

Revision of the revised 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.

Dear Editor and Referees,

We sincerely thank you for the time and effort you devoted to reviewing our manuscript again. We are particularly grateful to Anonymous Reviewer R1 for providing numerous valuable and constructive comments, and to Reviewer R3 for their careful and detailed feedback. We have carefully considered all comments and addressed them accordingly. We believe that these revisions have further improved the quality of the manuscript.

In detail, the major revisions are as follows:

- We have broadened the state-of-the-art discussion and clarified the positioning of our study within the existing literature.
- We have emphasized that our study represents a parsimonious calibration approach, focusing on the estimation of a single parameter—the snowmelt coefficient—using EO-based snow cover fraction (SCF) data, and we assess the effect of this post-replacement on the model setup.
- We have improved the discussion to better address the limitations of the study.
- Necessary corrections have been made to the equations as suggested.
- We have better justified the way we compute the parameter k_{accum} in the snow cover parametrization and acknowledged the limitations in the manuscript.

Our detailed point-by-point answers to the comments by Anonymous Reviewer R1 and Anonymous Reviewer R3 is following **in red**. We also sincerely thanks Dr. Francesco Avanzi for his very positive feedback and appreciating the improvements.

Sincerely,

Valentina Premier and Francesca Moschini on behalf of all co-authors

Answers to Anonymous Reviewer R1

We warmly thank the anonymous Reviewer #R1 for taking the time to carefully review our manuscript once again. We are particularly grateful for the very detailed comments, which we have carefully considered and which have helped us improve the quality of the manuscript.

General Comments

The revised version represents progress: structure and clarity have improved, and some figures are better presented. Nevertheless, substantial revision is still required before this paper can meet HESS publication standards.

Thank you for acknowledging the improvements and effort made in the first revision.

The manuscript continues to lack clear scientific positioning, conceptual depth, and interpretative discussion. While the topic (linking Earth Observation (EO) data with large-scale hydrological modelling) is relevant, the paper does not yet demonstrate a well-defined scientific contribution or clear novelty relative to existing work on EO-driven or multi-objective calibration of degree-day snow models.

The Introduction remains too brief and descriptive. It lists references but does not sufficiently articulate the research gap, justify the approach, or explain how it advances the state of knowledge. This section needs a fundamental rewrite to strengthen conceptual framing and situate the study within the broader literature.

We have revised the Introduction and expanded the state-of-the-art to better position our work. Our method is framed as a parsimonious calibration, focusing on a single parameter, in line with previous studies suggesting that fixing certain parameters and calibrating only specific ones can achieve results comparable to fully calibrated models (Ruelland, 2023). In this study, we specifically evaluate the effect of a post-replacement of the snowmelt coefficient.

We believe our study is significant, as it offers a viable and computationally efficient alternative to full recalibration by improving a specific module. Compared to most state-of-the-art works, which—despite using multi-objective calibration—rely on lumped coefficients or elevation-dependent coefficients, our approach provides a **pixel-based coefficient**. While previous studies have also considered pixel-varying coefficients (Gyawali and Bárdossy, 2022; Asaoka and Kominami, 2013), in our view they do not fully exploit the snow cover fraction information, as they rely on snow cover duration or binary snow cover data.

Moreover, the high-resolution SCF product used in this study represents an added value, which we will clarify in the detailed responses, although we acknowledge that

operational products could be used for large-scale applications. Given the operational and continental context of LISFLOOD, we agree that the manuscript primarily represents an applied, operational study, in fact as previously indicated it was initially submitted as a “technical note.” However, we still believe that it constitutes a relevant contribution to the journal’s scope, both for the reasons outlined above and for the valuable analyses enabled also by the reviewers’ constructive comments.

Methodologically, several aspects remain ambiguous or insufficiently justified: the rationale for the MODIS downscaling/reaggregation workflow, the temporal averaging of snowmelt coefficients, the calibration–control procedure, and the regionalisation approach applied to some basins.

While figures have improved, some still add limited insight and require better readability.

We have addressed these concerns in the specific comments to the Reviewer.

At a deeper level, the core scientific question is weakly posed. The study tests whether a spatially variable snowmelt coefficient improves snowmelt representation relative to a fixed, discharge-calibrated coefficient. However, since the spatial coefficients are derived from EO snow data, this result is almost self-evident. Reported improvements are small and within model uncertainty, with no clear evidence of enhanced discharge skill or reduced equifinality.

We would like to clarify that the aim of this study was not solely to assess whether snowmelt representation was improved, as we agree that this result may appear self-evident. The main objectives were, first, to evaluate the current LISFLOOD setup, which is particularly relevant given the operational context of the model, and second, to investigate whether modifications to this specific parameter—the snowmelt coefficient—affect the hydrological model following its post-replacement.

Our findings align with previous studies (He et al., 2014), which reported relatively modest improvements in streamflow but greater gains in reproducing more realistic snow cover patterns. Notably, our analysis also reveals local differences in discharge, as highlighted by the NED analysis, which we consider a relevant outcome of this study. This suggests that model performance can depend on the spatial scale at which the processes are evaluated.

The discussion remains mostly descriptive, lacking physical interpretation. The authors should explain more clearly why spatial variability in the snowmelt coefficient is physically justified (e.g. slope, aspect, solar radiation) and to what extent the observed patterns reflect structural limitations of LISFLOOD, notably its limited treatment of elevation-dependent precipitation.

We have addressed these concerns in the specific comments and have also revised the Discussion section based on your suggestions.

Finally, while writing has improved, the manuscript still requires careful language editing for conciseness and fluency.

In summary, the paper addresses an important topic but still needs major revision to clarify its contribution, justify its methods, and deepen its discussion.

We have revised the manuscript according to the Reviewers and Editor suggestion.

Specific Comments

L6–7: Melt factors in degree-day models vary spatially and temporally with topography and energy fluxes. Some models use radiation-indexed melt factors. Please consider mentioning in the introduction section.

Thanks, we agree and we modified L62-65 in this way “While the standard LISFLOOD setup relies on a lumped C_m constant for each sub-catchment disregarding temporal and spatial variability, our approach tests whether C_m can be estimated pixel-wise from Earth Observation snow cover fraction (EO-SCF) data after standard streamflow calibration. This approach isolates the effects of snowmelt parameterization without recalibrating the entire model.”

L10–11: Round numerical values; excessive decimal precision implies false accuracy.

Thank you for pointing this out. We have revised the text and all relevant tables to round numerical values to 0 decimal places when reporting SCF and SWE errors (Tables 4, 5, B1). This rounding is consistent with the accuracy of the underlying variables and avoids implying false precision.

L12: “Highlighted an effect” is vague—please specify the nature of the effect.

Thanks for the comment. We rephrased the sentence as “In general, the optimized coefficients did not significantly change the simulated discharge at the basin level in terms of KGE, but their application led to noticeable divergences in discharge within smaller upstream catchments.”

L13–14: Clarify whether the reported change in snow dynamics affected discharge simulations.

As stated in the previous answer, and rephrased for improved clarity, the optimized coefficients did not significantly change the simulated discharge at the calibrated outlets, in terms of KGE, but their application led to noticeable divergences in discharge within smaller upstream catchments. Given these findings, we believe that it is interesting to keep the analysis related to Fig. 15 (see comment below). Nonetheless, the improved representation of snow cover led in some cases to shifts in the timing and

magnitude of snowmelt and total runoff, relevant for water resources management applications. This is reflected in the monthly KGE figure (Fig. 14), where some catchments exhibit some significant changes in terms of KGE.

L16–25: Modern calibration rarely relies on discharge alone. Multi-objective calibration (snow + discharge) should be acknowledged consistently in the introduction and discussion.

We thank the reviewer for the comment. We have expanded the Introduction to provide more context on multi-objective calibration and have acknowledged the use of multi-objective approaches incorporating snow data. Note also that the type of calibration approach strongly depends on the model type, the data availability and the geographical context of the model. In this study we focus on LISFLOOD, a model calibrated at continental (EFAS) and Global (GLOFAS) scale where the classical calibration approach is normally used.

L32–34: Clarify your positioning relative to the wide range of multi-objective calibration approaches (SWE, snow depth, SCA; sequential vs simultaneous; optimisation strategies). The introduction should reflect this diversity and specify your scientific contribution.

