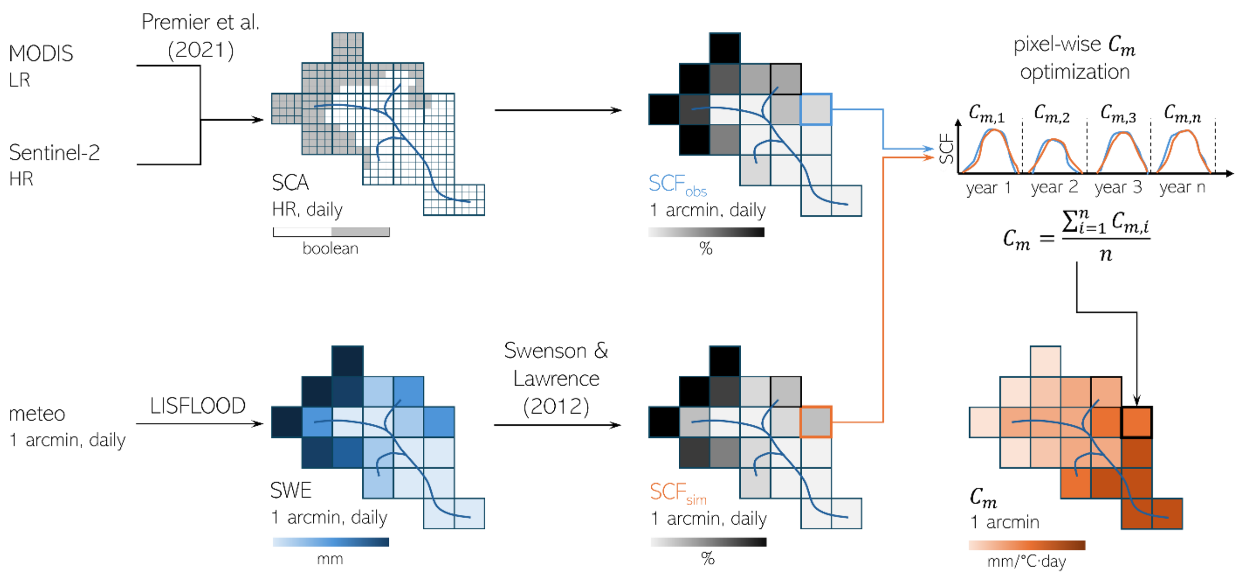


Figure 1 Workflow overview.

In this graphical abstract, we present our approach. We derive detailed daily SCF information from satellite data, which serves as a benchmark in our workflow. Concurrently, the LISFLOOD model is run, and the resulting SWE is converted to SCF using an appropriate parameterization. In this process, the snowmelt coefficient (Cm) is treated as a free parameter and calibrated through a pixel-wise optimization on a yearly basis. Finally, a pixel-wise average is computed. One season from the dataset is excluded from the optimization and reserved solely for evaluation purposes.

While this results in a post-replacement of the snowmelt coefficient, it could be possibly integrated upstream and result in a better representation of a specific module of the model.

We would also like to highlight that we initially considered submitting this work as a technical note. However, a HESS associated editor pointed out in such a case, the format would have required significant shortening and potentially omitted key methodological details. We chose the full research article format to provide a more comprehensive presentation of our methods and findings.

Should we be given the opportunity to revise the manuscript, we are confident that we can improve its clarity, reproducibility, and broader relevance by addressing these points.

This is particularly important given that the improvements achieved with EO-calibrated snowmelt coefficients remain modest, or even questionable, with respect to discharge simulations. This raises broader concerns about the hydrological value and operational significance of the proposed methodology.

We thank the Reviewer for raising this important point. Indeed, we recognize that the improvements in discharge simulations achieved through EO-calibrated snowmelt coefficients are modest and, in some cases, may appear limited. However, we can obtain a more accurate representation of the snow cover fraction (SCF). This said, we try to address a broader methodological question: *should multi-objective calibration (e.g., streamflow + SCA) be pursued, or are alternative strategies that aim at a more realistic representation of SCA feasible*?

Our findings suggest that a sequential calibration strategy—where the snowmelt coefficient is first adjusted upstream using EO-derived Snow Cover Area (SCA), followed by downstream calibration on streamflow—can serve as a viable and potentially more efficient alternative to full multi-objective calibration. In addition, we demonstrate that a standard calibration on streamflow, followed by a targeted post-adjustment of the snowmelt coefficient based on SCA, can still yield acceptable performance without the need for recalibrating the full model.

Moreover, several critical aspects of the methodology—such as the SWE–SCF parameterization, spatial resolution strategy, calibration procedure, and test basin selection—are poorly explained, inconsistently justified, or insufficiently analyzed. The comparison between models, methods, and performance metrics is often difficult to follow and underinterpreted. Key information for reproducibility (e.g., calibration configurations, data preprocessing protocols) is also lacking.

Many of the methodological components applied in this study (e.g., SWE–SCF parameterization, calibration procedures) are based on previously published and validated approaches. For the sake of brevity and to avoid unnecessary repetition, we opted to refer to those sources and keep the descriptions concise. In a revised version, we will aim to strike a better balance between brevity and completeness.

While the topic is relevant and the integration of EO data into hydrological modeling remains important, the manuscript in its current form suffers from fundamental methodological opacity, weak novelty positioning, and limited hydrological impact. Key sections are unclear or poorly justified, the experimental design is inconsistent, and the results do not support the claimed contributions. For these reasons, I recommend rejection. A revised version would require a substantial restructuring of both the methodology and the scientific framing to meet the standards expected for publication in HESS.

We thank the Reviewer for highlighting the weaknesses of the manuscript. We are confident that by addressing these points in the revised version, we can significantly improve the quality and clarity of the work.

Specific Comments

L14–15: This sentence is too vague to provide meaningful insight into the methodology or key contributions.

We thank the Reviewer for this comment. We agree that the original sentence “These findings highlight the potential of integrating EO data to enhance snowmelt simulations and improve water balance predictions, with important implications for hydrological modeling and water resource management” is too vague. The sentence will be changed as “These findings highlight the potential of integrating EO data to calibrate the snow melt coefficient without changing other calibration parameters. This approach may offer practical advantages in situations requiring accurate snow cover representation, although our results also show that standard calibration procedures provided in this case an acceptable representation of snow dynamics”.

L15–75: The introduction overfocuses on LISFLOOD and insufficiently addresses broader research on EO-based calibration and snow data assimilation. The authors should frame the novelty of their study in light of widely used multi-objective calibration approaches and explain how their work differs in terms of technique or purpose—not merely model specificity.

We thank the Reviewer for this comment. However, we believe that an explanation of LISFLOOD in the introduction is necessary, as it is the model we use and central to our analysis. The emphasis on LISFLOOD reflects the practical scope of our study—it is the tool available and relevant for our operational context.

That said, the core objective of this work is to address a broader methodological question: *should multi-objective calibration (e.g., streamflow + SCA) be pursued, or are alternative strategies feasible*?

Our findings suggest that a sequential calibration approach—first adjusting the snowmelt coefficient upstream using EO-derived SCA, followed by downstream calibration on streamflow—can be a viable and potentially more efficient alternative. Furthermore, we find that a standard calibration on streamflow alone, followed by a post replacement of the snowmelt coefficient based on SCA, can still yield acceptable results without the need to recalibrate the entire model.

Although our experiments were conducted using LISFLOOD, we believe the rationale and calibration strategy explored here are applicable to other similar distributed hydrological models.

In response to this comment, we will revise the introduction to better highlight the broader methodological implications of our work and clarify how our approach differs from conventional multi-objective calibration strategies.

L59–60: The use of snow data in hydrological model calibration is not new and has become a common practice for over a decade. The statement should be revised to reflect this context.

Thanks for the comment. We will rephrase the sentence from "This calibration approach differs from traditional hydrological calibration methods by introducing an independent process that does not rely solely on discharge data" to "This study investigates the effects of adjusting the snowmelt coefficient with a post replacement of the snowmelt coefficient based on EO snow cover data without the need for a complete recalibration of the entire model." also based on the previous answer.

L66–67: The role of Sentinel-2 data here is unclear. If its main function is downscaling MODIS, this should be stated explicitly. Furthermore, cloud-free MODIS products (e.g., MOD10A1 Version 6) and well-documented gap-filling techniques are already available—please clarify why these were not used or compared.

We thank the Reviewer for the insightful comment, which allows us to clarify the role of high-resolution data in our methodology. While a simplified interpretation might suggest that Sentinel-2 data are used solely to downscale MODIS, this is not entirely accurate. As described in Premier et al. (2021), Sentinel-2 data are also employed to correct MODIS observations. Our method relies on the assumption that high-resolution (HR) data are more accurate than low-resolution (LR) data, particularly for fractional snow cover, where Sentinel-2 can detect snow patches that MODIS may miss.

Unlike other state-of-the-art approaches that only downscale MODIS while preserving its Snow Cover Fraction (SCF), our method also corrects SCF, offering a novel improvement. While we acknowledge the existence of various gap-filling and downscaling algorithms in literature, we consider the validation of our methodology beyond the scope of this paper, as it has already been published in Premier et al. (2021).

