Review paper egusphere-2025-2157 (Anonymous Reviewer 3)

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/her 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 et al. present a calibration method for simulating snow melt using the LISFLOOD hydrological model. The authors tested the method on a sufficient number of basins in Europe and demonstrated the benefits for SCF, SWE, and to a lesser extent, for water balance components.

In my view, the paper's novelty lies in the use of improved high-resolution snow cover data, combined with an SWE to SCF approach and an optimization algorithm. It seems to me that the topic is relevant and of interest to the journal, but it requires a major revision.

We thank the Reviewer for recognizing the importance of the topic and the novelty of the work. We believe that the manuscript can highly benefit from the provided feedback.

The paper is missing some points:

It appears that the authors have introduced the SCF calibration as an alternative to the discharge calibration. Stepwise calibration or data assimilation using snow data, soil moisture, and evapotranspiration has been performed quite often in the last decade. Even with the Lisflood model attempts have been made by Thirel et al. (https://www.mdpi.com/2072-4292/5/11/5825, https://www.sciencedirect.com/science/article/pii/S0034425712003604).

We acknowledge that the novelty of this work does not lie in the use of stepwise calibration or data assimilation techniques, and we do not claim otherwise. We agree with the Reviewer that this point should be stated more explicitly in the revised version of the manuscript. However, as also noted by the Reviewer, to our knowledge these approaches have not previously been applied using fully gap-filled SCA data derived from high-resolution satellite imagery (at least, for LISFLOOD). We believe that the use of such a dataset represents a significant added value.

The paper does not employ a multistep approach (first snow, second discharge) to improve overall calibration. As mentioned, Lisflood is the driving hydrological model of EFAS and GloFAS, which focus on discharge forecasting. An improvement in SCF is fine, but it cannot be the final goal. An improvement in SCF can even lead to a worse objective criterion of discharge, but might still be an improvement because it reduces the error of overfitting.

We agree that a multistep calibration approach would be a final goal for the full calibration of the model. This study focuses on the first step and the downstream consequences of calibrating only the SMC without modifying the other parameters. Existing models typically generate SCA maps that are broadly correct but often lack details. We introduce a method to improve the fine-scale representation of these SCA maps. A key aspect of our work is that these improvements are achieved without affecting the mean SCA, which in turn preserves the discharge accuracy.

This is the reason why the analysed catchments do not show a significant decrease in KGE when the model was run with the new SMC, so in case of a new calibration we expect the equivalent KGE or an improved one. We will expand on this in the last comment.

However, given the importance of the 2 system at global and European level, a 2-step approach would have to go through an increased number on tests, and a full 2-step calibration approach, since the results in KGE could be different in other areas, as suggested by the reviewer.

Here, the focus is solely on the snow ablation process, using the snow melt coefficient (SMC) as the parameter. The process of snow accumulation, with parameters such as snow factor or temperature threshold that determine whether precipitation falls as rain or snow, is overlooked.

We thank the Reviewer for this insightful comment. We agree that our current focus on the snow ablation process, represented by the snow melt coefficient (SMC), overlooks important aspects of the snow accumulation phase. As acknowledged in lines 244–246, we did not explicitly address parameters such as the temperature threshold that determines whether precipitation falls as rain or snow (e.g., *Tm*, usually set around 1 °C), or the snow factor.

We fully recognize the importance of accurately representing and tuning the snow accumulation process. In fact, errors in this phase may significantly impact the overall snowpack evolution. While such tuning can indeed be achieved by adjusting model parameters, we believe a major source of uncertainty may lie in the precipitation input data itself and specifically, in its amount, leading to incorrect accumulation regardless of model settings.

Additionally, we believe that relying solely on snow cover fraction (SCF) data may not be sufficient for constraining the accumulation process, particularly since precipitation events can occur over already snow-covered areas, where SCF remains unchanged.

Therefore, while SCF is valuable, it may not provide enough information to fully and accurately calibrate accumulation-related parameters.

However, we believe that considering an average SMC over different hydrological years is also smoothing the effects of possible errors in precipitations. In any case, we acknowledge this as a limitation of the current study and plan to explore the accumulation process more thoroughly in future work.

The Lisflood snow modul is not explained fully. It is mentioned that Lisflood uses three different elevation zones (line 134), but it is not explained how the SCF calculation from SWE, the calculation of SMC (e.g., equation 12), or the optimization is performed with these three zones.

Thanks for the comment. We improved the LISFLOOD documentation (https://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md) that will be published when LISFLOOD version 5.0 will be released. However, we did not consider the three elevation zones in the SCF parametrization, Eq. 12 or for the optimization approach. The three elevation zones play a role only in the SWE calculation, and in more detail in the temperature and consequently in the accumulation and ablation processes. Afterwards, a single SWE value per pixel is considered and the rest of the approach refer to the “average” SWE value for each pixel.

Especially with higher resolution (here 1 arcmin) the day-degree approach can accumulate too much snow at high altitudes, as temperatures will not too often drop below 1° C. Lisflood uses a workaround to melt additional snow in Summer (IceMeltS). The paper does not mention this approach, nor does it take it into account.

Thank you for the comment. To partially address the issue that the degree-day approach can lead to excessive snow accumulation at higher elevations—especially when applied at higher spatial resolutions (such as 1 arcmin)—the use of three elevation zones was introduced. This zonal approach helps to mitigate overestimation of snow accumulation by better accounting for the altitudinal variation in temperature and snow dynamics. Regarding your comment on the Ice Melt integration, this contribution was neglected in this work. However, we can include it for completeness in the new results.

The effect on the water balance is calculated on a monthly basis, even when the model is run on 6-hour timesteps. Here, it is really necessary to go on daily basis. With monthly evaluations, you miss the main advantage of your SCF calibration: having a better estimate of the timing of the main snow ablation, and therefore a more accurate estimate of the timing and magnitude of spring floods.

