Review paper egusphere-2025-2157 (Reviewer Francesco Avanzi)

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

By Premier et al.

We thank Dr. Francesco Avanzi 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.

Premier and colleagues have presented an improved calibration approach for the LISFLOOD snow module that is based on leveraging high resolution satellite data. This approach minimizes the error between observed and simulated snow cover fraction, and directly impacts the simulation of SWE. Authors test this method across a variety of study catchments in Europe and provide a detailed analysis of the improvements in snow simulation, as well as an interesting water balance perspective.

This paper is technically sound, and the effort of using high resolution satellite data into a large scale hydrological model is relevant and interesting for HESS. At the same time, there remain some aspects that could be expanded and improved. I still see value in this manuscript, and thus I am recommending a major revision.

We thank the Reviewer for recognizing the importance of the topic and for providing valuable feedbacks.

My main point is that using snow data in addition to streamflow data in calibrating a hydrologic model has been widely explored (see for example https://www.tandfonline.com/doi/abs/10.1080/01431161.2010.483493, https://www.sciencedirect.com/science/article/abs/pii/S0022169419312132?via%3Dihub, https://www.sciencedirect.com/science/article/abs/pii/S002216941300320X, https://hess.copernicus.org/articles/26/5627/2022/hess-26-5627-2022.html, https://www.sciencedirect.com/science/article/abs/pii/S0022169424013167). As a result of significant research in the hydrologic community over the last 15 years, a multi-objective calibration that involves at least snow and streamflow data is now considered state of the art. Meanwhile, even doing so does not necessarily imply an improvement in model performance.

We acknowledge that multi-objective calibration has been widely explored in the literature. In the revised manuscript, we will better position our work within the context of state-of-the-art approaches, including citations to the references provided by the reviewer in the Introduction section.

As clarified in our response to Anonymous Reviewer 1, our work addresses the methodological question: should multi-objective calibration (e.g., streamflow + SCA) be pursued, or are alternative strategies that aim for a more realistic representation of SCA feasible?

Our findings suggest that a sequential calibration strategy—where the snowmelt coefficient is first calibrated upstream using EO-derived SCA, followed by downstream calibration on streamflow—can offer a viable and potentially more efficient alternative to full multi-objective calibration. Furthermore, we show that a standard calibration based solely on streamflow, followed by a targeted post-adjustment of the snowmelt coefficient using SCA information, can still achieve acceptable performance without the need for recalibrating the full model.

This manuscript fits in this state of the art and confirms most of the conclusions above. To me, the most interesting points here are the use of high resolution satellite data and the inclusion of a variety of catchments, with different snow climatologies and various hydrologic characteristics.

Thanks for appreciating the work we have done in this direction. Using high resolution satellite data and a variety of catchments, we believe we have provided robust evidence for our claims about independent, ex post calibration of snowmelt coefficients.

In the revised manuscript, I would invest more effort in leveraging this variety of cathments as a way to draw process-based conclusions from this study that could allow for generalization: how are these catchments representative of specific snow climates? What do differences in results across these catchments tell us in terms of hydrological processes and the applicability of this approach in ungauged regions?

We thank Dr Avanzi for raising this key point. Our results indicate a dependency of model performance on catchment characteristics. As also discussed in our response to Reviewer 1, we observe differences in performance across basins depending on the climate/physiographic features. For the sake of brevity, we present here the results in terms of BIAS and RMSE for SCF derived from EO-Cm,2 (Figures 1 and 2 below). These performance metrics are evaluated against selected physiographic features (mean elevation, forest coverage, and slope) and climatic features (mean precipitation, temperature, and snowfall). Additionally, we distinguish glacierized and non-glacierized basins using different colours.