That said, we agree with the Reviewer on the importance of comparing our method with other existing and operational products. For this reason, Appendix A includes a comparison with the gap-filled VNP10A1F product, which demonstrates that it can serve as a good alternative to our more complex and labor-intensive algorithm. Although MOD10A1F could also be tested, we expect similar performance due to its comparable characteristics and gap-filling strategy. Additionally, VNP10A1F offers a slightly higher spatial resolution, which is advantageous.

L110–115: Calibrating 14 parameters without detailed justification seems excessive. A summary table of parameters and ranges is essential. A sensitivity analysis would help assess the importance of the snowmelt coefficient relative to other parameters and reveal possible interdependencies.

The table below specifies the 14 model parameters typically fitted in a LISFLOOD calibration. The model has more parameters (for instance, the temperature thresholds that define precipitation as snowfall or the start of snowmelt), but this is the selection of the most sensitive parameters after years of working with the model. Not all parameters are sensitive in all the catchments, e.g., the reservoir parameters are not in a catchment without reservoirs, or the snowmelt coefficient in areas where snowfall never occurs. These cases are identified by the calibration tool and the irrelevant model parameters are removed from the calibration. Further details can be found here: https://ec-jrc.github.io/lisflood-code/4_annex_parameters/.

Table 1. Calibration model parameters in the OS-LISFLOOD model.

Process	Parameter	Range			Unit
		Minimum	Default	Maximum	
Snow	Snowmelt coefficient	2.5	4	6.5	mm/°C·day
Soil	Xinanjiang b	0.01	0.5	5	-
	Preferential flow	0.5	4	8	-
	Groundwater percolation	0.01	0.8	2	mm/day
	Upper GW zone constant	0.01	10	40	days
	Lower GW zone constant	40	100	1000	days
	Lower GW zone threshold	0	10	30	mm
	Groundwater loss	0	0	0.5	mm/day
Streamflow routing	Manning's n modifier: main channel	0.5	1	2	-

	Mainning's n modifier: floodplain	0.5	1	2	-
	Trigger of split routing	0	2	20	-
Reservoirs and lakes	Reservoir: normal storage	0.01	0.8	0.99	-
	Reservoir: normal release	0.25	1	2	-
	Lake multiplier	0.5	1	2	-

Regarding the purpose of this research, the independent calibration of the snowmelt coefficient does not interfere with the rest of the model parameters. On the contrary, that is what sequential calibration tries to avoid. In the "traditional" calibration, the snowmelt coefficient would be calibrated together with the other 13 parameters to streamflow. In that scenario, due to equifinality, it may happen that the calibrated snowmelt coefficient does not correctly reproduce snow processes, but the streamflow simulation performs well. That is actually what we wanted to explore with this study.

L111: Please clarify whether model calibration is based solely on streamflow using KGE. If so, this should be justified in light of the study's stated focus on snow processes.

This paragraph explains the usual LISFLOOD calibration, where the snowmelt coefficient is fitted together with all the other parameters to maximize the Kling-Gupta Efficiency of the streamflow in a gauging station downstream. It is introduced here for reference, as a comparison with the sequential approach explored in the analysis. We will make clearer that this paragraph refers to the "traditional" calibration.

L123–124: If all 14 parameters are optimized per sub-basin against streamflow, why isolate the snow module for analysis? The risk of equifinality and parameter interactions should be acknowledged and discussed.

We thank the Reviewer for the comment and would like to clarify the calibration routine of LISFLOOD. The L-Cm snowmelt coefficient was calibrated as part of the LISFLOOD calibration for the European domain within the European Flood Awareness System (EFAS) and the European Drought Observatory (EDO). This calibration routine involves 14 parameters, and is based solely on streamflow using KGE, as detailed on the CEMS page:
https://confluence.ecmwf.int/display/CEMS/EFAS+v5.0+-+Calibration+Methodology+and+Data
In this study, LISFLOOD itself was not recalibrated. The evaluation of the module structure and the calibration routine were not within the scope of our work.

Given that the model calibration is based exclusively on streamflow and involves multiple parameters, it can lead to satisfactory streamflow reproduction but does not guarantee an accurate representation of snowmelt processes. This limitation may arise due to parameter interdependencies, model complexity, equifinality, and the common challenge in distributed hydrological modeling of "being right for the wrong reasons" (Beven and Cloke, 2012).
For this reason, we decided to calibrate the snowmelt coefficient using EO data and then re-run the model with only the EO-snowmelt coefficient different from the EFAS setup.
This process allowed us to evaluate how the snow module behaved.
What we found out is that by changing the Snowmelt coefficient, we observe a quasi-equifinal for streamflow but not as equifinal for SCA, SWE and melting. The catchment average runoff and the discharge at the outlet have low sensitivity to the changes in the snowmelt coefficient, effects of the new parameters in upper basins are expected (as we will show later), but the snowmelt coefficient calculated using the traditional calibration compensates for local differences. If we work with a model where this holds true, we can calibrate the snowmelt coefficient to our will and then calibrate the rest independently, thus arguably achieving higher accuracy on snow but very similar on streamflow. We show that for LISFLOOD this holds true.
We acknowledge that sensitivity analyses have been conducted for LISFLOOD:
parameter uncertainty (https://www.tandfonline.com/doi/pdf/10.1623/hysj.53.2.293)
global sensitivity analysis (https://www.gdr-mascotnum.fr/media/mascot13zambrano-poster.pdf)
calibrated parameter analysis:
https://www.sciencedirect.com/science/article/pii/S0022169418307467?via%3Dihub
sensitivity analysis
(https://www.sciencedirect.com/science/article/pii/S2214581816300817?via%3Dihub)
global sensitivity and uncertainty analysis
(https://www.sciencedirect.com/science/article/pii/S0022169417301671)
While we agree with the Reviewer that a detailed sensitivity analysis would be valuable, especially in the context of LISFLOOD, we consider it beyond the scope of the current study, which focuses primarily on the snow module and the calibration of the snowmelt coefficient.
We will add a subsection in the methodology section where we address the valuable comment of the reviewer:

2.5 Evaluation against current LISFLOOD setup

To complete the assessment, we evaluated the changes in hydrological response resulting from the snowmelt coefficient (Cm) that performed best in simulating the SCA. Specifically, we replaced only the snowmelt coefficient originally estimated during the EFAS calibration (ECMWF, 2022) (L-Cm), with the one calibrated using Earth Observation (EO) data (EO-Cm), and ran the LISFLOOD model from 1990 to 2022, including two warm-up years (1990–1991). We stressed that the model was not re-calibrated for the other 13 parameters, allowing us to isolate the impact of the Cm on river discharge in the selected catchments and to assess whether the current EFAS setup can be

trusted to realistically model snow accumulation and melting dynamics. The snowmelt coefficient in LISFLOOD is traditionally calibrated as part of a multi-parameter routine that optimizes 14 parameters simultaneously against observed streamflow data, focusing on maximizing the Kling-Gupta Efficiency (KGE). However, this approach is prone to equifinality, where multiple parameter combinations yield similar discharge performance (Beven, 2006), but may mask inaccuracies in the representation of other processes because of parameters interactions and errors compensation. Consequently, while EFAS calibration achieves good streamflow fits, it can produce less realistic simulations of processes not directly constrained by streamflow data (Beven, 2019).
This issue is especially pronounced in large-scale hydrological models applied at continental and global scales, where structural uncertainty becomes significant due to the application of a uniform model framework across catchments with widely varying climatic and physical characteristics (Beven, 2006; Beven and Cloke, 2012).

By calibrating only the Cm with EO data independently from the full 14-parameter calibration, we directly constrain the snowmelt process using spatially and temporally explicit snow observations, reducing the equifinality problem and compensatory effects among parameters. Replacing only the Cm in the LISFLOOD model while maintaining the remaining parameters fixed allows for the evaluation of two critical aspects: (1) the extent to which the full multi-parameter calibration accurately reproduces snow accumulation and melt dynamics, and so if the model is fit-for-purpose for snow related evaluations. And (2) the influence of the independently calibrated Cm on river discharge, specifically assessing its impact on model performance as measured by the Kling-Gupta Efficiency (KGE).

The results are presented as monthly averages between 1992 and 2022, derived from the mean monthly values of the original 6-hourly model outputs. The outputs include snow water equivalent (SWE), snowmelt, total runoff, and discharge, all expressed in mm/month. The discharge monthly average was calculated based solely on dates with available observed data. Dashed lines in the figure represent the 10th and 90th percentiles of the time series, providing insight into the variability and uncertainty of the modeled hydrological response."

L137: The description of elevation banding is unclear. How are elevation classes defined and implemented at the 1.4 km model resolution, which significantly smooths real terrain features? What are the implications for snow accumulation and melt representation?

We thank the Reviewer for the comment. We agree that our explanation of how elevation zones were defined was not sufficiently clear. Due to the relatively coarse resolution of LISFLOOD cells (1', ~1.4 km), significant sub-pixel variability in snow accumulation and melt can occur, particularly in areas with large elevation differences within a single pixel.