We thank the reviewer for the comment. We will remove the monthly analysis and show the daily analysis (as shown in the answer to the comments to Reviewer 1 and the answer and figures below). We agree that that daily analysis is showing better differences in the timing of the peak. We will also include the calculation of a seasonal KGE, showing the KGE and the relative metrics per season. Our analysis has highlighted negligible differences in terms of the overall metrics, however calculating metrics on a seasonally base might highlight a worse/better behaviour in the timing and magnitude of spring/summer floods.

What we regard as an important practical implication for modelling, though, is exactly the possibility of calibrating the SMC directly on snow cover and run the model with the other parameters unchanged, without deteriorating the performance on the streamflow. It is therefore possible to create a setup of the model arguably more realistic in terms of snow cover, without the need to recalibrate the other parameters. Not only is calibration of large models resource-intensive, but there is also a risk of model overfitting that is constrained according to our procedure. See also reply to the last specific comment by the reviewer below.

Plots with daily averages are attached to the document (Figure 4).

Discharge wise, Salzach river shows a worse performance, with lower daily discharge between February and April, while in June and July the discharge is overestimated, also performing worse than the benchmark.

The Arve River shows a better performance in July and beginning of August, with a good match against observed data, worse performance in February and March.

The Alpenrhein has an improved performance in June-July, but worse in March-April.

In the other catchments differences are negligible.

To investigate whether this is associated with poorer performance in terms of SCF, we further analyzed the trends of bias and RMSE across the different months. The results are presented in the figures below. Notably, worse SCF performance is observed particularly in April for the Salzach, and in March and April for the Arve. The Alpenrhein shows a higher bias in April, along with higher RMSE values from April through June.

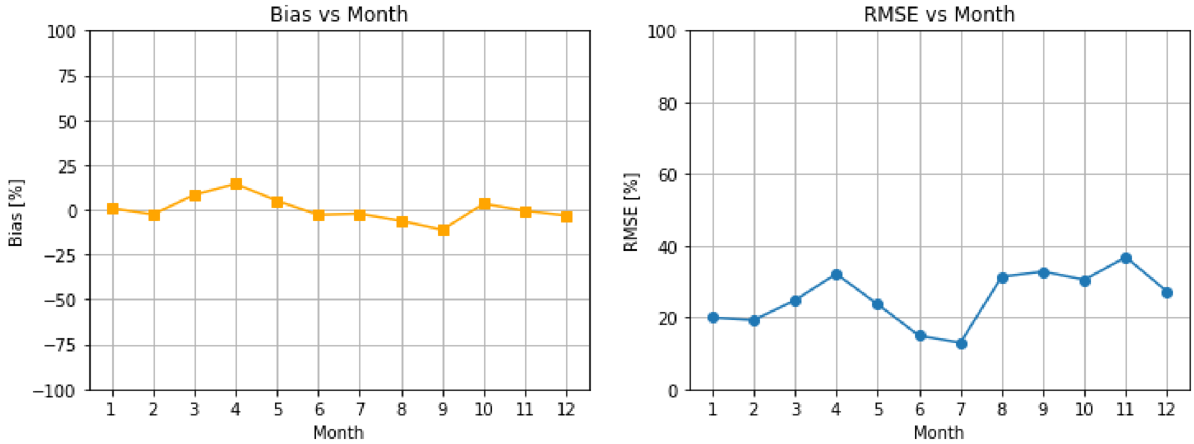


Figure 1 Monthly error trends in terms of SCF for the Salzach basin.

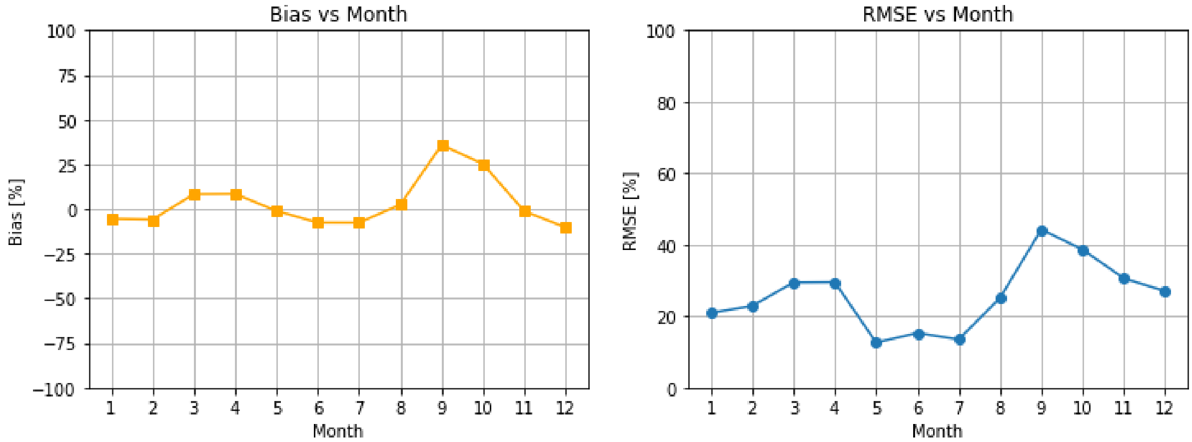


Figure 2 Monthly error trends in terms of SCF for the Arve basin.

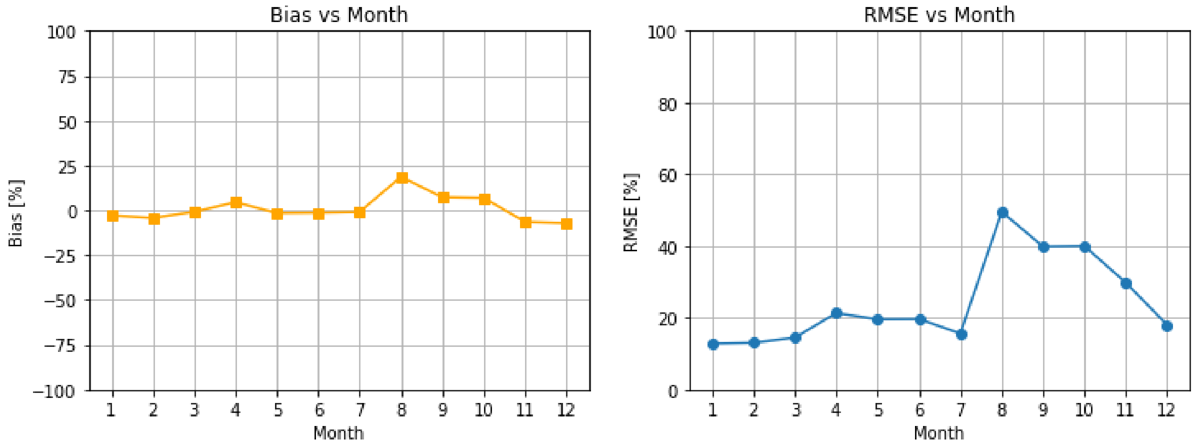


Figure 3 Monthly error trends in terms of SCF for the Alpenrhein basin.