Our approach cannot be fully classified as a multi-objective calibration, as it involves the direct estimation of a single parameter—the snowmelt coefficient—following a standard streamflow-based calibration of the full model. In this sense, our method is closer to the approaches proposed by He et al. (2014) and to previous studies that considered pixel-varying snowmelt coefficients (Gyawali and Bárdossy, 2022; Asaoka and Kominami, 2013). However, in our view, these studies do not fully exploit the information content of snow cover fraction data, as they rely primarily on snow cover duration or binary snow cover observations. Therefore, our analysis - that has been shaped as a post-replacement of a specific parameter estimated through EO based products - provides the basis for a potential two-step calibration framework, in which streamflow calibration is complemented by a targeted, observation-based estimation of snow-related parameters. We have revised the Introduction accordingly to better reflect the diversity of existing multi-objective calibration approaches and to clarify the specific scientific contribution of our study.

L36–40: What specific efforts are referred to here? Please elaborate rather than listing references without explanation.

Thank you for this comment. Here, we list studies in which the authors estimated spatially varying snowmelt factors rather than using lumped values, in line with our approach. Specifically, Asaoka and Kominami (2013) applied a degree-day model incorporating snow disappearance dates derived from 10-day composite Satellite Pour l'Observation de la Terre (SPOT)/VEGETATION data.

Riboust et al. (2019) improved a degree-day model by employing an optimization criterion based on both streamflow and snow cover, incorporating a hysteresis approach. They used MODIS data to estimate snow cover fraction across different elevation bands and tested several snow parameterizations, including the scheme adopted in our study from Swenson and Lawrence.

Gyawali and Bárdossy (2022) also presented a degree-day modeling study based on MODIS data, in which calibration was performed at the pixel level using binary snow-cover information from cloud-free days or selected time periods.

More recently, Ruelland (2024) tested different calibration strategies and robustness outcomes, addressing the practical question of when snow data effectively improve parameter identifiability and model robustness. Their results showed that the benefits depend on observation and forcing quality, model structure, and hydroclimatic conditions.

In this context, our contribution lies in the use of an optimization strategy based on daily snow cover fraction (SCF) derived from an improved, high-resolution EO product. This represents an advance compared to approaches relying solely on binary snow cover information or snow disappearance dates. Moreover, this study explores an alternative approach based on a post-replacement of the snowmelt coefficient that is suitable for continental-scale operational models.

We have revised the Introduction to better reflect this context and to address the points raised by the Reviewer. However, as a stylistic choice, we have aimed to maintain a balance between conciseness and completeness and therefore do not further expand the state of the art.

L38–40: This appears to be the central question of the manuscript. Calibrating snow parameters (including degree-day factors) based on snow-cover products independently of runoff parameters has already been examined (see suggested references). How does the present work differ from those? What is the scientific gap it aims to fill?

As already addressed in the previous response, we have expanded the Introduction to better position our work within the existing literature and have included the suggested references. This study can be framed as a direct and parsimonious single-parameter estimation approach. Specifically, we evaluate the effects of a post-replacement of the snowmelt coefficient, which is well suited for continental-scale operational models as it provides a simpler and computationally efficient pathway toward a more realistic representation of snow processes.

Moreover, we introduce an optimization strategy based on snow cover fraction derived from high-resolution EO data, which we believe represents an added value to the study. We acknowledge the concerns raised by the Reviewer on this point and address them in detail in the corresponding responses. In addition, previous studies have primarily focused on lumped snowmelt coefficient estimation or on coefficients varying across elevation bands, rather than on pixel-by-pixel optimization, as suggested by the Reviewer in the subsequent comment.

Another important aspect of this study is the inclusion of multiple river basins across Europe with diverse hydroclimatic and physiographic characteristics, whereas many previous studies have focused on single-basin case studies.

L43: The “pixel-by-pixel” basis might represent an original contribution compared with previous studies. This should be emphasised by better contextualisation.

Thank you. We have stated this more clearly in the Introduction (L55-56).

L46–47: Avoid forward references to results in the introduction.

We are not entirely sure which specific sentence the Reviewer is referring to. If the comment concerns the sentence “*The spatialized coefficient represents an important improvement over the standard LISFLOOD approach*”, this statement was not intended to anticipate or refer to the results, but rather to highlight the novelty of our work, namely that the output of the methodology is a spatialized coefficient, as also acknowledged by the Reviewer in a previous comment.

To avoid any potential ambiguity, we have decided to delete this sentence and to incorporate the same information at L63-64, rephrased accordingly, as also addressed in our response to the comment on L6–7.

Figure 1: The downscaling–reaggregation workflow (MODIS 500 m → Sentinel 50 m → 1 arcmin) is not clearly justified. Why not directly aggregate daily cloud-free MODIS images from 2000 onward to model scale?

Thank you for the comment. We have provided what we hope is a convincing response in our reply to the question concerning Appendix A below, where we discuss and illustrate the improvements that can be expected from a dataset derived from high-resolution imagery compared to a low-resolution product gap-filled using standard methods.

L65: Clarify why a “pixel-wise average over time” is used and which season is excluded from calibration.

Thank you for the comment. We rephrased the sentence (L88-90) as “Given the temporal stability of the obtained coefficient (see later in Sec. 4.1), a pixel-wise average

over time is then computed. One season (2022/23) is excluded from calibration and used only for evaluation.”

L76–77: Given the large number of studies using MODIS snow products, the statement “limiting their utility for hydrological applications” seems overstated. Methods exist to fill cloud gaps, and cloud-free products are available. Regarding spatial resolution, while 500 m may be coarse for some applications, it is adequate for others (e.g. models operating at similar resolution or elevation bands). In your study, since the model runs at 1 arcmin resolution, the MODIS data are degraded anyway: therefore, the limitation is not clearly justified.

Thanks for the comment. We revised the Section and changed the sentence with “To our knowledge, standard operational products rely on very basic gap-filling methods as multi-temporal filters, that do not consider the spatial context” (see L100-101).

L210: Unless I have missed it, the simulation periods were not previously defined. Hence, the term “final period” is difficult to interpret here.

We changed the sentence with “Subsequently, a mean value for each pixel is calculated using data from five seasons while one is used exclusively for a further independent assessment of the results.” and we stated more clearly in L301-302 “The last season 2022/23 is not used for the snowmelt coefficient calculation, but it is used exclusively for an independent assessment of the results.”.

L239: What is meant by “monthly basis”? If the model runs daily, why not compute KGE on a daily basis? What is the rationale for aggregating to monthly? If the intention is to assess the mean seasonal regime, then computing KGE on mean daily values would seem more consistent. Please clarify.

The KGE was indeed calculated using daily observed and simulated data, however in Fig. 14 (old Fig. 13), we aggregated the daily data for each month to better capture daily changes in each month of the year. We clarified this better in the manuscript (L269).

L261–262: Specify which anthropogenic activities (dams, abstractions, transfers) are modelled and how; otherwise, exclude these basins.

Water abstraction for agriculture, industry and civil uses is modelled in LISFLOOD as well as reservoirs. We specified that in Section 2.2 L128-131.

L264: I could not find any mention of the regionalisation approach in Section 2.2.1. However, you refer to using several gauging stations per basin for calibration. How many stations were used for each basin? Why do the Guadalfeo and Adige basins require regionalisation? Do they lack discharge observations? If so, why were they retained for model evaluation? These choices are unclear and could confuse readers.

The use of regionalization is mentioned in L145 “Parameters for ungauged basins are estimated using a regionalization approach (Beck et al., 2016), while coastal and endorheic catchments (area < 150 km²) use default values.” Observed river discharge was not available for the Adige and Guadalfeo basins at the time of EFAS5 calibration, hence, the regionalization approach was implemented instead. For this study we managed to retrieve observed river discharge, and we could perform the evaluation of LISFLOOD for both EFAS5 and the configuration with the EO-Cm. We have included this information in Section 3 “Test sites” L294.

Those 2 catchments were chosen in the study to assess also the regionalization methodology of EFAS, as stated there. We would like to stress that assessing model performance under regionalization is essential for the purpose of this study, as EFAS necessarily relies on such an approach when applied to ungauged areas.