To address this, snow processes are modeled separately within three elevation zones defined at the sub-pixel level. These zones are determined based on a normal distribution of elevation values, which has been shown to represent well the actual distribution. To this purpose, the standard deviation of elevation within a grid cell is calculated from the Multi-Error-Removed Improved-Terrain (MERIT) DEM with a spatial resolution of 90 m. The three elevation zones—A, B, and C—are each assumed to cover one-third of the pixel area.
Assuming that the average pixel temperature corresponds to the mean pixel elevation, temperatures for the lower zone A and upper zone C zones are estimated by applying a fixed lapse rate (L = 0.0065 °C/m) to the elevation differences from the mean. Snow accumulation and melt are then modeled separately for each zone, using the temperature at each zone's centroid as a proxy for local conditions.
To improve clarity, we will add these details that can be found in the LISFLOOD model official documentation (https://ec-jrc.github.io/lisflood-model/2_04_stdLISFLOOD_snowmelt/) to the manuscript.

L140–164: The SWE–SCF parameterization is central but confusing. Equations (7), (8), and (10) appear circular or contradictory. Their derivation, purpose, and assumptions must be clarified. Also, kaccum plays a key role but is not explained. A graphical illustration would help. The brief mention of the Swenson & Lawrence vs. Zaitchik & Rodell methods lacks depth and justification.

We thank the Reviewer for this comment, which lets us clarify the rationale and implementation of the SWE-SCF parametrization approach. First, regarding our choice of parameterizations, we are aware that several approaches have been proposed in literature. A non-exhaustive list that can be included in the manuscript is:

- Luce, C. H., Tarboton, D. G., & Cooley, K. R. (1999). Sub-grid parameterization of snow distribution for an energy and mass balance snow cover model. Hydrological Processes, 13(12-13), 1921-1933.
- Douville, H., Royer, J. F., & Mahfouf, J. F. (1995). A new snow parameterization for the Météo-France climate model: Part I: validation in stand-alone experiments. Climate Dynamics, 12(1), 21-35.
- Roesch, A., Wild, M., Gilgen, H., & Ohmura, A. (2001). A new snow cover fraction parametrization for the ECHAM4 GCM. Climate Dynamics, 17, 933-946.
- Liston, G. E. (2004). Representing subgrid snow cover heterogeneities in regional and global models. Journal of climate, 17(6), 1381-1397.
- Niu, G. Y., & Yang, Z. L. (2007). An observation-based formulation of snow cover fraction and its evaluation over large North American river basins. Journal of geophysical research: Atmospheres, 112(D21).

- Helbig, N., van Herwijnen, A., Magnusson, J., & Jonas, T. (2015). Fractional snow-covered area parameterization over complex topography. Hydrology and Earth System Sciences, 19(3), 1339-1351.
- Pimentel, R., Herrero, J., & Polo, M. J. (2017). Subgrid parameterization of snow distribution at a Mediterranean site using terrestrial photography. Hydrology and Earth System Sciences, 21(2), 805-820.

While we acknowledge the importance of a comprehensive treatment of SCF parameterizations, this lies beyond the main scope of our study, which is to propose an alternative calibration approach for the snowmelt coefficient in the LISFLOOD model. To that end, we chose to test two parameterizations that offer a balance between model complexity and data availability. Swenson & Lawrence is also an approach widely used in the Community Land Model (CLM). On the other hand, Zaitchik & Rodell is a simpler empirical method requiring fewer input data, yet shown to produce consistent results. Our results (Table B1) show that both parameterizations yield similar performance in our experimental setup, with a general better agreement with the EO data when using Swenson & Lawrence.

Regarding the equations, they are derived from the mentioned publication and from the code of the CLM model, available here:
[CTSM/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90 at master · ESCOMP/CTSM]

While we agree that we could better explain the meaning and role of the equations, we consider a full derivation of the SWE–SCF parameterization beyond the scope of this paper, as it would add considerable complexity and potentially distract from the main objectives. However, we provide here a brief conceptual explanation to help clarify the approach.

The accumulation formulation (Eq. 7) is based on the probability that a pixel becomes snow-covered after a precipitation event. Specifically, the snow-covered fraction (s) is defined as:

$$s = \min(1,\ k_{accum} \cdot SWE)$$

This defines *s* as the probability that a pixel is snow-covered, with *kaccum* acting as a scaling parameter that relates SWE to fractional coverage. Accordingly, the probability that a pixel remains snow-free is **p = 1 − s**.

If multiple snowfall events occur, and assuming independence (i.e., uncorrelated events), the cumulative probability that a pixel remains snow-free is the product of the individual **p** values. Therefore, after *N+1* events, the snow-covered fraction can be updated as:

$$SCF_{N+1} = 1 - (p_{N+1})(p_N) = 1 - (1 - s_{N+1})(1 - s_N)$$

Similarly, Eq. 7 that is also implemented in CTSM/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90 at master · ESCOMP/CTSM, is based on a probabilistic interpretation involving a *tanh* function, where *tanh* ensures that SCF asymptotically approaches 1 as SWE increases.

Regarding the depletion curve (melting), Equation 8 is derived empirically, as stated by the original authors. It is important to note that Equation 10 can be obtained by inverting Equation 8. Additionally, in the original paper, Equation 11 is reported with a typographical error; however, the correct formulation is implemented in the corresponding code. We have also been in contact with the original authors to confirm that we are using the correct version of the formula. Please, for deeper understanding check:

Swenson, S. C., & Lawrence, D. M. (2012). A new fractional snow-covered area parameterization for the Community Land Model and its effect on the surface energy balance. Journal of geophysical research: Atmospheres, 117(D21).

We agree that, as stated in the original paper, the accumulation parameter kaccum plays an important role. For this reason, we chose not to keep it fixed. Instead, we calculate it dynamically using our EO-derived SCF data at the time of the first snow accumulation, as also suggested by the original authors. This parameter represents the ratio between SCF and SWE at the onset of accumulation—when the pixel is still only partially snow-covered—and is therefore essential for determining the rate or "speed" of snow accumulation.

L165: Is the snowmelt factor calibrated independently of other LISFLOOD parameters? If so, a discussion of the implications and potential benefits of multi-objective calibration (including SCA and runoff) is needed.

We thank the reviewer for this insightful comment. Indeed, the snowmelt coefficient was calibrated independently from the other LISFLOOD parameters, and no multi-objective calibration was performed in this study. The primary objective of comparing the EFAS setup with the same setup incorporating the independently calibrated snowmelt coefficient was to evaluate whether recalibrating this single parameter would significantly impact the hydrological cycle. Additionally, this approach enabled us to assess the robustness of the full LISFLOOD calibration—which involves 14 parameters—in accurately reproducing snow dynamics.
We acknowledge that this aspect was not clearly articulated in the manuscript. To address this, we will consider to include a dedicated paragraph in the Methodology section discussing the implications of independent versus multi-objective calibration strategies, highlighting the potential benefits of jointly calibrating snow cover area (SCA) and runoff. Furthermore, we will expand the

Discussion section to incorporate these considerations and their relevance to model performance and parameter interactions.

L169: The snow balance equation is invalid in glaciated basins where annual melt can exceed snow accumulation due to negative mass balances. The method should either exclude these basins or account for ice dynamics.

We thank the Reviewer for this valuable comment. The basins that include glaciers are: Adige, Alpenrhein, Arve, and Salzach. The basins without glacierized areas are: Gallego, Guadalfeo, Laborec, Mörrumsån, and Umeälven.

Although the glacier-covered area is relatively small in most of the glacierized basins—less than 1% of the total area (approximately 0.9% for Adige and Salzach, and 0.6% for Alpenrhein)—we acknowledge that glaciers can still have a non-negligible influence, particularly in the Arve basin, where the glacierized area is approximately 5%. This influence is also visible in our results (see Figure 4 and discussion starting at Line 247).

We agree that the ice component has not been adequately addressed in the current version of the manuscript. Our initial intention was to mask out pixels where the glacier coverage exceeded a certain threshold during the calibration of the snowmelt coefficient. A proper representation of glaciated areas would require distinguishing between snow and ice surfaces and applying different coefficients accordingly. However, we believe this is beyond the scope of our work.

Furthermore, the LISFLOOD setup does not explicitly model glacier dynamics. The standard approach in LISFLOOD adjusts melt rates for ice-covered areas using a sinusoidal function to increase melt in summer, accounting for enhanced radiation and changes in surface albedo. However, this is a simplified treatment that does not capture true ice mass balance or dynamics.

L173–176: The intent of this paragraph is unclear. Please rephrase to clarify what is being estimated or illustrated.

Thank you for your comment. The purpose of the paragraph is to clarify the conditions under which Eq. 11 is valid. Specifically, this equation assumes a single, continuous snow period—defined as a sequence of days during which a pixel remains continuously snow-covered. In such cases, it is reasonable to assume that total accumulation equals total melt over the snow season, ignoring other processes like wind or gravitational snow transport.

However, in some pixels—especially at lower elevations or in temperate climates—multiple snow periods may occur (e.g., snow melts and re-accumulates later). In these cases, Eq. 11 should ideally be applied separately to each distinct snow period. While this would be more accurate, it would also introduce additional methodological complexity.

For consistency with the original approach proposed by Pistocchi et al., (2017), we retain their simplification of applying the equation across all snow-covered days, regardless of whether snow cover is continuous or intermittent. This simplification is a known limitation of the method and one reason we expect improved performance from the optimization-based approach proposed in this study.