The 2nd step of calibration for discharge is missing, as is an explanation of how to derive a better KGE with a change in SMC. In L_Cm version of Lisflood, SMC is calibrated to improve the KGE (SMC is optimized for discharge KGE). In the EO-Cm version, you changed only the SMC, and you keep all the other calibration parameters? The improvement in KGE (even the tiny one) can only be explained by a bigger range of SMC and/or by the single cell values. However, the calibration was performed on daily discharge; therefore, a comparison with daily values would be appropriate.

A 2nd discharge calibration is necessary to see the improvement vs the original calibration, using the new SMC as predefined values. I am not asking for all 9 basins but for those where you have only one subbasin (Arve, Laborec, Morrumsan, Umealven)

In the EO-Cm version, we changed only the SMC keeping all the other calibration parameters the same. It is true that the bigger range of SMC can be an explanation for the improved KGE, we will include that in the discussion and highlight the differences between the 2 snowmelt coefficients. We addressed the calibration comment in the specific comments below.

Specific comments:

- L2: I would not call it traditionally. It is not made because of tradition, but it has a reason. If you call it later traditional calibration, it is ok

  Thanks for pointing this out. We removed it.

- L21: This is unclear. It cannot be globally between 40-90% snow contribution from mountains. Please check Viviroli again

  Thank you for the comment. Our intention was to refer to *regional* contributions rather than *global* ones. This is clarified in the Introduction of Viviroli et al. (2007), which states: "regionally, mountain discharge may represent up to 95 percent of total flow in a catchment [Liniger et al., 1998]." This is further supported by Viviroli et al. (2004): "In humid areas, mountains supply up to 20–50% of total discharge, while in arid areas, mountains contribute from 50–90%, with extremes of over 95%."

  To avoid confusion, we will revise the sentence in the manuscript to read: "Depending on geography and climate, the regional contribution of snow to river runoff can vary substantially, from as low as 40% up to 95% of the total annual flow (Viviroli et al, 2007)."

- L25: "LISFLOOD is one of the most comprehensive operational models used in Europe to simulate hydrological processes". This is very general sentence. Maybe a unique selling point: Lisflood is one of few operational models calibrated for Europe to simulate hydrological processes.

Thanks. We changed with “LISFLOOD is one of the few operational models used in Europe to simulate hydrological processes.”

- L34: I think the equation which takes rain into account is from: Speers, D.D., Versteeg, J.D. (1979) Runoff forecasting for reservoir operations - the past and the future. In: Proceedings 52nd Western Snow Conference, 149-156

  We revised the sentence: “The snow module within LISFLOOD simulates snowmelt using a temperature-based approach—specifically, a degree-day model that also accounts for enhanced snowmelt during rainfall events (Speers and Versteeg,1982).”

- L65: for a “novel” method you explain not much in L183-184

  Thanks for the comment. As explained later, the optimization technique itself is not novel and it is part of the SciPy library. What we meant, is that this optimization approach has not been previously applied to LISFLOOD. For the sake of transparency, we will use the word “alternative” instead of “novel”.

- L103: “The current model setup operates ...”. Maybe put this after line 106, because the first part explains Lisflood, the second a special application of Lisflood for the EFAS setting

  We inverted the two sentences.

- L131 it is rainfall per day not rainfall intensity. Somewhere else it should be hydrological year instead hydrological season

  Thanks. We replaced with the correct terminology.

- L136: The 3 zones can be explained in more detail and has to be included in 2.3 and 2.4.

  We thank the Reviewer for the comment. Also as pointed out by Reviewer 1, 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://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md) to the manuscript, that has been recently revised to improve clarity.

- Also the IceMelt part in https://github.com/ec-jrc/lisflood-code/ is not explained at all and not taken into account.

Thanks for the comment. We revised the documentation regarding the ice melt part at https://github.com/ec-jrc/lisflood-model/blob/jcr_revision/2_04_stdLISFLOOD_snowmelt/index.md, that will be published as soon as LISFLOOD version 5.0 is released.

At high altitudes, where the temperature never exceeds 1°C, the model accumulates snow as the temperature threshold for melting (Tmelt) is never exceeded. In these altitudes runoff from glacier melt is an important part. Snow will accumulate and convert into firn; then, firn is converted into ice and transported to the lower regions. This process can take decades or even hundreds of years. In the ablation area the ice is melted.

In LISFLOOD, this process is emulated by melting the ice in higher altitudes on an annual basis over summer.

$$IM_z = T_z \cdot C_{im} \cdot \Delta t$$

where:

- IMz is the icemelt (mm) per time step and elevation zone.
- Cim is the seasonally-varying icemelt coefficent (mm/°C day).

The seasonal icemelt coefficient enforces that icemelt only happens during summer (from June 13 to September 13 in the Northern Hemisphere, from December 13 to March 14 in the Southern Hemisphere). It also takes the shape of a sine function with a maximum value of 7mm/°C day:

$$C_{im} = \begin{cases} 7 \cdot \sin\left((\text{doy} - \text{start}) \cdot \frac{\pi}{365.25} \right) & \text{if start} < \text{doy} < \text{end} \\ 0 & \text{else} \end{cases}$$

where start and end are the days of the year representing the beginning and end of the icemelting season, i.e., approximately summer.

However, in our exercise we did not include this component since 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%. 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. The simplified approach as implemented in LISFLOOD can be integrated in the next version.