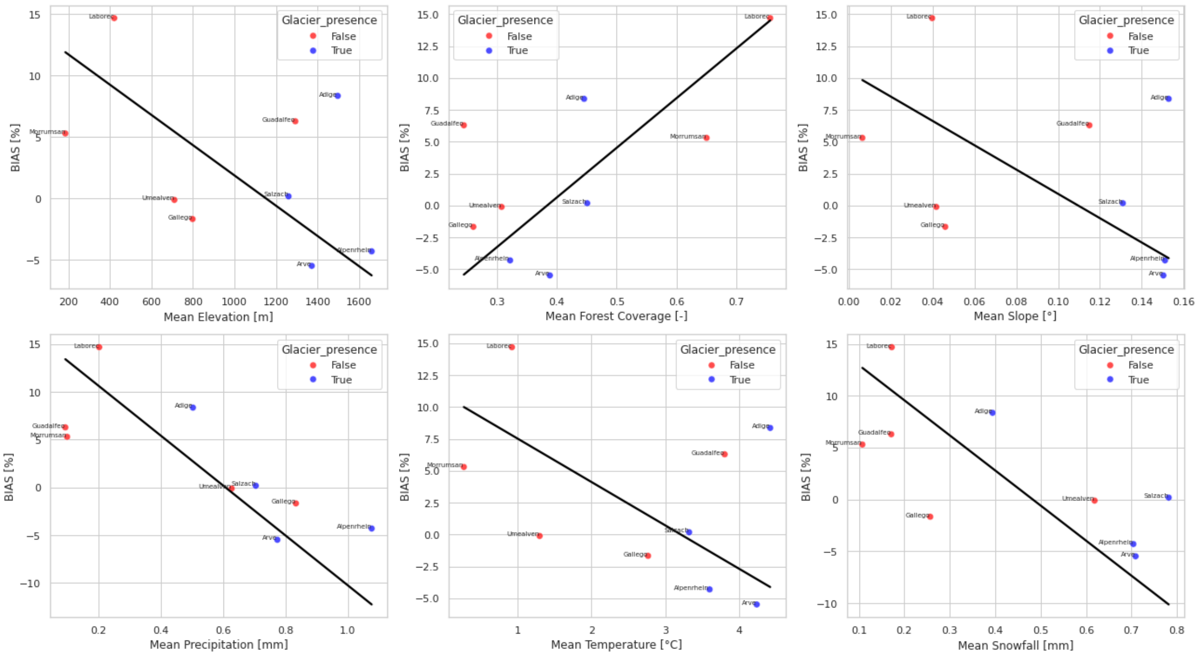


Figure 1 Performances in terms of SCF BIAS versus different physiographic and climatic features when using EO-Cm,2.

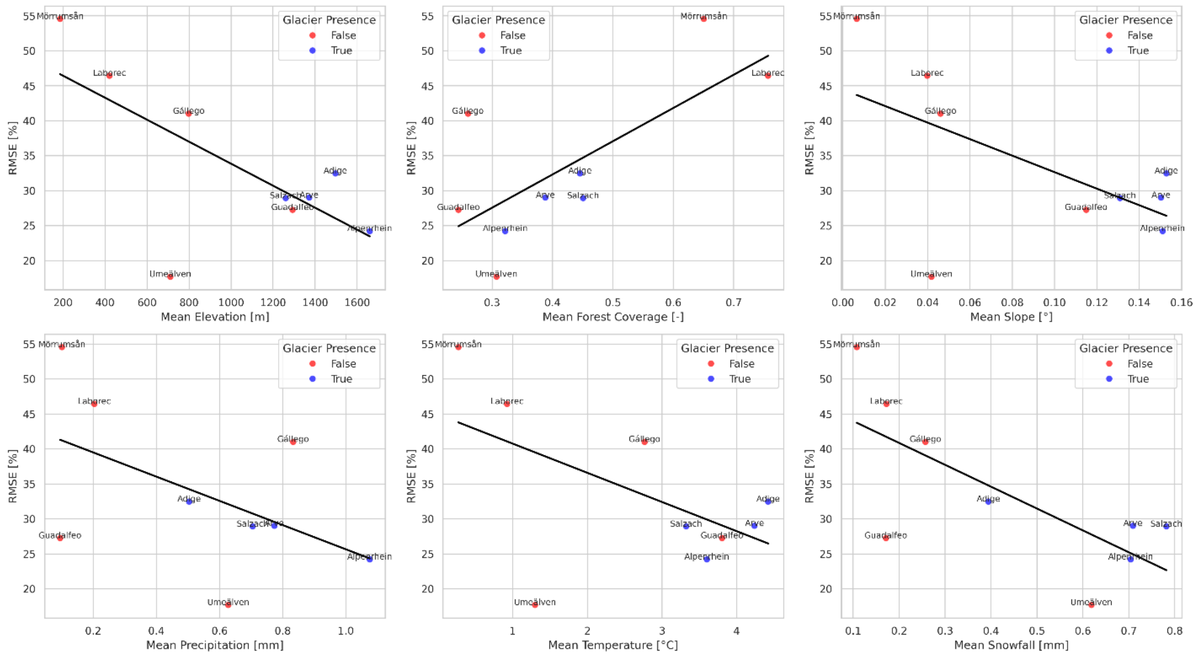


Figure 2 Performances in terms of SCF RMSE versus different physiographic and climatic features when using EO-Cm,2.