L268–272: Should it be understood that the evaluation of snow dynamics against remotely sensed products is limited to 2017–2023, whereas discharge simulation with LISFLOOD covers 1992–2023? What is the calibration period? Are there independent evaluation periods? This is presented in a very confuse manner and should be more rigorous. Please clarify.

Regarding the EO products, as we state in L300, “the EO dataset spans six hydrological years, from October 1, 2017, to September 30, 2023, corresponding to the period with maximum availability of Sentinel-2 data (both Sentinel-2A and Sentinel-2B).” We added this further information “The last season 2022/23 is not used for the snowmelt coefficient calculation, but it is used exclusively for an independent assessment of the results.” to make clearer that one season is used for evaluation of the results only.

EFAS5 was calibrated using a variable calibration/validation period, which depends on the total length of the timeseries and the period of data availability. However, the final KGE reported in the operational system is however calculated on the full observed timeseries.

The daily averages of SWE, snowmelt and discharge in our study are also evaluated using the full observed discharge timeseries, as stated in Section 2.5, L262. The start and end dates are reported in Fig. 13.

(See https://european-flood.emergency.copernicus.eu/efas_frontend/#/home under evaluation – model performance and <https://confluence.ecmwf.int/display/CEMS/EFAS+v5.0+-+Calibration+Data>)

Since the focus of the study was not on the LISFLOOD calibration, but on the effect of the recalibration of the melting coefficient using EO data, we do not think that including this information in detail for each station would be relevant. We added in L139-143

(section 2.2.1 “EFAS5 Setup”) that EFAS goes through a calibration routine and that KGE was calculated for all available observation data.

Figure 2: Please include scale bars on each map to enable comparison of basin areas.

Thank you for the comment. We added the scale bars to the figure.

Figures 3–4: The legend for C_m is too small and difficult to read. Since it appears in each panel, please enlarge it and place it outside the Adige panel, at the bottom of the figure. The black-to-yellow colour scheme is not intuitive and poorly represents gradual C_m variations. A red gradient (light to dark, $0\text{--}10\text{ mm }^\circ\text{C}^{-1}\text{ day}^{-1}$) would be potentially clearer. Also, please add basin outlines to aid spatial interpretation (especially where white areas occur, e.g. Guadalfeo in Fig. 4).

Thank you for noticing it. We have updated Fig. 3, 4 and B1 accordingly to your suggestion.

L282: The statement “compensate for erroneous precipitation inputs” raises the question of whether elevation-dependence of precipitation should instead be explicitly treated, as done in other snow and glacier models. Discuss whether C_m compensates for underestimation of high elevation precipitation rather than true melt variability.

We thank the reviewer for this observation. We acknowledge that precipitation is subject to substantial uncertainty, as already discussed in the Discussion section (old L465–471). We have further emphasized this aspect in the revised manuscript (see response to the next comment). However, we believe that testing alternative elevation-band models with a more explicit treatment of orographic precipitation is beyond the scope of this study. We note that the operational model used in this work is LISFLOOD, and the objective of our analysis was to evaluate a specific model parameter, namely the snowmelt coefficient.

To provide additional context, LISFLOOD within EFAS uses meteorological forcing that combines station observations—often sparse at high elevations—with gridded and model-based data to improve coverage in complex terrain and data-scarce regions. Specifically, datasets from research projects (e.g., CarpatClim, EURO4M-APGD) and operational products such as ERA-Interim are integrated into the CEMS Meteorological Data Collection Centre as so-called “virtual stations” (see for details the paper of Thiemiig et al., 2022 that we have also cited in L136 – see Table 2 for the list of the datasets that form the input for EMO-5). These data significantly enhance coverage over complex terrain, including the Alps, as documented also in the EFAS v5.0 Wiki (see EFAS v5.0 meteorological forcing - Copernicus Emergency Management Service - CEMS - ECMWF Confluence Wiki). In Figure 1 of the Wiki is shown that particularly the Alps are covered by these gridded data.

While we acknowledge that this remains an open research topic, we believe that the EFAS approach is expected to be more robust than explicitly prescribing an elevation–precipitation dependence by adjusting precipitation through a simple linear relationship with elevation (as we interpret the Reviewer’s suggestion).

L287: This now clarifies the use of a “temporal average” but very late (see earlier remark). The temporal stability of C_m probably reflects systematic precipitation-forcing errors with elevation. In particular, poorly represented orographic gradients and snow under-catch (due to coarse model resolution and lack of dedicated parameters) likely explain the spatial variability in C_m , which compensates these errors. The influence of slope and aspect appears secondary here.

We have modified the sentence in L89 (see earlier answer). We also agree with the Reviewer meaningful observation, and therefore we have added it to L332-334 “The observed correlation, together with the temporal stability of the snowmelt coefficient, may also suggest a systematic elevation-dependent bias in the precipitation forcing.” as well as in L518-521 in the Discussion Section “The temporal stability of the EO- C_m (Fig. 5) likely reflects systematic elevation dependent biases in the precipitation forcing. In particular, poorly represented orographic gradients and snow undercatch—arising from coarse model resolution and the absence of dedicated parameters—likely explain the spatial variability in EO- C_m , which compensates for these errors”

L295–296: Highlight that this reflects model structural limits, not physical variation in melt.

Following your previous comments, we have added this reflection to L332-335.

L296–297: The correlation with elevation is positive for flat basins, but is it statistically significant? Report significance (p-values) for correlations.

We have added p-values to Table 3 only for cases in which they exceed 0.05, indicating a lack of statistical significance. These instances correspond to correlations that are already close to zero.

L302: If I understand correctly, this L- C_m configuration is not entirely lumped, since it may depend on sub-catchments (see Fig. 3). Please state this clearly.

Yes, you are right. It is lumped for given sub-catchments that are defined based on the availability of streamflow data. For some of the analysed basins, they are further divided in sub-catchments with possible different values of the snowmelt coefficient. This was already mentioned in old L144 “When multiple stations are available within a catchment, the calibration process follows a cascading approach: the basin is divided into sub-catchments, and each is calibrated sequentially from upstream to downstream.” However, we remarked this also in L309 of the revised manuscript “L- C_m

are lumped at the sub-catchment scale within the major basin, with subcatchments defined according to the availability of streamflow data for calibration, as explained in Sec. 2.2.1.”

L386–387: Dashed lines add little information and make the figure harder to read. Figure 12: This figure contains too many curves and colours, reducing legibility. Avoid dashed lines and retain only three curves per panel (observed; L–Cm; EO–Cm).

We thank the reviewer for its comment. However, we believe that it is important to include the q10 and q90 in the figure to give an indication of the variability of the snow/hydrological components and if and how more extreme value were affected by the new coefficient, to improve readability we removed the dashed lines.

L404–409: Including this basin seems questionable given its strong influence from dam operations, which invalidates interpretation of snow-process sensitivity. Consider removing it from the analysis.

We thank the reviewer for the comment, however we respectfully disagree that the Guadalfeo basin should be excluded on the basis that dam operations invalidate the snow-process interpretation.

As stated in the full paragraph the reduced model performance in this basin arises from two factors: the use of regionalized parameters rather than locally calibrated ones, and the temporal inconsistency in reservoir representation.

This uncertainty does not imply that snow processes are irrelevant or that dam operations physically dominate the basin response. Rather, it indicates that model structural assumptions and parameter transfer both affect performance in ways that cannot be separated with the available information. For this reason, we consider it more appropriate to retain the basin and explicitly document these limitations than to remove it and thereby implicitly attribute the poor performance to a single physical cause.

Table 6: It is unnecessary to include the subcomponents of KGE here. They are numerous and do not enhance the comparison between L–Cm and EO–Cm.

We thank the reviewer for the comment, however we believe it is important to include the KGE subcomponents in order to show if there is/isn't compensation among the component of the KGE. Depending on the application of the model the KGE components do have different importance. E.g. in flood forecasting correlation is more important than variability, whereas bias is more important than correlation in water resources applications.

L428–438: I recommend deleting this section (and the corresponding Figure 13). It adds little to the comparison between L–Cm and EO–Cm and presents low KGE values indicating poor discharge performance.