- L146ff: This part can be done in a nicer way. E.g. https://egusphere.copernicus.org/preprints/2025/egusphere-2025-1214/egusphere-2025-1214.pdf has a better way to structure this.

Thank you for the comment. We have reviewed the suggested paper, but we are not entirely certain about the specific concern raised—whether it relates primarily to stylistic presentation (e.g., adding a table with symbol definitions) or to conceptual clarity. Nevertheless, we will revise the section to improve clarity, and we will consider including a table of symbols if it enhances readability. These revisions will also be aligned with the changes made in response to Reviewer 1's comments.

- L152: How is kaccum calculated?

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 k_accum at the pixel level for each of the five "calibration" seasons and then averaged these values over time to obtain a representative constant. We will explain this better in the revised manuscript. An answer and maps representing the used k_accum coefficients are provided also to Reviewer 2.

- L155: What is the reference of this equation?

  All this part refers to the methodology proposed by Swenson and Lawrance (2012). More in detail, for the equations please refer to the code of the CLM model, available here: CTSM/src/biogeophys/SnowCoverFractionSwensonLawrence2012Mod.F90 at master · ESCOMP/CTSM

  With respect to the original paper, there are some differences. In detail, 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. Anyway, as also reported to Reviewer 1, we plan to make this part clearer in the next version.

- L161: How is this calculated with the 3 elevation zones

  Thanks for the comment. We have explained in a previous answer how the elevation zones play a role in the computation of a different temperature. 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. However, this does not affect the SCF parametrization, as explained in a previous answer.

- L169f: The equation 11 is invalid for glaciers and cannot applied for areas with always snow and with several snow-cycles. Equa 12 is again without the snow elevation zones.You showed it anyway, that this equation is not leading somewhere. It is fine to keep this approach.

  Thanks for the comment. We agree that this equation is a simplification, and it is not valid in the particular cases mentioned by the Reviewer (as mentioned in L173). Anyway, both this Equation as well as the optimization approach, are not applied per each elevation zone. As stated in L136, the processes that are modeled separately for 3 separate elevation zones to take into account sub-pixel heterogeneity linked to elevation differences given the large pixel size are only the snow melt and accumulation.

- L183f: You featured this as "novel" approach. It appears to be an optimization for a single-parameter part of the standard SciPy library. Does seem to be novel and not explained at all.

  Thanks for the comment. You are absolutely right, the optimization technique itself is not novel at all and it is part of the SciPy library. What we meant, is that this optimization approach has not been previously applied to LISFLOOD and specifically to compute Cm, while previous work as the one of Pistocchi et al., 2017 has focused on simpler methodologies. On the other hand, other works as Thirel et al., 2012 or Thirel et al., 2013 have focused on a comparison or data assimilation. We will revise the sentence to make this clearer.

- Table1: the max elevation is not explained. In the original Merrit DEM it is much higher, so I assume you average that for 1 arcmin. But in the model the max elevation is the highest elevation zone. I think you should correct the elevation by the values from original Merrit DEM
  Thanks for the comment. Yes, you are absolutely right that this can be misleading and depending on the resolution of the used DEM, the values can be different. The DEM (also showed in Figure 1 of the manuscript) is the one aggregated at the LISFLOOD resolution of 1 arcmin. Therefore, we will retain the figure and the color bar ranges as they currently are, as they correspond to the 1 arcmin DEM. Hence, we will clarify this in the figure caption to avoid confusion by adding "The elevations reported here (and hence the color bar ranges) refer to the MERIT DEM aggregated at the model resolution of 1 arcmin."

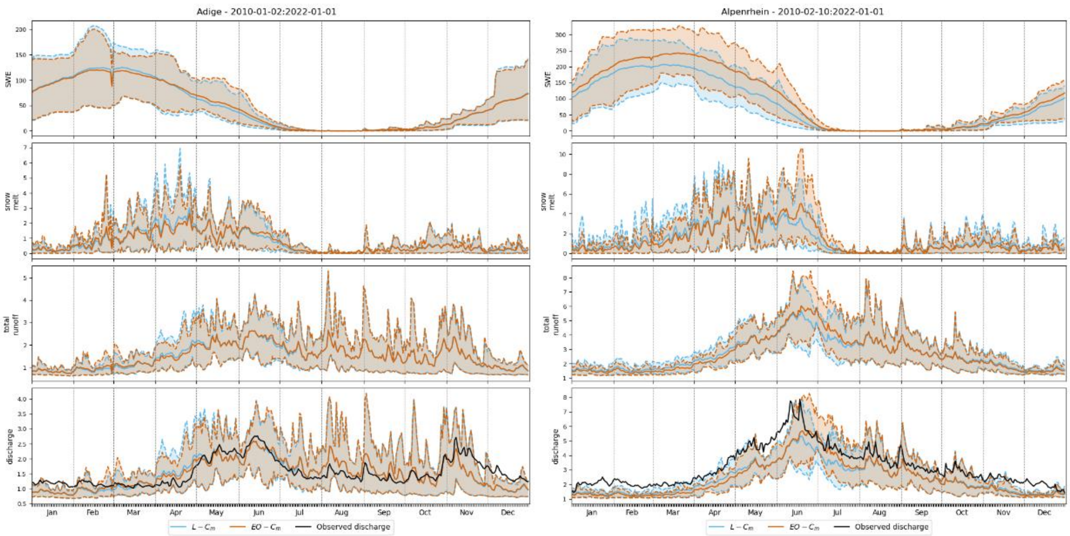
  Regarding Table 1, we consider it more appropriate to report the elevation values that correspond to the minimum and maximum heights used in the model, based on the division into three elevation zones. Therefore, we define the minimum elevation as **$\min(DEM_i - \sigma_i)$** and the maximum as **$\max(DEM_i + \sigma_i)$**, reflecting the lower and upper bounds of the elevation ranges we model. We will update both the table and its caption accordingly to clarify this approach.