We propose rephrasing the paragraph as follows to make this point clearer: “Eq. 11 is strictly valid only over a single, uninterrupted snow period—defined as a sequence of days when the pixel remains continuously snow-covered—we follow the approach of Pistocchi et al. (2017) and apply the equation across all snow-covered days, regardless of continuity. This simplification avoids additional complexity that would arise from segmenting and analyzing multiple snow periods per pixel. However, we acknowledge this as a limitation of the method, particularly in lower-altitude regions where snow accumulation and melt cycles occur more frequently within a single season.”

L178: What is being compared here? A model simulation using observed SCFs? The terminology and structure are confusing and require clarification.

We thank the Reviewer for this comment. By L-SCF (LISFLOOD SCF), we refer to the snow cover fraction (SCF) estimated using the LISFLOOD model. The LISFLOOD model itself does not directly provide SCF as an output; instead, it outputs snow water equivalent (SWE), which is computed using Equations 4 to 6, as a function of the snowmelt coefficient Cm.

To derive SCF from the modelled SWE, a parameterization (Equations 8–10) must be applied. This parameterization derives SCF from SWE to SCF, thus being SCF a function of Cm too.

The snowmelt coefficient Cm is treated as a free parameter in our framework and is subject to optimization. To optimize it, we minimize the error between L-SCF and EO-SCF, which refers to the SCF derived from Earth Observation (EO) data and serves as a reference.

L165–191 (Section 2.4): This section should be rewritten to clearly explain both EO-based methods for estimating melt factors. The current text lacks transparency and methodological rigor.

Thanks for the comment. For the sake of clarity, we will revise the section and add information about parameter ranges, the combination of the different hydrological seasons, and how the algorithm works in snow-free pixels.
Also please refer to Figure 1 to have a general overview of our approach.

The resolution mismatch between EO data (50–500 m) and model grid (1.4 km) introduces major issues. Downscaling MODIS to 50 m and then reaggregating to 1.4 km is not clearly justified. How are orographic gradients in precipitation and temperature accounted for at this coarse scale? The authors should better discuss whether a semi-distributed approach (e.g., elevation bands) or higher-resolution modeling would improve consistency with EO data and SWE estimates.

Thank you for the comment. As also mentioned in a previous response, our methodology is not straightforward downscaling of MODIS data. Instead, it includes a correction step aimed at addressing known limitations of the MODIS sensor, such as errors due to grain size variability, solar zenith angle, viewing geometry, and atmospheric effects that are not fully accounted for in the retrieval algorithm. Following the approach described in Premier et al. (2021), we do not treat the MODIS-derived snow cover fraction (SCF) as an absolute value. Instead, we interpret it within a "safety belt" of uncertainty and primarily rely on high-resolution data to reconstruct snow patterns through robust statistical analysis. These reconstructed patterns implicitly account for topographic effects, including orographic influences. While the snow cover retrieval method is not the main focus of this paper, we agree that this aspect deserves a clearer explanation and will clarify it in a revised version.

Regarding orographic gradients, as noted in a previous response, our model accounts for elevation-dependent snow processes by dividing each pixel into three elevation zones. This semi-distributed approach allows us to represent variations in snow accumulation and melt processes with altitude. Also note that meteorological forcing (EMO-1) considers the temperature gradient with altitude. That's not the case for precipitation.

We agree, however, that higher-resolution modeling could improve consistency between model outputs and EO-derived SWE, particularly in complex terrain. Nevertheless, it is important to stress the fact that the current model has been developed to run at continental scale (resolution > 1km). Increasing the resolution would increase computational time and output data size, crucial considerations for a model that runs operationally. Increasing resolution does not guarantee improved performance in all model compartments, since some processes, now simplified or ignored, might become more relevant and higher resolution (Van Jaarsveld et al., 2025). Moreover, we would like to emphasize that fine-resolution modeling is only as accurate as the quality of the input forcing data. In many basins—especially at high elevations—observational data are scarce or of limited accuracy, which poses challenges for high-resolution modeling. In contrast, EO observations may capture some processes, such as wind redistribution of snow, more effectively when high-resolution acquisitions are available. Thus, the integration of EO data remains a valuable complement to physically based modeling.

The manuscript suggests the key research question is spatial calibration (pixel vs. basin scale, L47–48), but this is insufficiently explored. How do calibration results differ at each spatial scale? What is gained or lost?

Thank you for this important comment. As shown in Figures 2 and 3, the pixel-wise calibration might result in a distribution that highly differs from that of the lumped coefficients. Initially, our idea was to investigate whether snowmelt coefficients calibrated at pixel scale could reveal meaningful patterns or correlations with topographic, geographic, or land cover features. Our initial guess was that such correlations might eventually allow for snowmelt parameterization based on spatial characteristics alone—potentially reducing reliance on EO data, which are often labor-intensive to process. However, as shown in Table C2 and discussed in Line 370, our analysis did not reveal strong correlations between the calibrated snowmelt coefficients and those spatial features. We are also aware that while pixel-level calibration allows for more spatial detail and potentially better alignment with EO-derived patterns, it increases computational burden. Basin-scale calibration has shown in this study to be already sufficiently robust but may hide local heterogeneity.

L202–209 & Table 1: The basin selection lacks justification. Several catchments (e.g., Arve, Salzach) include glaciers, while others (e.g., Guadalfeo) are subject to strong anthropogenic influences (e.g., reservoirs, diversions). These factors are not modeled and introduce significant uncertainty. Their inclusion must be explained and justified—or their results treated with caution.

We thank the Reviewer for this comment. As already stated in Section 3, the basins were selected based on the following criteria: i) they are all snow-dominated catchments, ii) they represent a range of geographical contexts, iii) they cover diverse land cover types, and iv) they span a range of elevation zones. Additionally, as mentioned in L204 of the manuscript, the initial selection also considered the availability of discharge data.

We acknowledge the Reviewer's point that some of the selected catchments, such as the Arve and Salzach (influenced by glacier melt), and Guadalfeo (subject to significant anthropogenic influence), include processes that introduce additional sources of uncertainty. In the specific case of the Guadalfeo River, the Rules reservoir—constructed in 2006, midway through our analysis period (1990–2020)—is included in EFAS. However, EFAS assumes the reservoir was present throughout the simulation period, which introduces bias in the model output for this catchment. Additionally, the Guadalfeo basin was not part of the EFAS calibration, which further contributes to the relatively poor model performance shown in Figure 7. These limitations will be clearly acknowledged in the revised discussion to help contextualize the results.
For a detailed explanation of the reservoir modeling approach in LISFLOOD, we refer the Reviewer to the official LISFLOOD documentation:
https://ec-jrc.github.io/lisflood-model/3_03_optLISFLOOD_reservoirs/
And for a comprehensive list of reservoirs included in EFAS, refer to:
https://hess.copernicus.org/articles/28/2991/2024/

Table 1 / Calibration vs. Regionalization: It is unclear why some basins are calibrated while others are regionalized. This methodological inconsistency needs to be explained. Consistent baseline comparisons are essential to interpret calibration effectiveness.

We thank the reviewer for the comment. Some basin parameters came from a regionalization approach and not calibration because of the lack of river discharge observation. The map of the domain calibrated with river discharge is presented here: https://confluence.ecmwf.int/display/CEMS/EFAS+v5.0+-+Calibration+Methodology+and+Data

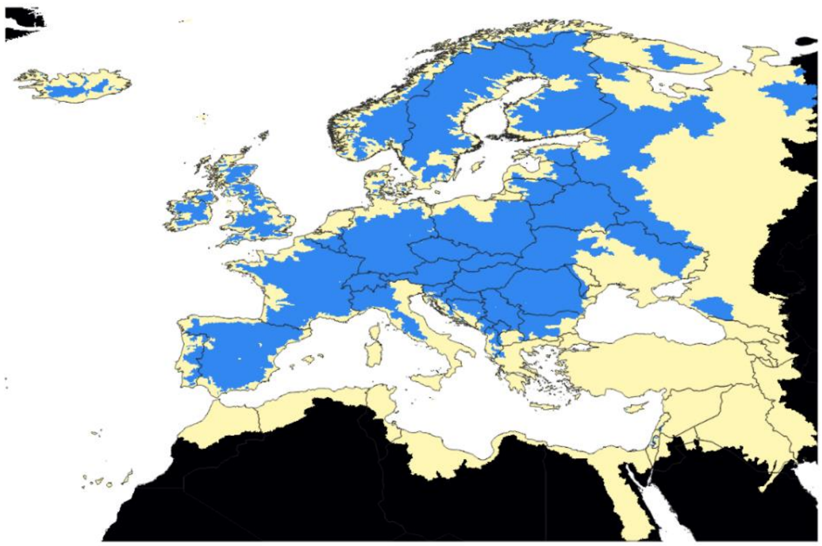


Figure 2 In blue the area of the pan-European domain for which discharge observations were available; in yellow the area of the pa-European domain for which discharge observations were NOT available for EFAS v5 calibration. The area in black is not included in the modeling domain.