We thank the reviewer for this suggestion. Figure 13 (now 14) together with Table 6, shows how an equivalent performance in KGE (see Arve for example) does not result in an equivalent performance during snowmelt. At the same time a decreased overall performance (Laborec) is driven by a significantly decreased performance in March. Finally, a decreased overall performance could hide a slightly increased performance in some months and a decreased one in other months (see Alpenrhein).

L439–452: Likewise, I suggest deleting this section (and Figure 14). It is overly technical and does not provide convincing or insightful results for the comparison.

We thank the reviewer for the suggestion. We note, however, that the purpose of this analysis is to illustrate how the EO-Cm impacts river discharge upstream of the calibrated station. The limited impact of EO-Cm at the basin outlets demonstrates that, with the current calibration, LISFLOOD can reproduce the catchment-average hydrological dynamics. Upstream of the calibration station, however, this is not the case, and the analysis quantifies where and how much discharge can differ solely due to changes in the snowmelt coefficient. Given that LISFLOOD outputs are available at European and global scales, these findings provide potential users with valuable information on how to interpret and trust simulated discharge in snow-influenced catchments.

L454: The study aims to test whether spatially variable snowmelt coefficients improve snowmelt representation compared with fixed coefficients calibrated against runoff. Since the spatial coefficients seem preliminary calibrated against remotely-sensed snow cover, the research question seems somewhat trivial, and the answer self-evident.

We changed the text with “This study tested the snow module of LISFLOOD and evaluated the outcomes of a post-replacement of the snowmelt coefficient with EO-calibrated values after a standard streamflow calibration. The approach improves the representation of snow cover without deteriorating hydrological performance and without requiring a full, time-consuming model recalibration.”

L455: “Enhances realism of what?” Please specify.

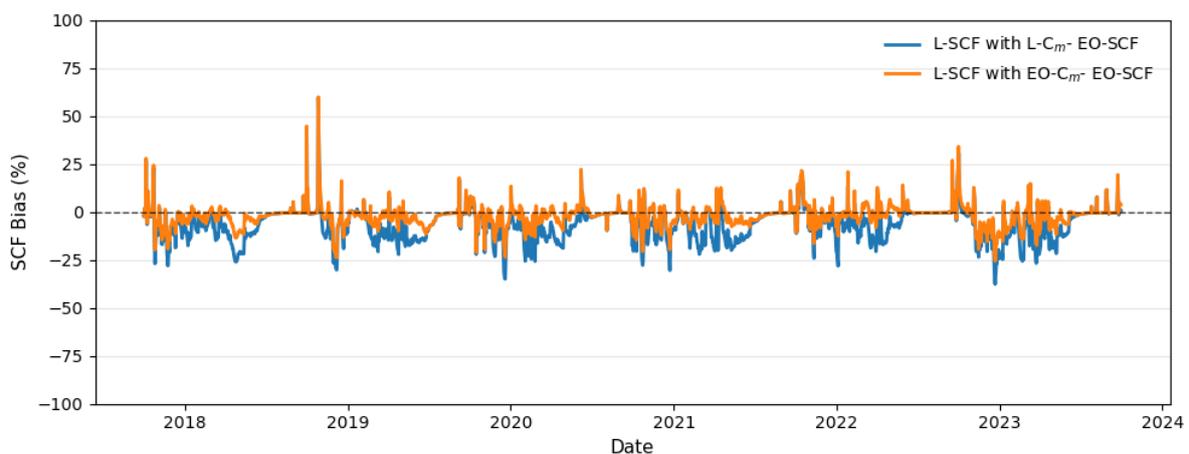
We meant that the model improves snow cover representation. We changed it with “The approach improves the representation of snow cover...” (see previous answer).

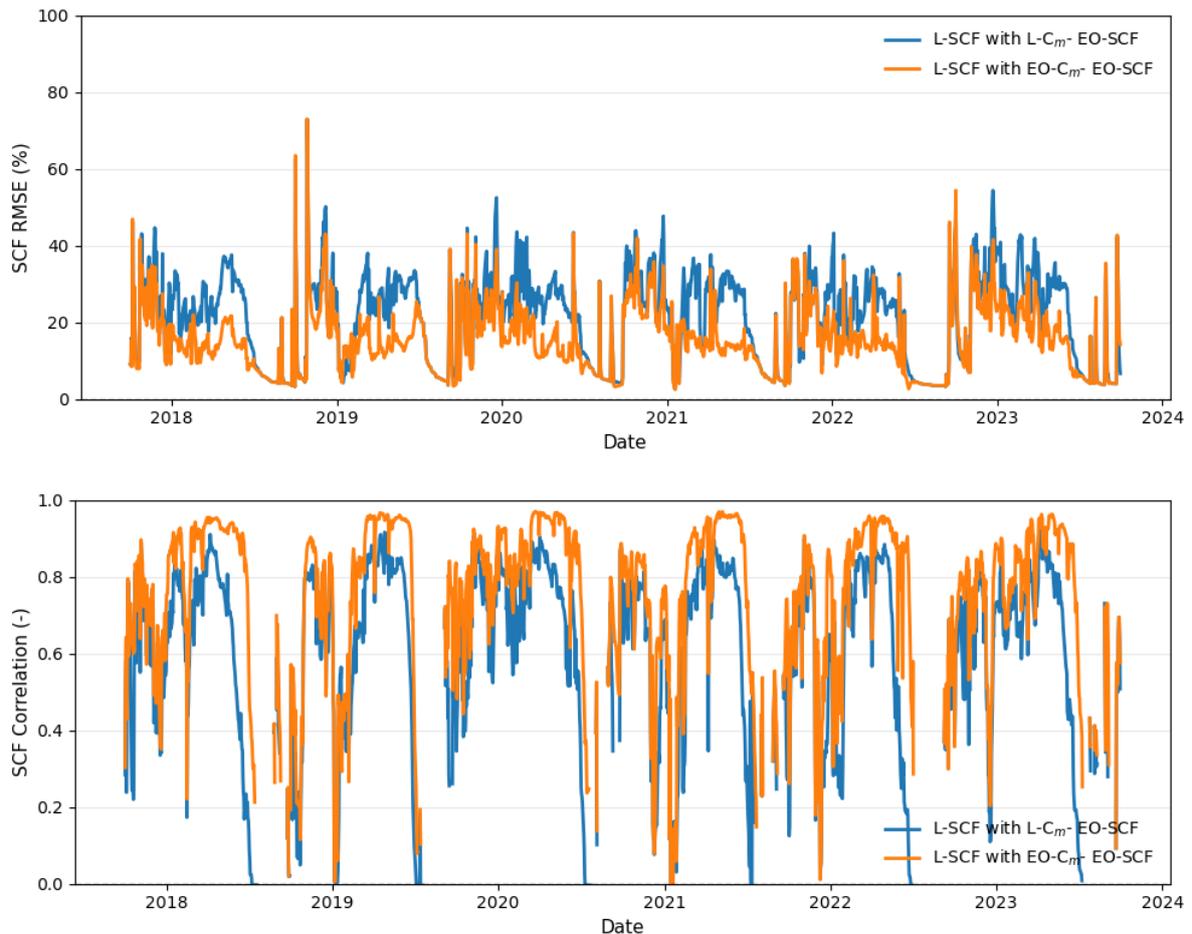
L456: The procedure appears lengthy and computationally demanding (downscaling MODIS with Sentinel, upscaling to 1 arcmin, multi-year spatial calibration of Cm) for an improvement that is debatable. There is no clear evidence that snow cover, SWE, or discharge are better simulated. The added spatial variability in Cm introduces more parameters without clear gains in predictive skill, internal consistency, or uncertainty reduction.

We acknowledge that we have introduced additional complexity and computational demand to the procedure. However, we deliberately chose to build our study on a dataset that we believe has higher accuracy compared to standard operational products. For further details on this aspect, we refer to the answer to the comment on Appendix A, while an evaluation of the underlying methodology can be found in Premier et al. (2021). This choice also represents, in our view, one of the novelties of the work, as highlighted by other Reviewers during the first revision round. The limited improvements, which are of course not questionable, are not related to the use of one snow product rather than another, but rather confirm the already good performance of the LISFLOOD snow module prior to recalibration. We consider this an important result in itself. Nevertheless, we do claim a better fit with the observed snow cover fraction during the melting phase (see next comment). Finally, we provide in Appendix A insights on the use of standard operational products, which can represent a viable alternative for future applications with reduced complexity and computational demand.