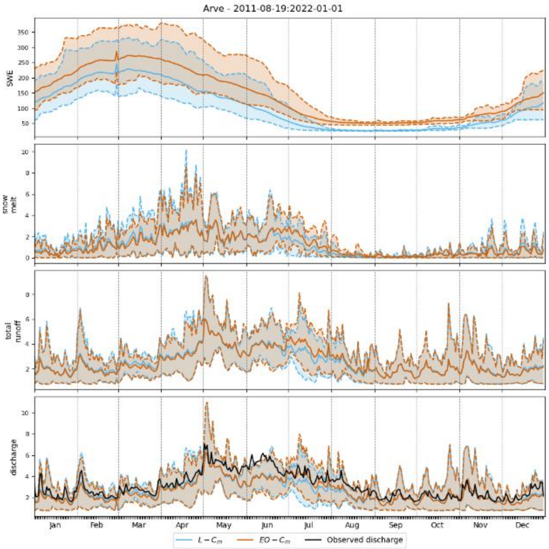
Table 1. Overview of the nine hydrological catchments selected in this study, including their respective countries, area, and elevation information. The mean elevations reported here refer to the elevation from the MERIT DEM aggregated at the model resolution of 1 arcmin. Maximum and minimum elevations are calculated considering σ_{topo}, thus representing the minimum and maximum elevations modelled.

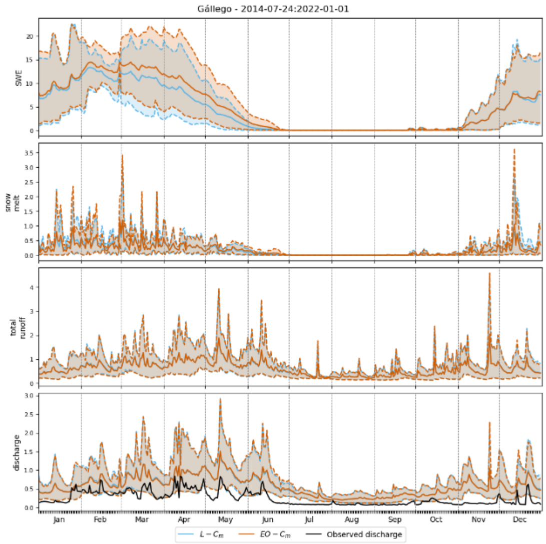
Basin	Countries	Area [km^2]	Elevation [m]		
			min	mean	max
Adige	Italy	12100	3	1497	3724
Alpenrhein	Switzerland/Austria	7400	389	1660	3276
Arve	France	2000	344	1372	4413
Gállego	Spain	3900	199	798	2922
Guadalfeo	Spain	1200	70	1293	3294
Laborec	Slovakia	1300	136	421	980
Mörrumsån	Sweden	3500	9	186	310
Salzach	Germany/Austria	6700	348	1260	3295
Umeälven	Sweden	6100	318	711	1557

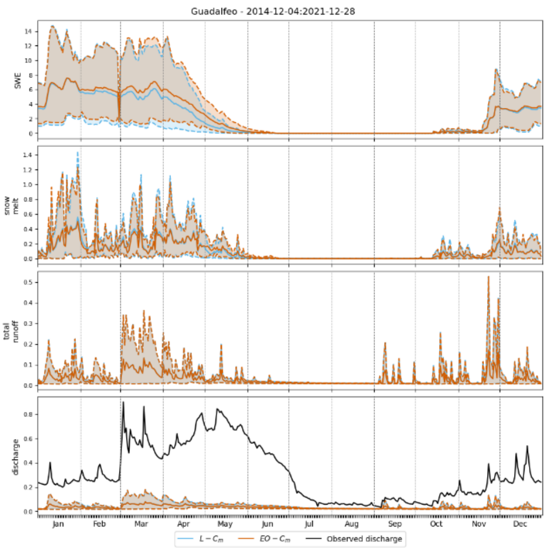
- L297ff: Why using a monthly comparison. The original model is calibrated on daily values. The biggest effect of your improvents are the timing in days of the biggest drop in snow accumulation. The comparison to daily discharge is necessary to conclude if the models performance is improved (maybe the over discharge KGE is reduced, but some spring floods are better timed, the snow cover is better estimated).

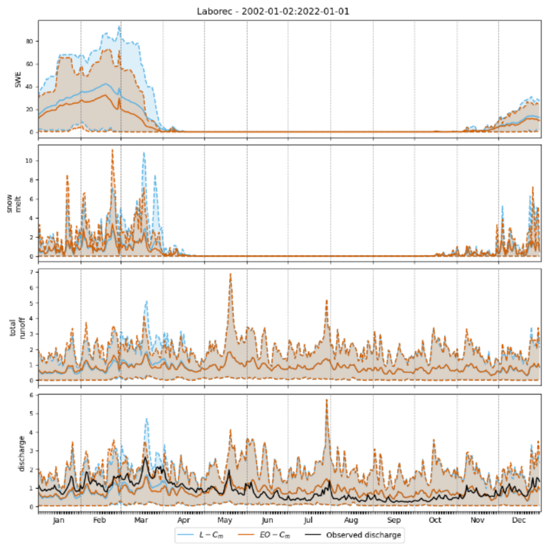
  We thank the reviewer for the comment we will include a comparison on average daily discharge over years, as shown in the following figures.