The Adige and Guadalfeo basins were not calibrated during the calibration of the EFAS system; their parameters were assigned using the regionalization methodology from Beck et al. (2016) [L120].
For this study, we managed to get observed data of river discharge, so the comparison against the simulated river discharge from LISFLOOD was possible.
As much as this could be seen as inconsistent, we believe that this was actually an opportunity to evaluate the regionalization approach effectiveness, a common challenge in data-scarce/ungauged basins. We will stress this better in the methodology and discussion

Figure 1: The coarse DEM resolution leads to incorrect hypsometry (e.g., Arve basin's maximum elevation is ~4800 m a.s.l., not 3700 m). This smoothing likely affects snow (and ice) accumulation and melt modeling and should be discussed.

Thank you for the comment. We agree that the resolution is very coarse, but LISFLOOD has been developed as a large-scale model. However, as discussed previously in another answer, the intra-pixel variability is partially considered by using three elevation zones inside the pixel. Assuming that the average pixel temperature corresponds to the mean pixel elevation, temperatures for the lower zone A and upper zone C zones are estimated by applying a fixed lapse rate (L = 0.0065 °C/m) to the elevation differences from the mean. Snow accumulation and melt are then modeled separately for each zone, using the temperature at each zone's centroid as a proxy for local conditions.

L204–209: The temporal alignment of model forcing (1992–2022) and snow data (2017–2023) is confusing. Are independent evaluation years used? If so, how is calibration/control separation ensured? A proper split-sample test would strengthen the study.

Thanks for the comment. The snowmelt coefficient has been calibrated over a five-season period, from October 1, 2017, to September 30, 2022. The sixth hydrological season, from October 1, 2022, to September 30, 2023, is used only for evaluation purposes. This period was chosen being the period of maximum availability of Sentinel 2 data (as stated from L205). After using the previous EO seasons to calibrate the snowmelt coefficient, we ran LISFLOOD in the period 1992-2022 to compare the effects of the differently calibrated snowmelt coefficients in the SWE climatology. We might include also the last seasons to have more consistent periods, however we believe this is not affecting our outcomes.

L216 and Throughout: The manuscript uses many overlapping abbreviations for calibration methods (e.g., EO-Cm, EO-Cm1, EO-Cm2, LBFGS-B, L-Cm) with insufficient explanation. This confuses readers. Provide a summary table of methods and a glossary of acronyms. Terms should be redefined when introduced in different sections.

We thank the Reviewer for his comment. In a future version, we will add a summary table and redefine the terms in each section.

L220–225: The differences in results across basins should be discussed. Are certain physiographic features (e.g., elevation, land cover, glacier presence) associated with better or worse performance?

We thank the Reviewer for raising this interesting point, which inspired us to carry out an additional analysis. For the sake of brevity, we report here the results in terms of RMSE, evaluated for SCF derived from both L-Cm (Figure 3) and EO-Cm,2 (Figure 4). The performances are analyzed against selected physiographic features (mean elevation, forest coverage, and slope) and climatic features (mean precipitation, temperature, and snowfall). Furthermore, we distinguish basins with and without glaciers using different colors.

The results show a tendency for higher errors in lower-elevation and flatter catchments, while increased forest coverage is also associated with higher errors. Regarding the climatic features, an inverse relationship with RMSE is observed: basins with higher precipitation and snowfall tend to show lower errors. This is expected, since basins with less precipitation — especially less solid precipitation — have more ephemeral snow cover, leading to a higher fraction of partial snow cover and thus greater potential for errors.

Glacierized catchments do not appear to show substantial differences compared to non-glacierized ones. It is also noteworthy that the Umealven basin consistently stands out as an outlier with lower RMSE. This may be explained by its prolonged and complete snow cover, which likely results in more stable snow conditions and fewer errors.

We can include these additional results in a future version of the manuscript.

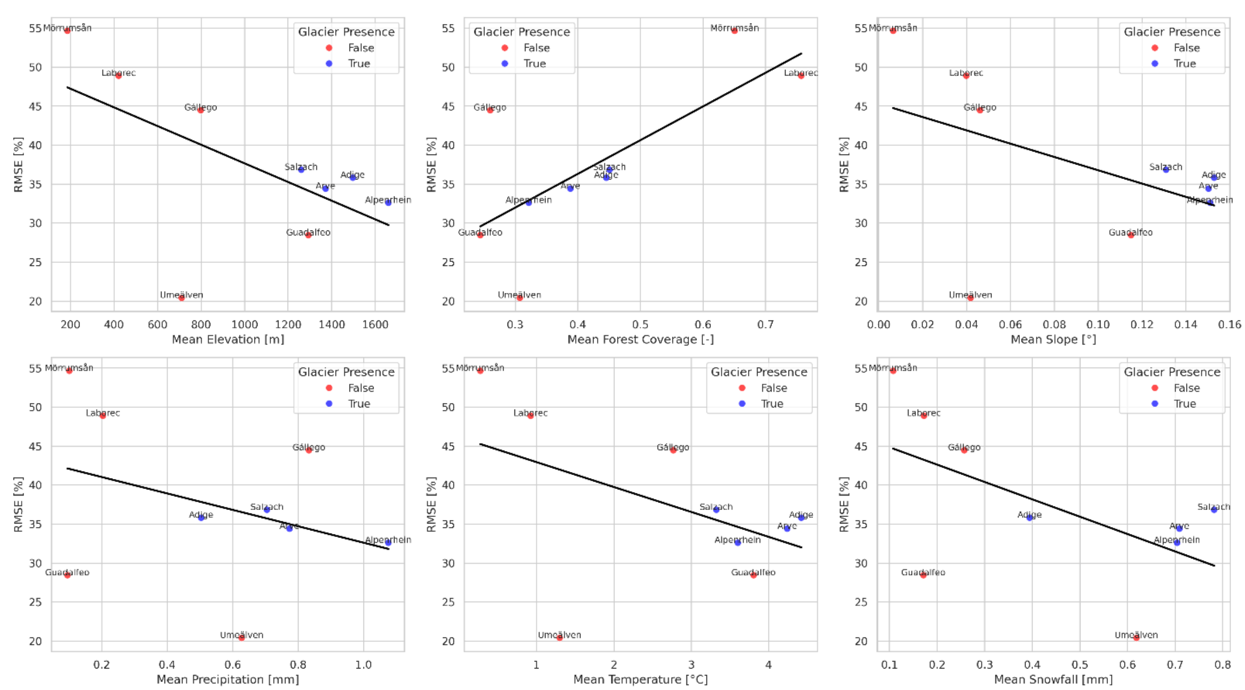


Figure 3 Performances in terms of SCF RMSE versus different physiographic and climatic features when using L-Cm.

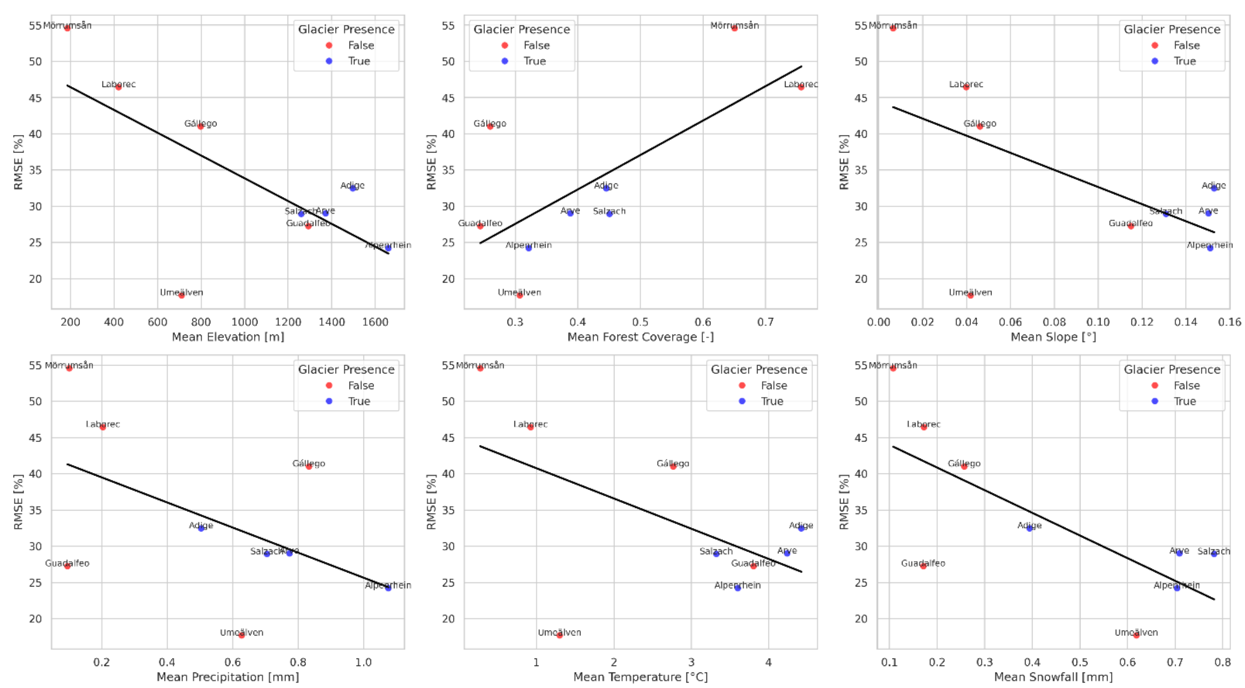


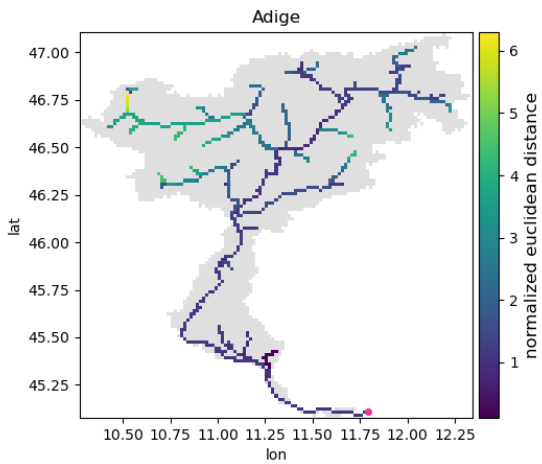
Figure 4 Performances in terms of SCF RMSE versus different physiographic and climatic features when using EO-Cm,2.