L458–461: It seems that this statement is not supported by the presented results.

This behaviour is already evident in Fig. 6, where the orange snow cover (new coefficient) shows a closer agreement with the reference (black curve) compared to the cyan curve (old coefficient). To further highlight this improvement, we report here—focusing on the Alpenrhein basin for brevity—the temporal evolution of the performance metrics (bias, RMSE, and correlation) instead of the SCA. It is possible to appreciate improved metrics especially in the melting phase, while the errors remain the same in the accumulation phase since we change the snowmelt coefficient only, thus not affecting the performance during the accumulation. Consequently, bias and RMSE remain in general relatively high both prior to and following the snowmelt coefficient replacement, and thus do not fully capture the improvements introduced.





L465–471: Other operational models can be applied at similar scales (1,000–10,000 km²) with greater realism in mountain environments, as they explicitly account for elevation-dependence in meteorological forcings. Compare explicitly with elevation-band models that better treat orographic precipitation.

Thank you for the comment. However, as also mentioned in our response to the comment on L282, we believe that a comparison with elevation-band models is beyond the scope of this work, for the reasons explained above. To further clarify our reasoning, we would like to add an additional consideration that may not have been sufficiently clear: EFAS is a continental-scale, operational model. The spatial scale considered is not on the order of 10,000 km², as for the largest catchment analyzed in this study, but rather on the order of 10,000,000 km². This scale inherently limits the spatial (and temporal) resolution of the model. To our knowledge, there are currently no large-scale operational models running at substantially higher spatial resolution. To avoid ambiguity in the use of the term “large-scale,” we have replaced it with “continental” throughout the text.

Moreover, L182–185 describe the subdivision of pixels into elevation bands used to correct temperature, which is the main driver of snowmelt. This correction affects only

temperature, as there is a physical basis supporting the use of a constant temperature lapse rate of $0.0065\text{ }^{\circ}\text{C}/\text{m}$. In contrast, no similarly robust and spatially consistent relationship exists for precipitation. Therefore, applying an elevation-based correction for precipitation at the continental scale is not straightforward and was not considered appropriate in this study.

L473–474: Cite studies showing that SCA-based calibration alone provides limited constraint.

We have revised the paragraph (see from L522).

L475: What is meant by “required for a more robust calibration of accumulation processes”? Please clarify and suggest how this could be achieved.

We have rephrased that part. See from L522.

L481–485: Mention other models incorporating solar radiation for variable melt factors.

Thank you we added to L537 the sentence “These limitations are intrinsic to temperature-index models and may be mitigated through enhanced temperature-index formulations or energy-balance models (Ruelland, 2023).”

L488: Clarify whether timing and magnitude changes impacted discharge; results suggest otherwise.

Thanks for the comment. The EO-based coefficient modifies the seasonal phase of snow processes, which in some basins leads to small seasonal shifts in the timing and, to a lesser extent, the magnitude of discharge during and immediately following the snowmelt period. However, these effects are localized in time and do not translate into meaningful differences in integrated or daily discharge performance, as reflected by the minimal changes in KGE and daily mean discharge across the analysed catchments.

We clarified that better in the manuscript.

L492–495: Please support this statement with appropriate references.

We have added an appropriate reference.

L497–499: You have not demonstrated improved discharge simulations (higher KGE) or reduced equifinality.

In fact, we are not referring to demonstrated results, but rather to a reasoned expectation of what might be achieved with a two-step calibration, first constraining the snowmelt coefficient using EO data, followed by recalibration of the remaining parameters. This is why we used cautious phrasing in the sentence, such as “This suggests” and “could achieve.” We agree that this hypothesis would need to be formally

demonstrated; however, based on the outcomes of our study, we do not foresee worse performance.

L500: I am also not convinced that internal model consistency has improved. The presented results do not substantiate this claim.

We agree with the Reviewer that the improvements are not drastic; however, the snow cover representation is improved in the melting phase (see response to comments at L458–461). This improvement is also reflected in Table 4. We would also like to remind the Reviewer that these metrics are computed by omitting SCF values equal to zero in both the model and the observations, which we believe is a meaningful choice and results in more representative, albeit more conservative, performance metrics.

L513–516: Discuss correlation with elevation and relate findings to existing EO-constrained modelling studies.

We have expanded the Discussion section according to this and other comments. Issues related to elevation dependent errors are discussed following your comment to L282.

L539–540: If snow accumulation and melt timing or magnitude were truly affected, one would expect a corresponding influence on runoff timing. How do you explain that discharge performance remains similar across approaches? Are snow dynamics genuinely impacted?

We acknowledge the Reviewer's point that changes in snow accumulation or melt timing would typically be expected to influence runoff timing. In our case, such implications can indeed be observed in some basins, for example in the Alpenrhein. However, the overall similarity in discharge performance across approaches can be explained by the fact that the applied methodology primarily affects the snowmelt coefficient and therefore the partitioning and timing of melt at the local scale, whereas the KGE is calculated at the basin scale. As a result, local changes in snow dynamics may be averaged out or smoothed in the aggregated discharge signal, leading to limited differences in global performance metrics. The differences in discharge are present upstream the calibration station, as highlighted in Fig. 15.

L540–541: This formulation comes from Kirshner (2006) (please cite). Beyond this reference, do you consider that your model achieves the “right answer” (discharge simulations) for the “right reasons” (internally consistent snow dynamics)? If so, why? If not, why not?

Thank you for the comment. We have added the reference. We believe that, thanks to the post-replacement of the snowmelt coefficient, snow cover representation is more “right”, as evidenced by the better agreement between simulated and observed SCF.

This is demonstrated in the manuscript (see Table 4), which shows reduced bias and RMSE, as well as improved correlation across all basins. If we assume the observations—acknowledging their inherent uncertainties—are reliable, this indicates an improvement in the “realism” of the snow module. At the same time, the fact that discharge performance is not deteriorated was not entirely expected. We have extensively discussed this point in our previous responses to the Reviewers and in the manuscript. So, yes, we are getting a “right answer” for a “right reason” that is a better snow cover representation.

L542–543: “... our findings suggest that a sequential calibration approach—first adjusting the snowmelt coefficient using EO-derived SCA, followed by post-calibration of streamflow—can be a viable and potentially more efficient alternative.” More efficient compared with what?

We apologize for the confusion. Here, we intended to convey that the approach is not necessarily more efficient, but rather a more strategic alternative to the current EFAS setup, which relies on standard streamflow calibration. Accordingly, we have revised the sentence to: “...can be the correct approach to pursue.”

L544–545: What concrete improvement does this approach bring? Is the added complexity justified by demonstrable gains in model performance, internal consistency, or uncertainty reduction?

We agree with the Reviewer that the improvements are marginal. An important and transparent outcome of this study is precisely that the proposed methodology does not substantially alter the overall results—neither through significant improvements nor through notable degradation—which we explicitly acknowledge and discuss in the manuscript. This limited impact was not obvious, as the approach does not involve a full recalibration of the model and thus a deterioration of the performances in terms of streamflow could also be expected (see Discussion L479).

Nevertheless, we argue that the method provides a more internally consistent representation of snow processes as shown in Table 4. In particular, the snowmelt coefficient is calibrated by directly minimizing the mismatch with observed snow cover, which leads to a better alignment between simulated and observed snow dynamics.

Regarding the added methodological complexity, this point is addressed in our response to the comment on L456.

L554–618: Appendix A is rather long and highly technical. The products differ in many subtle ways (spatial resolution, temporal depth, revisit frequency, etc.), which makes a direct comparison difficult to present within an appendix. Given the spatial resolution used in LISFLOOD (≈ 1.4 km), it is unclear why snow cover area data derived from the

daily 500 m MOD10A1F MODIS snow product was not used. MOD10A1F is a cloud-gap-filled version of MOD10A1 that provides daily, highly accurate snow-cover estimates from 2000 onward (Hall et al., 2019). I suggest using these products rather than downscaling MODIS with Sentinel data and then upscaling to 1.4 km. Your approach is difficult to justify because it reduces the available temporal depth due to the limited historical record of Sentinel, casts doubt on the ability to gap-fill cloud-contaminated products given Sentinel-2's limited temporal frequency, and does not add a spatial-resolution benefit since snow cover is ultimately aggregated to 1.4 km for your study.