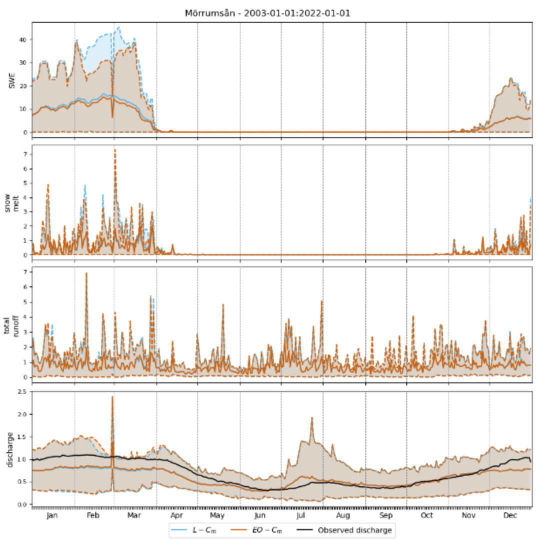


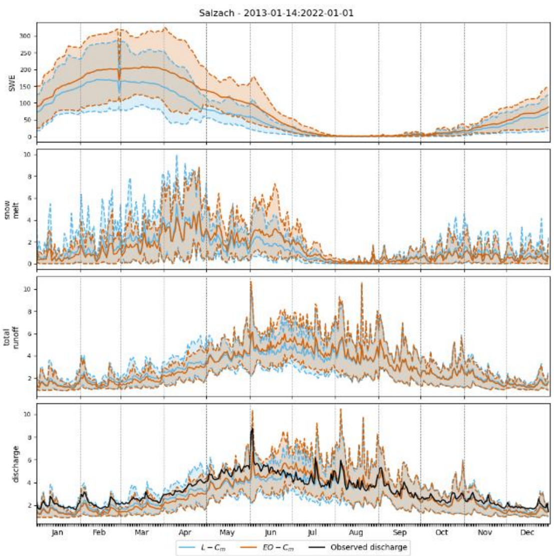

Arve - 2011-08-19:2022-01-01
SWE
snow melt
total runoff
discharge
Jan Feb Mar Apr May Jun Jul Aug Sep Oct Nov Dec
$L-C_m$
$EO-C_m$
Observed discharge


Gállego - 2014-07-24:2022-01-01
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


Guadalfeo - 2014-12-04:2021-12-28
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


Laborec - 2002-01-02:2022-01-01
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


Mörrumsån - 2003-01-01:2022-01-01
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


Salzach - 2013-01-14:2022-01-01
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

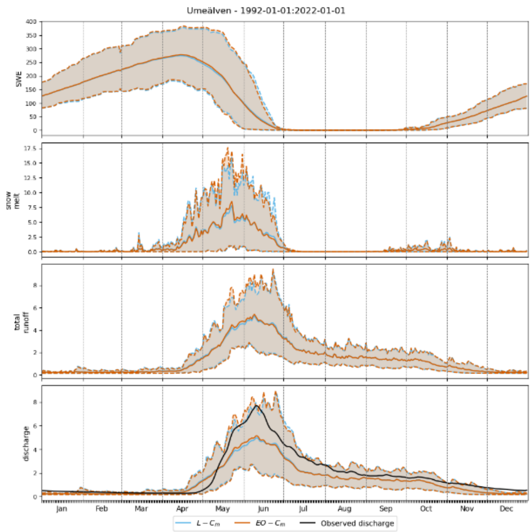


Figure 4 Comparison of daily average discharge over years for the different catchments. In cyan, the discharge computed with the old coefficient, in orange the discharge with the new coefficient and in black the observed discharge.

We made some preliminary analysis on peak timing of discharge; however, it was difficult to have drawn conclusions looking at single events. In some cases, peak was better captured in some other events no, in other events no difference was noticeable etc., this can be explained with the fact that are other factors contributing to the discharge at the location of the observed river discharge. The results are shown in the following figures.

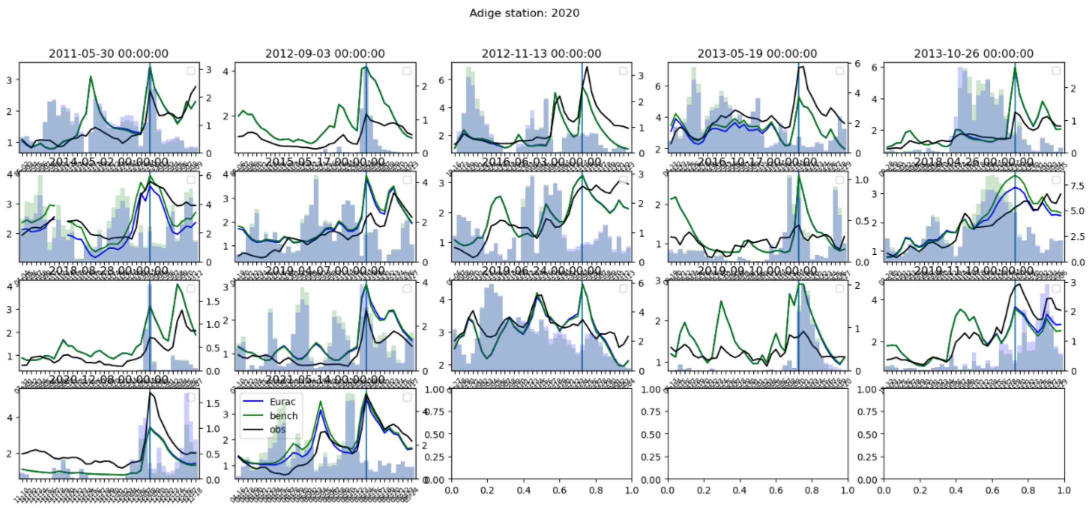


Figure 5 Peak events for the Adige basin.

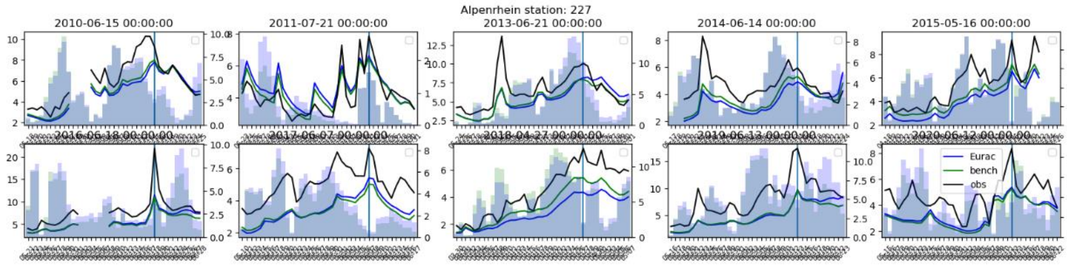


Figure 6 Peak events for the Alpenrhein basin.