Our analysis reveals a trend of higher errors in lower-elevation and flatter catchments, and a similar increase in error with higher forest coverage. Regarding the climatic features, an inverse relationship with RMSE is observed: basins with higher precipitation and snowfall tend to exhibit lower errors. This is consistent with expectations, as lower precipitation—particularly solid precipitation—typically leads to more ephemeral snow cover, resulting in a greater proportion of fractional snow-covered areas, which are more prone to detection and modeling errors. Glacierized catchments do not appear to differ substantially in performance compared to non-glacierized ones. One notable exception is the Umealven basin, which consistently appears as an outlier with significantly lower RMSE. This may be due to its prolonged and near-complete snow cover throughout the season, which likely reduces snow cover variability and modeling errors. However, we acknowledge that it remains difficult to draw direct associations between specific climatic conditions and performance outcomes.

Regarding the applicability of the method to ungauged basins, note that both the Adige and Guadalfeo basins are modeled using a regionalization approach. Despite this, our results show that LISFLOOD performs acceptably in terms of SCF for these basins. The improvements introduced by the new snowmelt coefficient (SMC) are comparable to those observed in the gauged basins, suggesting the method's robustness. However, as discussed in the original manuscript, the Guadalfeo basin shows significant underestimation of river discharge. This poor performance could be partially attributed to the limitations of parameter regionalization and/or model assumptions related to reservoir operations. Specifically, the Rules reservoir, which was opened in 2004, is included in the model throughout the entire simulation period, potentially introducing structural inconsistencies. While a more detailed assessment would require further investigation, we believe the proposed method is applicable to both gauged and ungauged basins. The improvements in snow representation appear to provide benefits in either case, particularly in enhancing the accuracy of the snow component without requiring full model recalibration.

Authors also consider a fairly long period of data, with several snow drought episodes. Maybe commenting results across extremes and average years could be another way of bringing about more novelty.

We thank Dr Avanzi for this important suggestion. As shown in the trends of SCA and SWE across Figures 4, 5, and 6 in the original manuscript, snow drought events appear to be reasonably well reproduced by both the original and the revised SMC—for example in the case of the Adige basin during the 2021/22 and 2022/23 seasons. However, in our view, a more detailed evaluation of snow drought representation would require the use of a temporally (or at least seasonally) varying SMC. This is because different characteristics that define snow droughts—such as thinner snowpacks, earlier onset of melt, and accelerated melt rates—are likely tied to different melt dynamics and would benefit from a seasonally adaptive calibration approach. In this study, we opted for a single optimized SMC averaged over five seasons, regardless of whether individual years experienced drought conditions or not. While a detailed exploration of snow drought processes is beyond the scope of this work, we agree that including some of these considerations could add value to the revised manuscript.

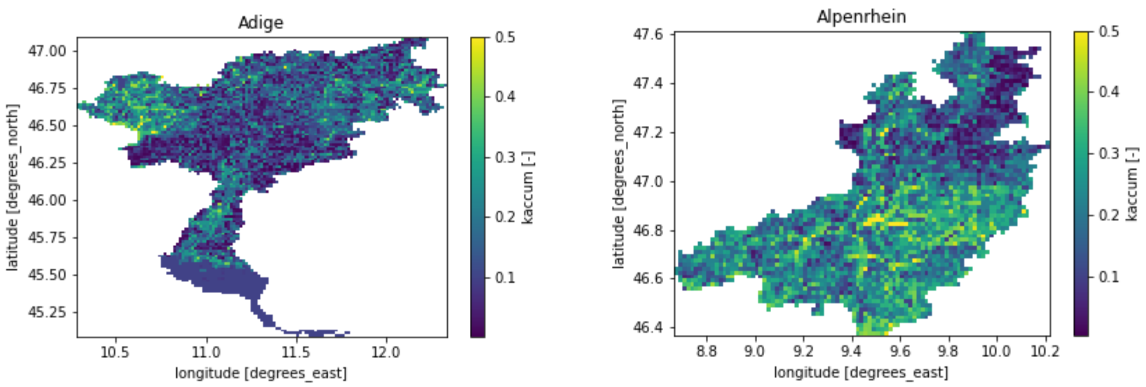
The Introduction and the Discussion section could be revised to (1) make the scope of the manuscript broader and (2) discuss how this methodology compares to previous attempts in this realm (see above).

We thank Dr. Avanzi for the comment. We will revise the Introduction according to this and other reviewers' comments.

I agree with authors that a calibration in terms of snow cover fraction is currently the only feasible approach at these scales, even though this requires the additional complication of a SCF parametrization to convert modelled SWE into modelled SCF. This is well discussed in the manuscript, with the only recommendation of providing some additional results on the calibration of the k constants.