We thank the Reviewer for again raising this point, which we acknowledge may not have been sufficiently clarified in the first revision round. We agree with the Reviewer that products with different spatial resolutions are inherently difficult to compare directly. When using high- versus low-resolution sensors, different spatial footprints are sampled, and this is precisely why MODIS (500 m resolution) is not well suited for capturing snow cover dynamics in complex mountainous areas, where snow distribution varies at much finer spatial scales, invalidating the Nyquist-Shannon theorem of sampling.

The use of low-resolution imagery results in a non-linear mixture of multiple surface components within a single pixel (e.g., forest, clouds, mixed snow and snow-free areas, different illumination conditions). Past research has demonstrated that spectral information can successfully resolve mixing issues through unmixing procedures (e.g., Painter et al. 2009). However, as Aalstad et al. (2020) note, while the 500 m operational MOD10A1 and MYD10A1 products perform reasonably well, 'a higher resolution merged operational unmixing product would yield marked reductions in error' (Aalstad et al. 2020). For this reason, we developed an internal methodology at Eurac that enables snow cover detection at a substantially finer sampling resolution, which represents one of the key novelties of this work.

An important aspect that should be emphasized is that our approach exploits the strong information content provided by the repetition of spatial patterns observed at high resolution using Sentinel-2 imagery. This characteristic allows us, in principle, to reconstruct snow cover conditions retrospectively for periods when Sentinel-2 data are not available, potentially through integration with Landsat data. In the present study, however, we deliberately limited the analysis to the Sentinel-2 era to ensure better data coverage and reliability, and in light of the relative stability of the snowmelt coefficient over time, as demonstrated in the manuscript.

Moreover, our approach differs substantially from conventional gap-filling techniques that rely solely on temporal propagation of information. Instead, it explicitly incorporates spatial context by learning from historical pattern repetition, enabling a more advanced and physically meaningful gap-filling of cloud-covered areas based on an appropriate spatial sampling of snow. In fact, our ongoing research in accordance

with Aasltad et al. 2020, suggests that even Sentinel-2 resolution may be insufficient in highly complex terrain, where very high-resolution imagery (e.g., WorldView or Planet) would be more appropriate. However, such data are clearly not feasible for continental-scale applications.

We believe that the comparison with other snow products presented in the Appendix remains meaningful. At the same time, we fully agree with the Reviewer that for faster, large-scale, and operational applications, users may reasonably rely on established products such as MOD10A1F or VNP10A1F.

To further illustrate the improvement of our product with respect to MOD10A1F, we could provide two illustrative examples for the Adige River basin. We do not intend to claim that our product is free from errors, nor that this comparison is exhaustive; rather, the aim would be to highlight specific strengths of our approach and, in this way, better address the Reviewer's concerns. We have chosen to omit this additional discussion from the main manuscript, as we believe it would go beyond the scope of the present study.

Moreover, as stated at old L92, we refer to other products as “alternative” options, and we do not claim that users should necessarily adopt our algorithm in all cases. In fact, at the outset of this study, we expected a stronger improvement in LISFLOOD performance resulting from the assimilation of the snowmelt coefficient. Given the relatively limited sensitivity observed, we believe that the overall results and main conclusions would not be substantially affected by the use of a different snow product.

Nevertheless, we also acknowledge that the accuracy and depth of the analysis depend strongly on the spatial scale and the specific application. As shown in our results, basin-scale analyses appear largely unaffected, whereas local effects become more evident at finer spatial resolutions. It is therefore at the pixel scale that the choice of a more advanced algorithm, rather than a simpler one, may meaningfully influence the results.

We analyzed the snow cover area (SCA) time series for the Adige River basin for the hydrological year 2018/2019. Fig. 1 below shows our SCA estimates compared with those derived from MOD10A1F. Overall, a good agreement is observed; however, local differences can be identified, likely related to the gap-filling procedure adopted by MOD10A1F, which is based on a multitemporal filtering approach (notably affecting some periods during the melting phase).

We further examined two dates showing pronounced discrepancies, for which Sentinel-2 imagery was available as a qualitative reference: 03/11/2018 and 19/04/2019. For both dates, MOD10A1F underestimates the SCA compared to the Eurac product. This underestimation is particularly evident in areas affected by cloud cover, as shown in the

Sentinel-2 false-color composites, suggesting that MOD10A1F does not fully reconstruct snow information under cloudy conditions, leading to an overall underestimation of snow-covered areas.



Figure 1 Snow Cover Area (SCA) for the Adige basin. In blue, the SCA derived from the gap-filled Eurac product; in orange, the SCA derived from the MOD10A1F product.

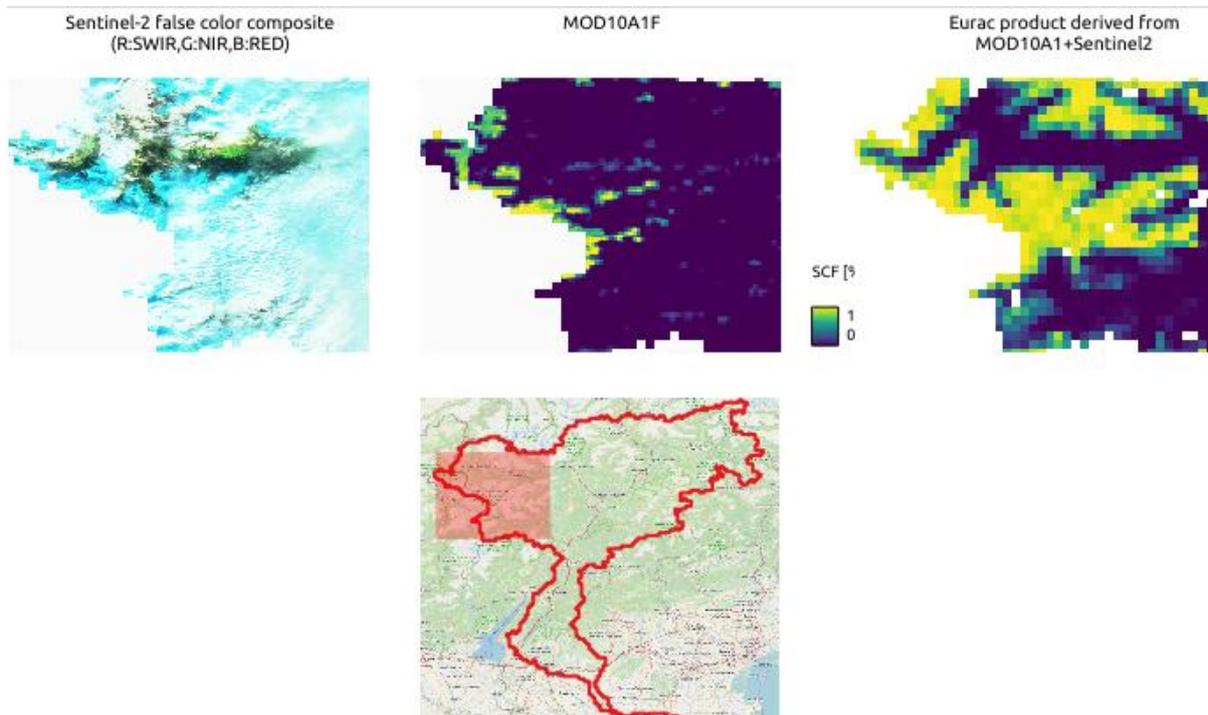


Figure 2 Qualitative comparison between MOD10A1F and Eurac product against the Sentinel-2 false color composite for 03/11/2018.