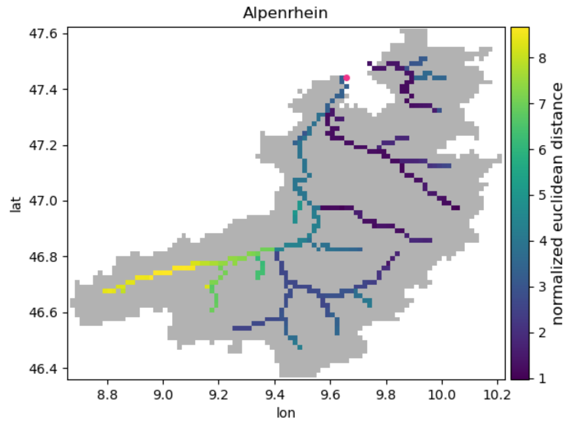
L270: This section is mischaracterized as a "water balance" analysis, but it is actually a comparison of LISFLOOD SWE with other model outputs. The full hydrological balance (precipitation, evapotranspiration, storage changes) is not analyzed, which would be relevant.

We thank the reviewer for the comment. We agree that the terminology is misleading since we are not reviewing the water balance of the model.
We will create 2 separate subsections. The first one "4.3 Snow Water Equivalent Exploratory Evaluation" will cover the comparison against other models that estimate SWE, the other subsection will be called "4.4 effects on LISFLOOD long-term simulation" and it will cover the comparison of the performance of the LISFLOOD model run against the LISFLOOD results using the EO-Cm. The monthly (and daily will be added) averages for discharge, snowmelt, snow cover and total runoff will be presented. Moreover, we will include a spatial comparison of river discharges that will show us in which areas of the catchment the discharge is affected by the EO-Cm.
Regarding the analysis of the other components of the model, our analysis focused on the impact of the snowmelt coefficient on discharge. Moreover, given the fact that LISFLOOD is a mass balance model, the effects on other components are limited, especially looking at the limited impact of EO-Cm on total runoff, shift (decrease) in infiltration/evapotranspiration are expected in

Salzach, Arve, and Alpenrhein for the months of June-July given the higher runoff compared to the EFAS simulation.

The limited impact of the new snowmelt factor at catchment scale will be further analysed in the revised manuscript where we will include the following analysis that looks at the impact of the EO-Cm at sub-catchments level, and for upstream areas above 100 km2. The impact of EO-Cm is visible locally, and more precisely in some sub-catchments. This corroborates our thesis in saying that the current EFAS calibration serves its purpose in representing well catchment average snowmelt dynamics. However, users should be careful when using simulated discharge upstream of the calibrated stations, since river discharge, in some cases, can be very different between the discharge from EFAS5 and the discharge computed using EO-Cm.
Those differences are shown in the plots below, when we computed the Normalized Euclidian distance (NED) between the EFAS river discharge and the EO-snowmelt coefficient of river discharge. River discharge was selected for upstream areas above 100 km2 for both model outputs and min-max normalized. The NED was then computed between grid cells at the same location. The NED is presented for each catchment in Figure 5. Darker colors represent grid cells/river sections where the two models produce similar daily discharge outputs (lower NED); lighter colors indicate areas where the models diverge more significantly.
The spatially heterogeneous EO-snowmelt coefficients have a noticeable local impact in some river reaches with low upstream area, here the differences between model outputs are more pronounced. However, this influence decreases progressively downstream as localized effects are smoothed out along the flow path. By the time the discharge reaches calibration points, typically located further downstream, the impact becomes negligible, as the calibration process compensates for or overrides local parameter variations.
This is confirmed also by looking at the daily and monthly averages at catchment level, where differences between the two discharges are negligible, besides Salzach, Arve, and Alpenrhein. Therefore, while users can trust the current EFAS5 version to represent catchment-scale snowmelt-runoff dynamics, we recommend caution when interpreting simulated river discharge in upstream or mountainous areas, especially where inflow to reservoirs is critical, as local snowmelt-runoff processes may not be fully captured. These points will be clarified and supported with references in the revised manuscript.


Adige
lat
lon
normalized euclidean distance


Alpenrhein
lat
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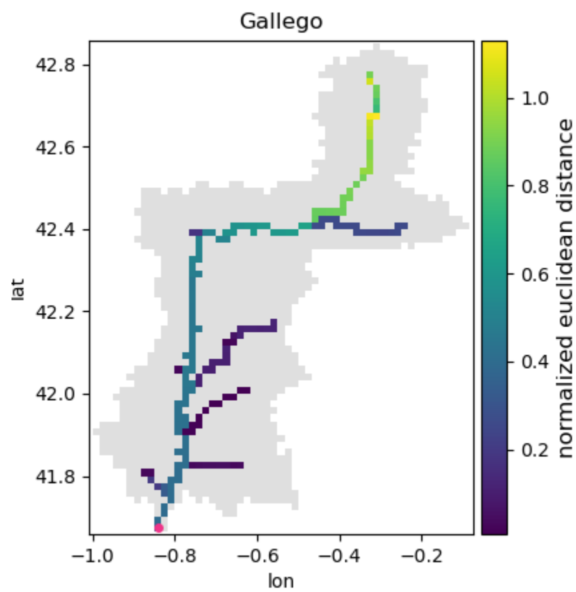
normalized euclidean distance


Gallego
lat
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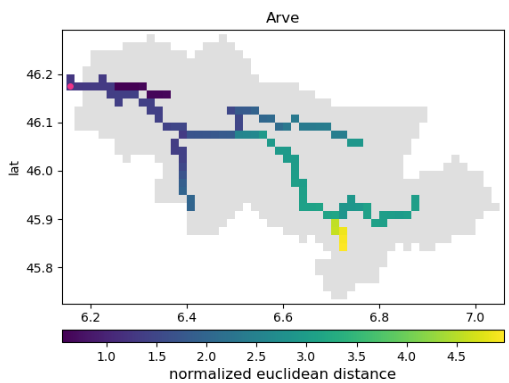
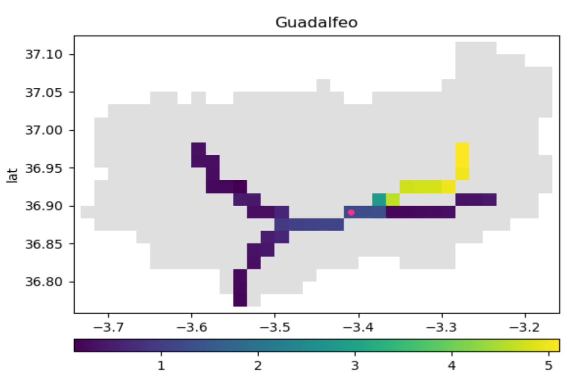
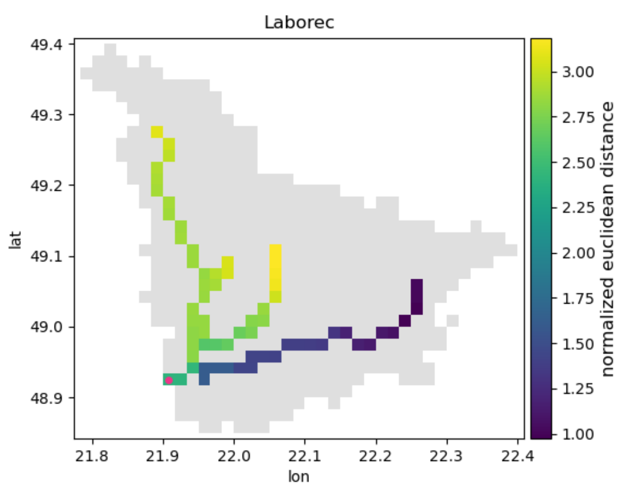