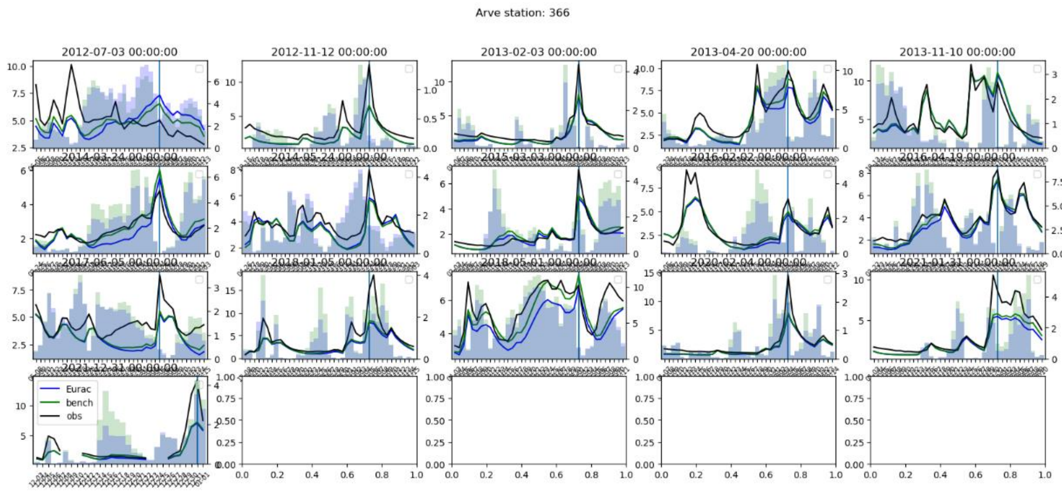


Figure 7 Peak events for the Arve basin.

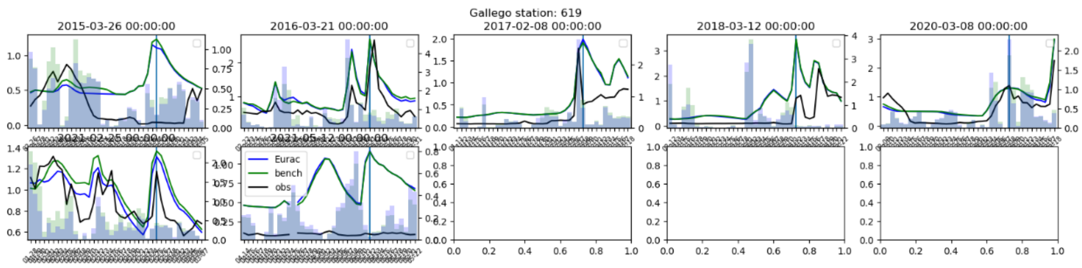


Figure 8 Peak events for the Gallego basin.

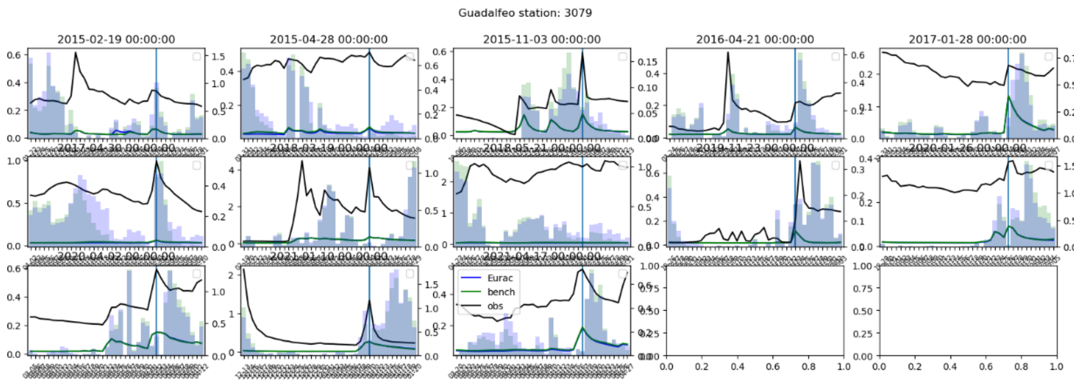


Figure 9 Peak events for the Guadalfeo basin.

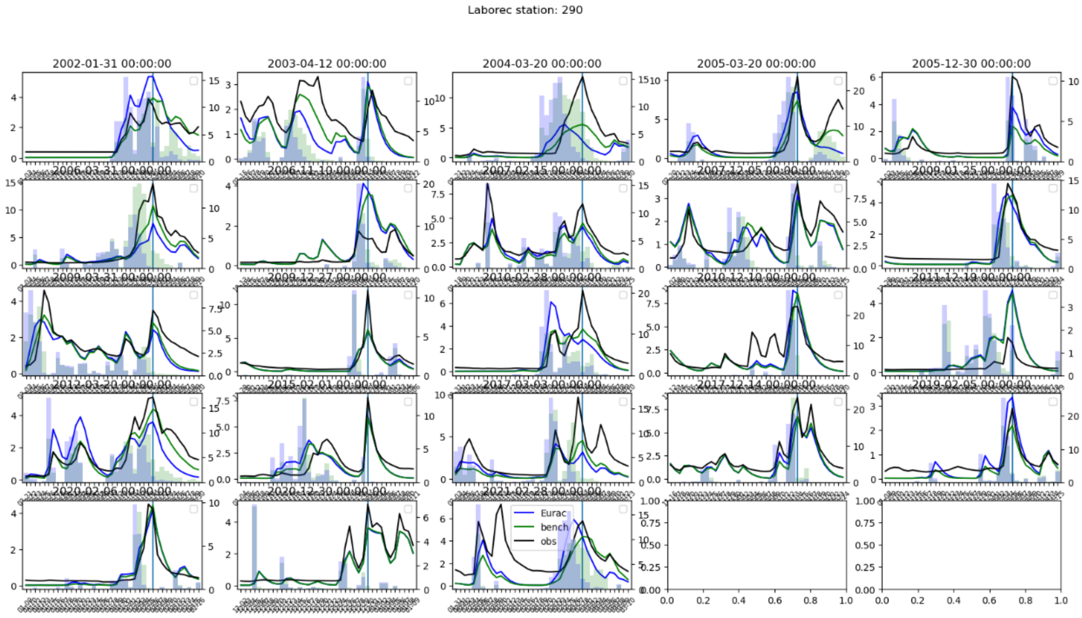


Figure 10 Peak events for the Laborec basin.