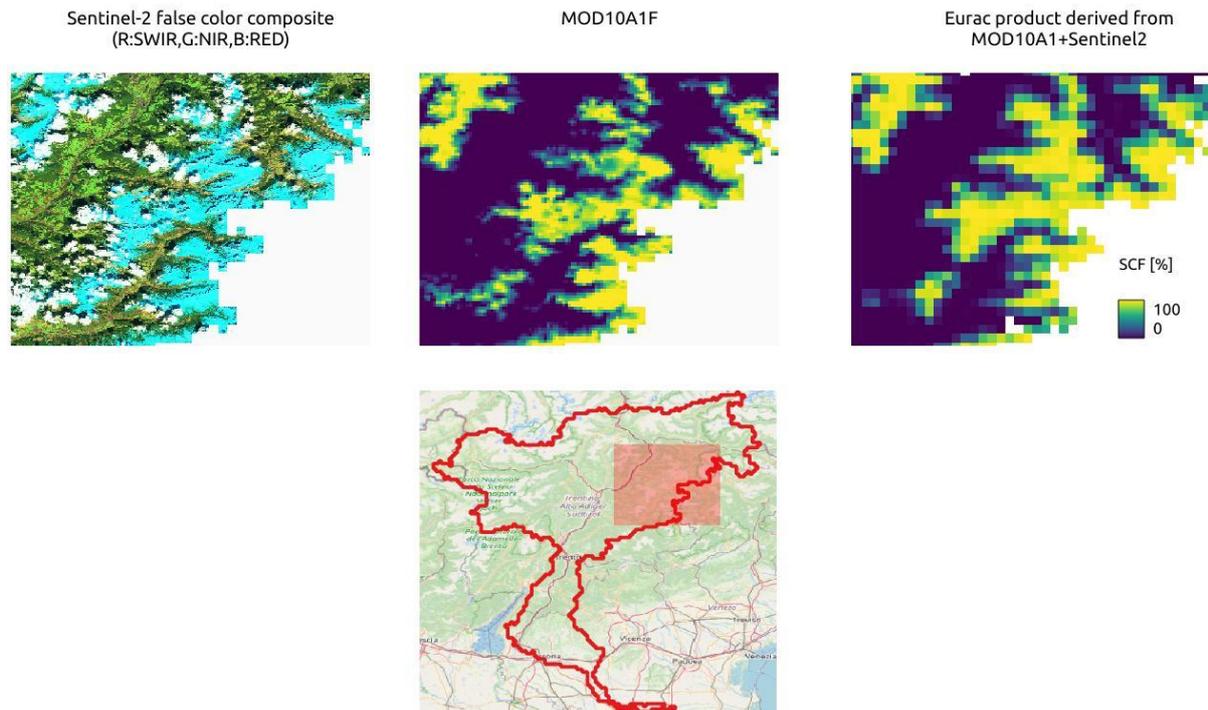


Figure 3 Qualitative comparison between MOD10A1F and Eurac product against the Sentinel-2 false color composite for 19/04/2019.

References

Aalstad, K., Westermann, S., & Bertino, L. (2020). Evaluating satellite retrieved fractional snow-covered area at a high-Arctic site using terrestrial photography. *Remote Sensing of Environment*, 239, 111618.

Painter, T. H., Rittger, K., McKenzie, C., Slaughter, P., Davis, R. E., & Dozier, J. (2009). Retrieval of subpixel snow covered area, grain size, and albedo from MODIS. *Remote Sensing of Environment*, 113(4), 868-879.

L620–636: This comparison is interesting. The formulation of Zaitchik and Rodell (2009) is clearer and simpler than that of Swenson and Lawrence (2012). It does not require tuning free parameters because τ and SWE_max are fixed values. Given that both formulations produced comparable performance in your tests, you should rather favour the Zaitchik and Rodell (2009) formulation.

We agree with the Reviewer that the two formulations led to similar results; for some basins, one parametrization performed better, while for others the alternative did. There is no strong justification to favour one over the other. Selecting the appropriate formulation was one of our main concerns. On one hand, we agree that simpler formulations are generally preferable. On the other hand, we believe that a more complex parametrization allows for better adaptation of the methodology to diverse topographical conditions. And however, the only parameter that required tuning was k_{accum} .

We also note that we favored the parametrization of Swenson and Lawrence (2012) because it is employed in a widely used model such as CLM. As explained in Sec. 2.3, this parametrization is more flexible: it differentiates between accumulation and melting phases and computes SWE_{max} iteratively.

L627: change SCF_{max} to SWE_{max}.

Thank you for noticing it. We changed it.

L640–661: This section is central to the manuscript and should be included in the main paper rather than relegated to an appendix. Equation C2 shows that the computation of EO-C_m depends strongly on the precipitation estimate. Orographic precipitation errors largely explain the spatial variability of the EO-C_m that is ultimately computed.

We thank the Reviewer for this comment, but we believe there may be a misunderstanding regarding the purpose of this Appendix. This section was moved to the Appendix because it describes an alternative methodology previously developed by Pistocchi et al. (2017), which differs from the approach adopted in the main text of this study. As stated in old L655-656 regarding the method of Pistocchi et al. (2017) “overall, the results indicate poorer performance compared to the optimization-based approach adopted in this study, and in some cases also relative to the standard EFAS5 calibration”. For this reason, we believe that our choice to adopt a more recent optimization-based approach for calibrating the EO-derived snowmelt coefficient is fully justified.

While in the first version of the manuscript this method was also part of the main body, relocating this material from the main text to the Appendix has improved the readability of the manuscript by simplifying the presentation of results and figures. Retaining this section in the main text would have required the introduction of additional acronyms and comparisons, making the manuscript more difficult to follow, as also noted by the Reviewer in the previous round of review.

In the main text, we therefore focus on the optimization-based method, in which the snowmelt coefficient is estimated by solving an optimization problem that minimizes the error between L-SCF and EO-SCF (see Section 2.4). In this case, there is no direct link between the EO-C_m estimation and the precipitation estimates. Nonetheless, we agree with the Reviewer’s previous observation that C_m might compensate for underestimation of high elevation precipitation rather than true melt variability.

Anyway, we decided to remove Appendix C from the manuscript, accordingly to your suggestion.

L646: Please provide the full derivation and all details of the equation to help readers understand the contribution of each component.

We have removed the Appendix C.

Appendices C-D: Appendices C and D are highly technical and are presented in a somewhat confusing manner; they are at times unnecessarily redundant with the main text. As currently presented they create more confusion than clarity about the manuscript's aims. Please remove, simplify and/or clarify these appendices.

Following your suggestion, we removed Appendices C and D. As we considered the information previously reported in Table D1 to be relevant—since it contains the metrics for the evaluation season 2022/23—we moved this content to the main text and presented it in graphical form rather than as a table, in order to improve readability (see Fig. 7 in the revised manuscript).

L668–671, L672–678, L679–698: Author contributions, competing interests, acknowledgements, and references are misplaced within the appendices. Please move them to their correct locations.

Thank you for the comment. However, we believe that it is not worthwhile to focus too much on figure placement at this stage, as the final layout will differ in the event of acceptance, when the manuscript will be formatted. We will check this detail with the editorial team responsible for the final formatting.

Suggested bibliography on the topic <https://doi.org/10.1029/2010WR009824>

<https://doi.org/10.5194/hess-21-3325-2017>

<https://doi.org/10.5194/hess-18-4773-2014>

<https://doi.org/10.1016/j.jhydrol.2023.129867>

<https://doi.org/10.1029/2011WR010559>

<https://doi.org/10.1016/j.jhydrol.2024.130820>

Thank you for the suggestions. We have included them in the Introduction.

Answers to Anonymous Reviewer R3

The major revision significantly improved the paper. You addressed:

- The resulting KGE of discharge, without recalibrating the other parameters
- The three layers of the Lisflood snow module
- The ice melt integration
- 2.3. Snow Cover Parametrization is now much better explained
- A daily comparison in fig 12,13

The workflow as fig 1 is beneficial.