Arve
lat
normalized euclidean distance


Guadalfeo
lat
normalized euclidean distance


Laborec
lat
lon
normalized euclidean distance

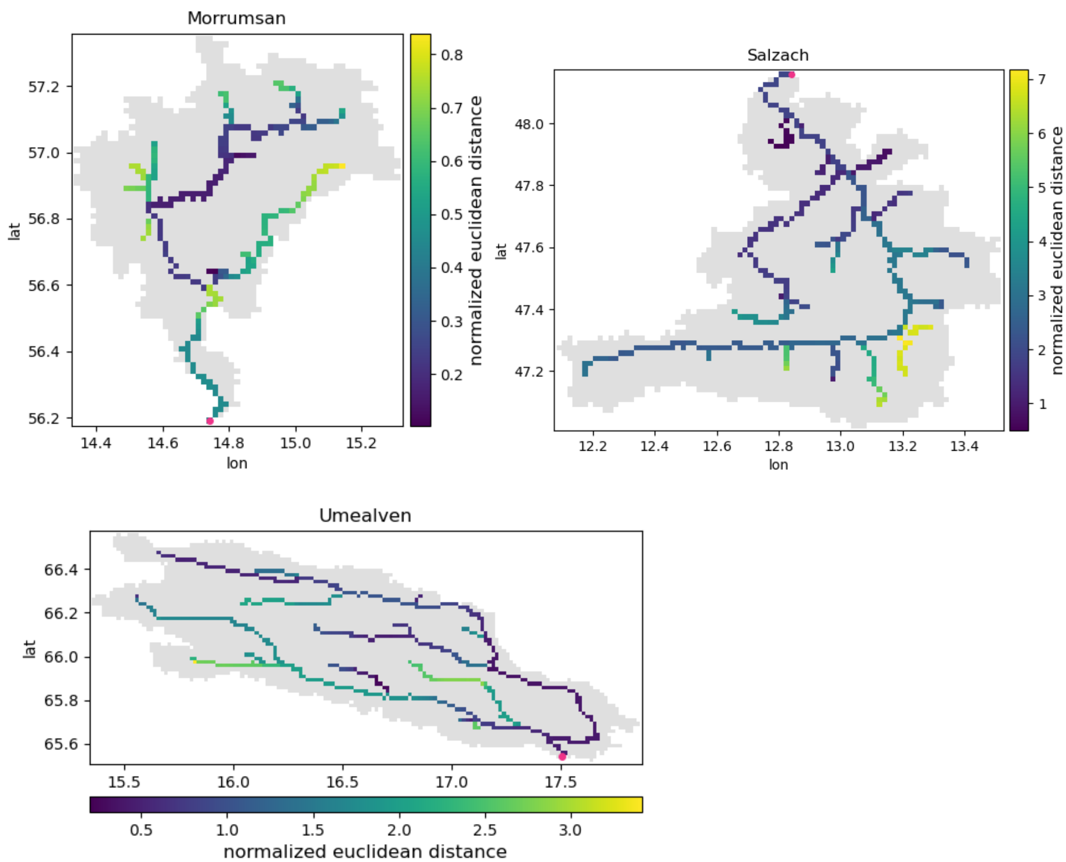


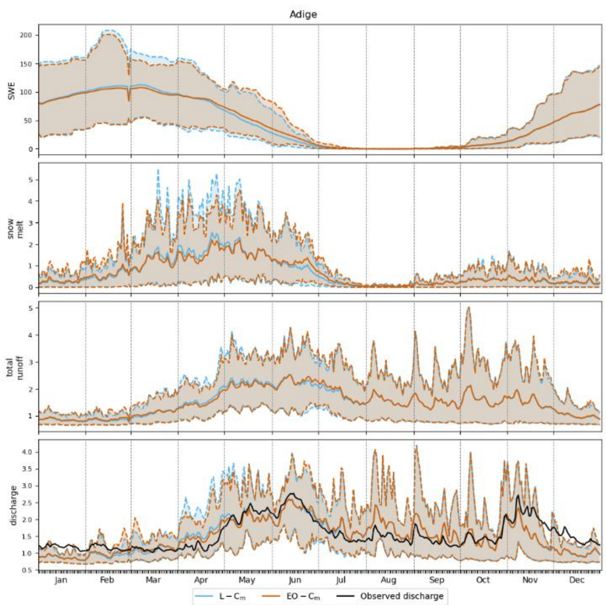
Figure 5 normalized Euclidean distance between daily discharge from the EFAS5 model and the discharge from the LISFLOOD model run with the new EO-coefficient. In grey the catchment area. In magenta the station used in the evaluation in section.

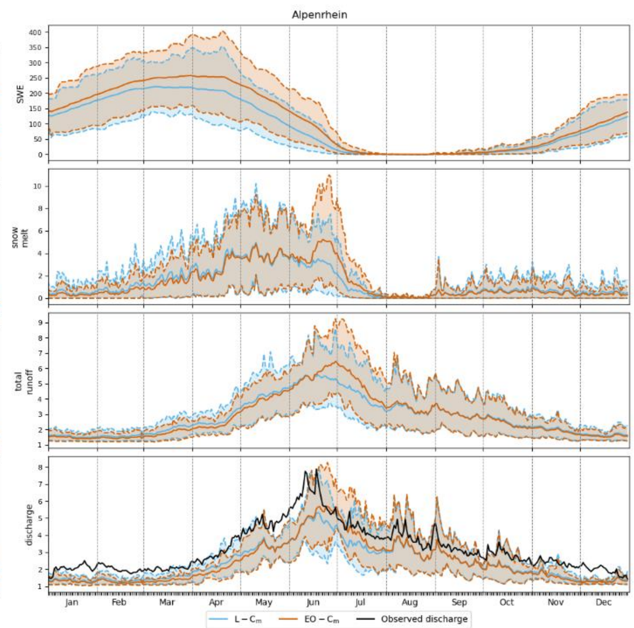
L279–281: Comparing LISFLOOD SWE with other models without harmonized forcing data is misleading. The comparison should be framed as qualitative or exploratory—not as validation.

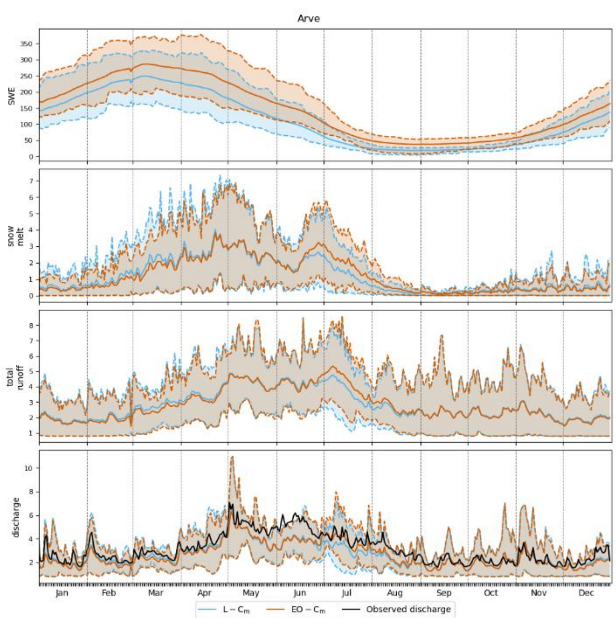
We completely agree with the reviewer that the analysis does not represent a formal validation. As stated in L282 of the manuscript, we refer to the analysis as an intercomparison of existing SWE products. We acknowledge that this analysis is not exhaustive and should be considered a preliminary step, especially given the lack of reference SWE datasets (as stated in L292-293). Even if in-situ SWE measurements were available, they would not provide a suitable reference due to the coarse resolution of the LISFLOOD model and the high intra-pixel variability introduced by complex topography. Additionally, other models cannot be considered absolute references, as they may have inherent limitations stemming from model parameterizations and the quality of forcing inputs. We will clarify in the revised version of the manuscript that this is intended as an exploratory analysis rather than a comprehensive validation.

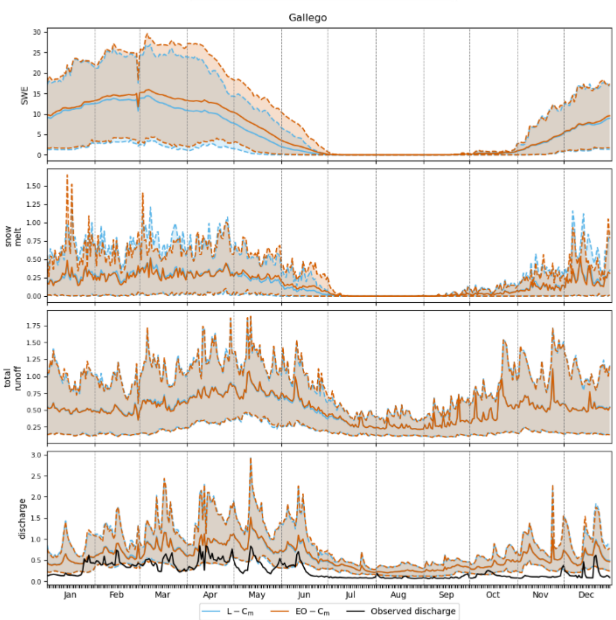
Figure 7 and L319 etc.: “Climatology” is misused. Use “seasonal average” or “mean monthly values.” Also, explain what the envelopes in the figure represent. Monthly aggregation may obscure important daily dynamics—consider showing daily averages instead.