We thank the Reviewer for this comment. As described in the manuscript and following the approach suggested by Swenson et al. (2012), *k_accum* can be estimated by analyzing *SCF* and ΔSWE during at the time of the first precipitation event over an initially snow-free pixel. This is done by inverting Eq. 8 of the manuscript and by replacing with $SCF^n=0$. In line with our approach for estimating Cm, we computed kaccum at the pixel level for each of the five "calibration" seasons. The values were bounded to a maximum of 0.5, and pixels for which the coefficient could not be determined were assigned a default value of 0.1. The resulting seasonal values were then averaged over time to obtain a single representative constant.

For the sake of brevity, we did not include these results in the current manuscript, but we present them in Figure 3 below. In the revised version, we might consider including these results in a new appendix to provide additional transparency and methodological detail.



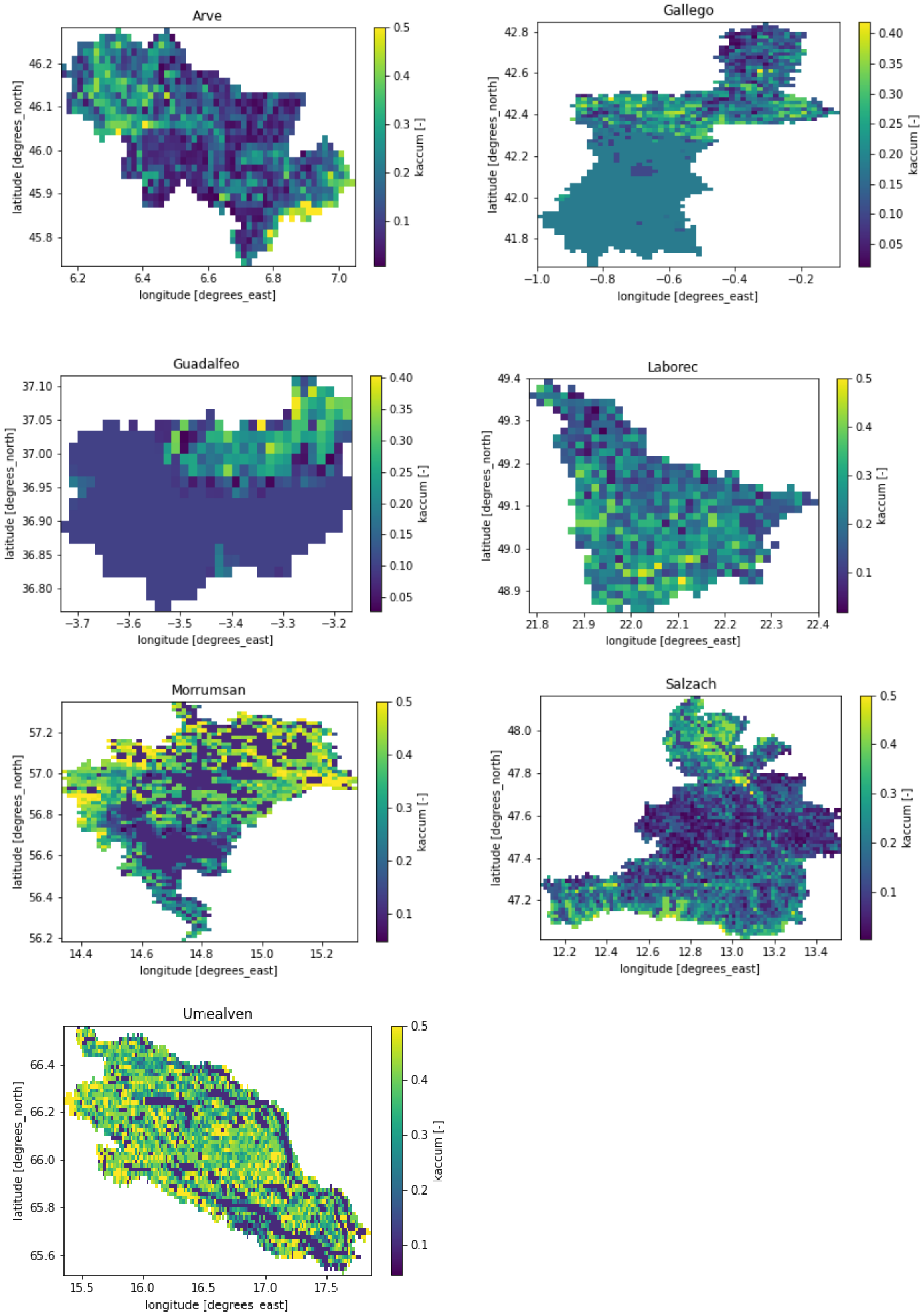


Figure 3 kaccum for the considered basins.

In general, the manuscript is well written.

We thank Dr. Avanzi for his valuable feedbacks.