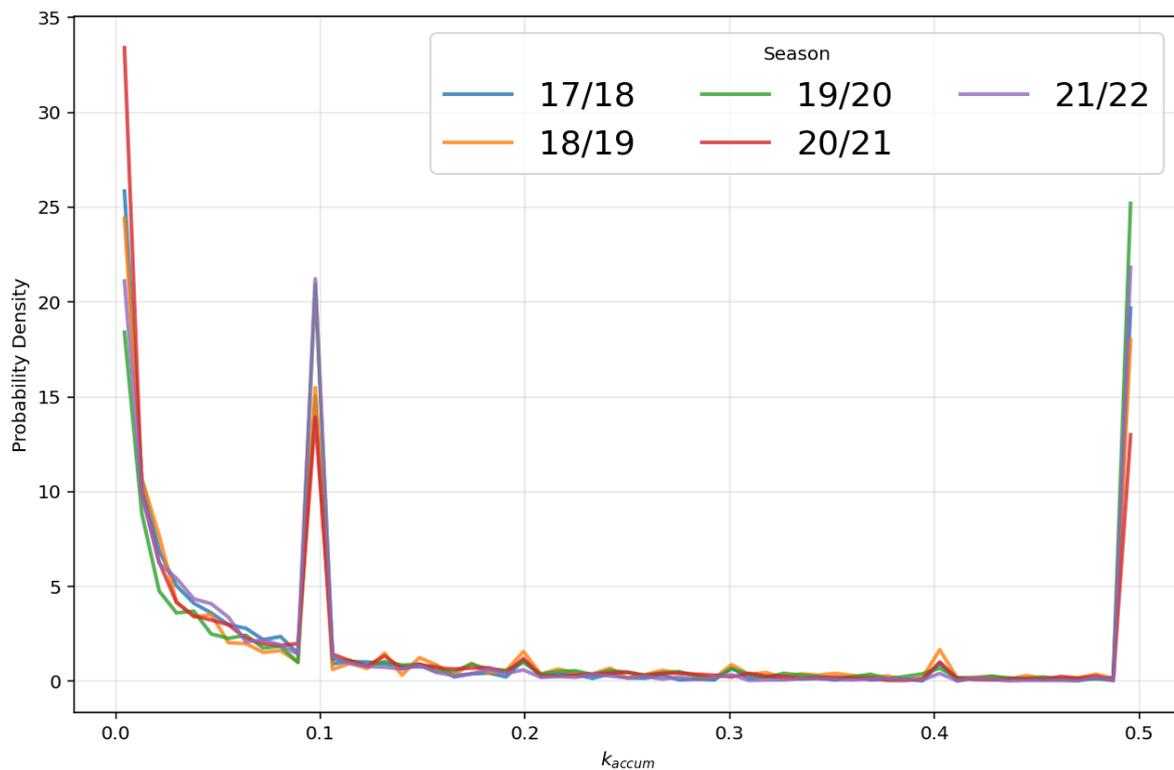
We warmly thank again Anonymous Reviewer R3 for providing meaningful comments that have greatly helped us improve the quality of our manuscript.

You put in some lines about how k_{accum} is calculated. Still unclear to me, how you select the first day. "... in which both EO-SCF observations and LISFLOOD SWE are greater than 0". If Lisflood SWE reacts later than EO, you will already have a higher SCF. Also, the first snow in the Alps occurs Sept-Nov, which usually does not stick and may not represent the winter season. Maybe some more sentences in Appendix B2 will help.

Thank you for this thoughtful and constructive comment. In our approach, we define the first day of snow accumulation as the first day on which both EO-SCF observations and LISFLOOD SWE are greater than zero, regardless of the time of year and the snowpack conditions. We acknowledge that this definition may not always be optimal, particularly in cases where LISFLOOD SWE responds later than EO-SCF or when early-season snowfall (e.g., September–November in the Alps) does not persist and may not be representative of the winter snowpack.

Your observation regarding potential mismatches between the timing of satellite observations and model responses is valid. However, from a qualitative perspective, we found a general agreement between EO-SCF and LISFLOOD SWE during the accumulation phase (see also Fig. 6). In addition, averaging k_{accum} over five seasons helps to smooth the impact of timing mismatches or short-lived early snow events. We also found a relatively stable behavior of k_{accum} across different seasons—similar to what was observed for C_m —which supports our choice of using a temporal average. For brevity, we report here only one example of the probability density distribution of

kaccum for the Adige basin.



Furthermore, when examining basin-averaged values of k_{accum} (Fig. B1), we find that the estimated values are broadly consistent with those reported by Swenson and Lawrence, 2012. We also observe systematically higher k_{accum} values in flatter basins such as Mörrumsån and Umeälven, which are characterized by less complex topography. In these areas, higher k_{accum} values correspond to higher SCF for a given SWE, consistent with the representation of flatter pixels.

In our view, introducing additional empirical rules to constrain the timing of the first snowfall would have increased methodological complexity without necessarily improving the robustness of the results. Furthermore, during an initial testing phase we also evaluated the use of a constant value of 0.1, as suggested in the literature, and found no substantial differences at the basin scale, although some local pixel-level effects were observed. In future work, we may consider optimizing k_{accum} as well within the proposed workflow. Nevertheless, we acknowledge this limitation and have clarified the limitation both in the Appendix B2 in L712 as well we have mentioned the limitation in the Discussion L526.

Another issue is the independence of the independent variable when calculating the objective function (KGE). Here, you use EO SCE to calculate k_{accum} , which you then use in equation 7 to calculate Lisflood SCE, which you then test against EO SCE. Therefore, the independent variable is no longer independent. I think, because it is only

a minor aspect of the Lisflood SCE, you can do this, but it would be worth mentioning in the discussion that you violated this independence criterion a little bit.

Thanks for the comment. Rather than the KGE that is calculated on streamflow and hence on an independent variable, we agree with the Reviewer that when calculating bias, RMSE and correlation (Table 4) the independent variable is no longer independent. We added this sentence to L340-345 “Note that, since the conversion from SWE to SCA is performed through a parameterized approach (see Section 2.3) that relies on the parameter k_{accum} , which is derived from EO-based SCF, the two SCA estimates compared in this evaluation are not fully independent variables.”

My biggest concern is equation 11 (sorry for not pointing it out in the first review). You inverted equation 9, then put SWE_{max} back into it. This is mathematically correct but does not make to much sense. The original idea of Swenson and Lawrence 2012, and Luce and Tarboton 2004 was a seasonal maximum. Swenson and Lawrence 2012 account for a period of mixed accumulation and melt in their equation 11 (page 12) <https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2012JD018178> Maybe using this equation instead of your equation 11. BTW the equation is wrong. The second part should be divided from the first part (in utils.py you did it right)

Thank you for carefully checking the equation. You are indeed correct that there was an error in how the equation was reported in the manuscript. The correct formulation is obtained by inverting Equation 9 and solving for SWE_{max} .

$$SWE_{max} = \frac{2SWE}{\cos(\pi(1 - SCF)^{1/N_{melt}}) + 1}$$

We apologize for reporting the incorrect equation in the paper; however, the correct formulation was implemented in the code (utils.py), as you rightly noticed. We sincerely thank you for this careful verification.

We acknowledge that some confusion may also arise because the original reference paper (Equation 11 on page 12) reports a different formulation. We contacted the original authors, who confirmed that there was an error in their manuscript as well, and that the correct way to derive SWE_{max} is indeed by inverting Equation 11 in our manuscript. This is also consistent with their reference implementation (<https://github.com/ESCOMP/CTSM/blob/master/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90>).

Regarding the question of circularity, we clarify that Equation 9 is computed as a function of $SWE(t)$ and $SWE_{max}(t)$. After a new snowfall event, SWE_{max} is updated in such a way that the previous snow cover fraction (prior to snowfall) is preserved along the depletion curve. In other words, the SCF used in the inverse formulation refers to

SCF(t-1). Since the SCF in the inverse equation is taken from the previous time step, the formulation is not circular.

We corrected Eq. 11 and we added the clarification about circularity in L225-227.

Line 102: 1 arc-minute grid (approximately 1.4 km) 1 arcmin grids = $1.852 \times 1.852 \times \cos(\text{lat})$ (in the Alps $\sim \rightarrow 1.852 \times \cos(47^\circ) = 1.26$. Doesn't have to be stated like this, but not 1.4 km

Thank you for this careful observation. You are correct that, considering our test sites, the spatial resolution ranges from approximately 1.48 km for the basin located at lower latitude (about 37° for the Guadalfeo) to approximately 0.75 km for the basin located at higher latitude (about 66° for the Umeälven). Accordingly, we have removed the statement in old L102 and added this information in Section 3, L304: "ranging from ≈ 1.48 km for the Guadalfeo to ≈ 0.75 km for the Umeälven."

Fig 3,4, B1: The legend is very small and not easy to read.

Thanks for the comment. According also to R1 comment, we have updated those figures.