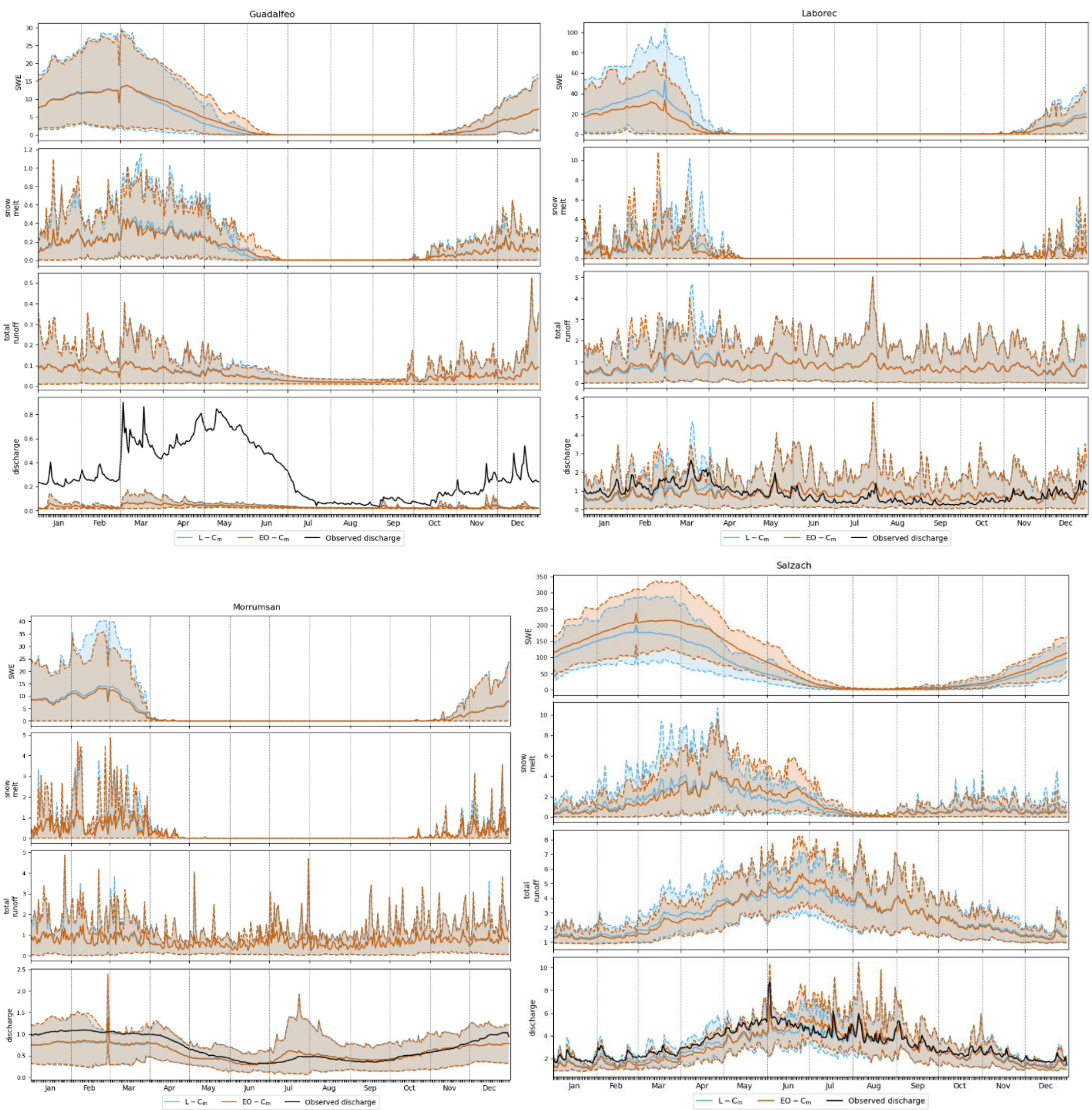
We agree with the reviewer, and we will amend the terminology in the manuscript. We will add daily averages as shown in the following plots.

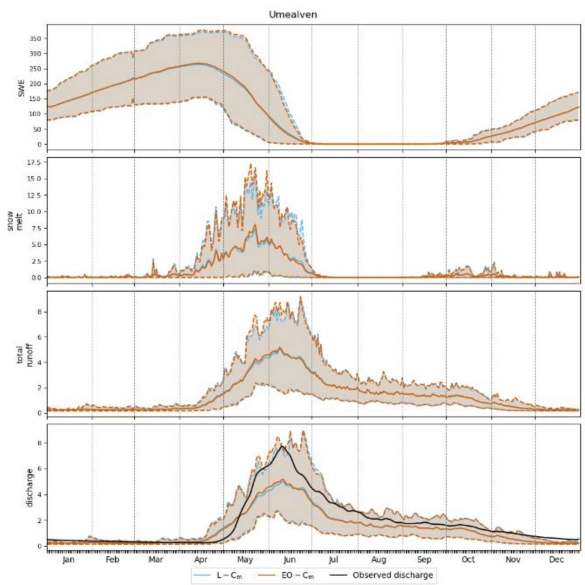









Guadalfeo
Laborec
Morrumsan
Salzach
SWE
snow melt
total runoff
discharge
Jan
Feb
Mar
Apr
May
Jun
Jul
Aug
Sep
Oct
Nov
Dec
$L - C_m$
$EO - C_m$
Observed discharge



Metrics Reporting: The interpretation of metrics (e.g., RMSE, KGE) lacks depth. What does a specific improvement mean in operational or hydrological terms? A summary table of relative improvements across basins would aid comparison.

We thank the reviewer for the comment. Given the limited differences between the 2 performances of the LISFLOOD model we do not believe that an extra table is necessary.
We will include in the result a better description of the metrics. Such as: Bias is practically identical, which means that water is not stored nor lost between the two runs. The correlation coefficient is slightly worse in some catchments, which means that the time of the peak flows is slightly hindered. The variability has mixed outcomes.
The decreased performance in correlation is the most significant in operational terms. A lower correlation coefficient suggests that the timing of peak flows is somewhat less accurately captured. This is particularly important in the operational context of EFAS5, where changes in correlation directly impact the system's ability to anticipate or delay peak flows, an aspect that is critical for effective flood hazard communication and early warning.
It should be stressed however that model outputs from the LISFLOOD run using the EO-snowmelt coefficient were not recalibrated. If the model were to be used operationally, it would undergo a calibration round where 13 parameters ought to be calibrated, the original ones (14) minus the snowmelt coefficient. After the re-calibration of the model, the new metrics should be analyzed in terms of their impact in operational terms.

L295–300: This methodological content appears out of place in the results section, indicating a need for clearer structure throughout.

We thank the Reviewer for the comment. We will move the paragraph to the Methodology section.

L325–385: The discussion should better engage with existing literature on snow data assimilation. It is widely recognized that improvements in snow state representation do not always lead to improved streamflow prediction. This should be acknowledged and contextualized.

We thank the reviewer for the comment; however, our study does not assimilate the SWE state into the model. The SCA is converted into SWE and this value is used to calibrate the snowmelt coefficient at pixel level, as further explained in the previous answers. We will clarify better in the revised version of the manuscript.

L345 & L380: The SWE–SCF conversion is treated superficially. Other formulations exist and should be discussed. Additionally, the distinction between calibration and data assimilation should be made clearer, especially if the authors position their method as a calibration approach.

Thank you for the comment. We have already provided additional details on the SWE–SCF parameterization in our previous response. In the revised manuscript, we will better justify the choice of the Swenson and Lawrence, (2012) formulation and mention alternative approaches available in the literature.

Regarding the distinction between calibration and data assimilation, we position our method as a calibration approach, since the snowmelt coefficient is statically optimized and kept constant across multiple hydrological years. That is, we use EO-SCF to calibrate a model parameter (the snowmelt coefficient), rather than dynamically adjusting the model state during runtime.

However, we also recognize that the methodology could be extended to a data assimilation framework, in which EO-SCF is assimilated in near real-time to update model states based on the observed EO-SCF.

Model Structure: The limited impact of improved melt factors on discharge suggests structural limitations in LISFLOOD (e.g., degree-day assumptions, decoupling of snow and runoff). These issues deserve more attention in the discussion.

We thank the reviewer for the comment.
The degree-day method is a very simple, conceptual approach that is broadly used in other large-scale hydrological models, such as PCRGlob, CWatM or mHM. Even though the model might benefit from an improved representation of the snow processes, we believed that, given the model scale and purpose, LISFLOOD is able to satisfactory capture the main processes. We will,

however, mention in the manuscript that more sofisticated models should be tested in future, also taking into condiration the higher resolution of this model version (1', ~1.4 km) compared to the previous one (5km).
Regarding the limited impact of the EO-cm on discharge, we believe that this is generally true when looking at the discharge at the station outlet (with some seasonal differences in Salzach, Arve, and Alpenrhein basins). As shown in the Normalized Euclidean Distance plots, local differences in discharge are present upstream.

L408–409: This conclusion is not strongly supported by the preceding results and should be revised or qualified.

We thank the reviewer for the comment. We agree with the comment, and we will amend the conclusion accordingly. Our study highlights that calibrating with Snow Cover Area from EO can improve local dynamics, but that for the scale of the basin analyzed the impact on river discharge simulation is comparable with the parameters estimated by the traditional LISFLOOD calibration.

The manuscript would benefit from careful revision for clarity, structure, and language. Sections are often dense and overly technical, with insufficient explanation of key decisions. A clearer narrative structure, consistent terminology, and simplified figures would greatly improve readability.

We fully agree with the Reviewer that the manuscript can be further improved thanks to their constructive comments. We hope to have the opportunity to revise it accordingly, based on these responses.