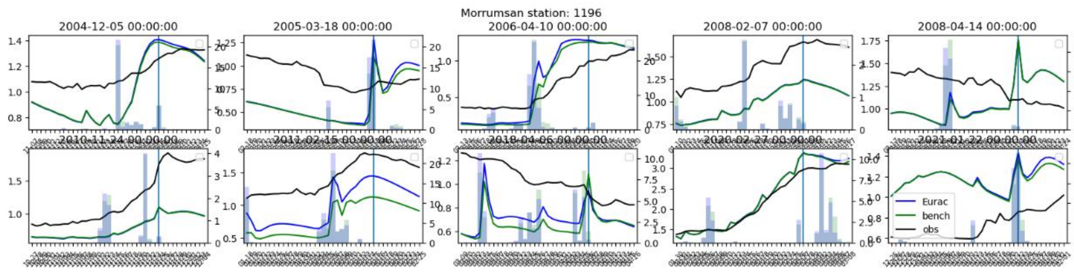


Figure 11 Peak events for the Morrumsan basin.

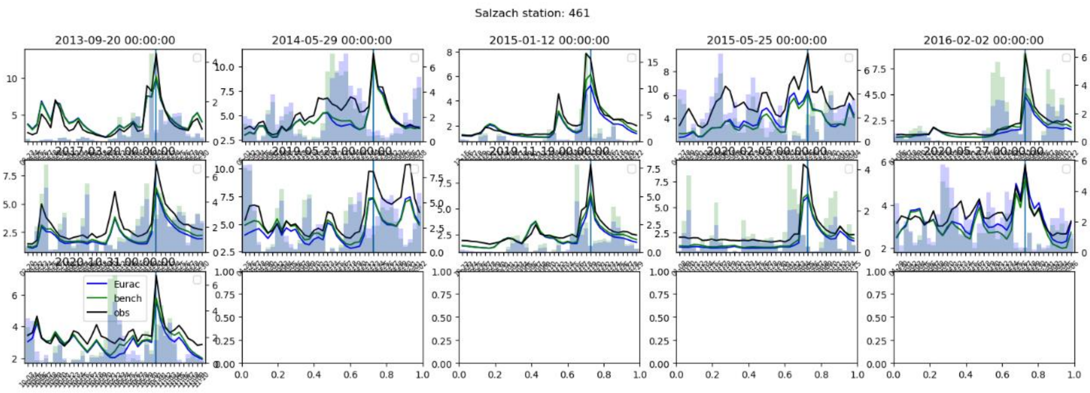


Figure 12 Peak events for the Salzach basin.

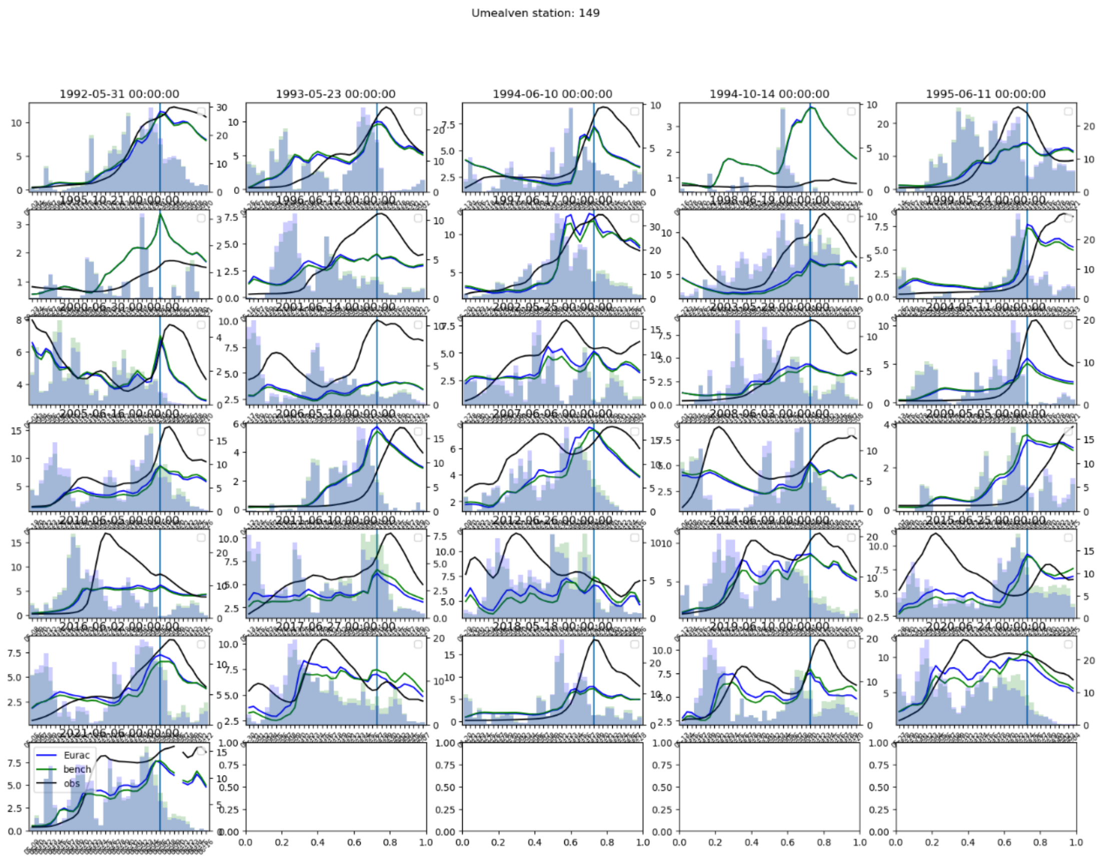


Figure 13 Peak events for the Umealven basin.

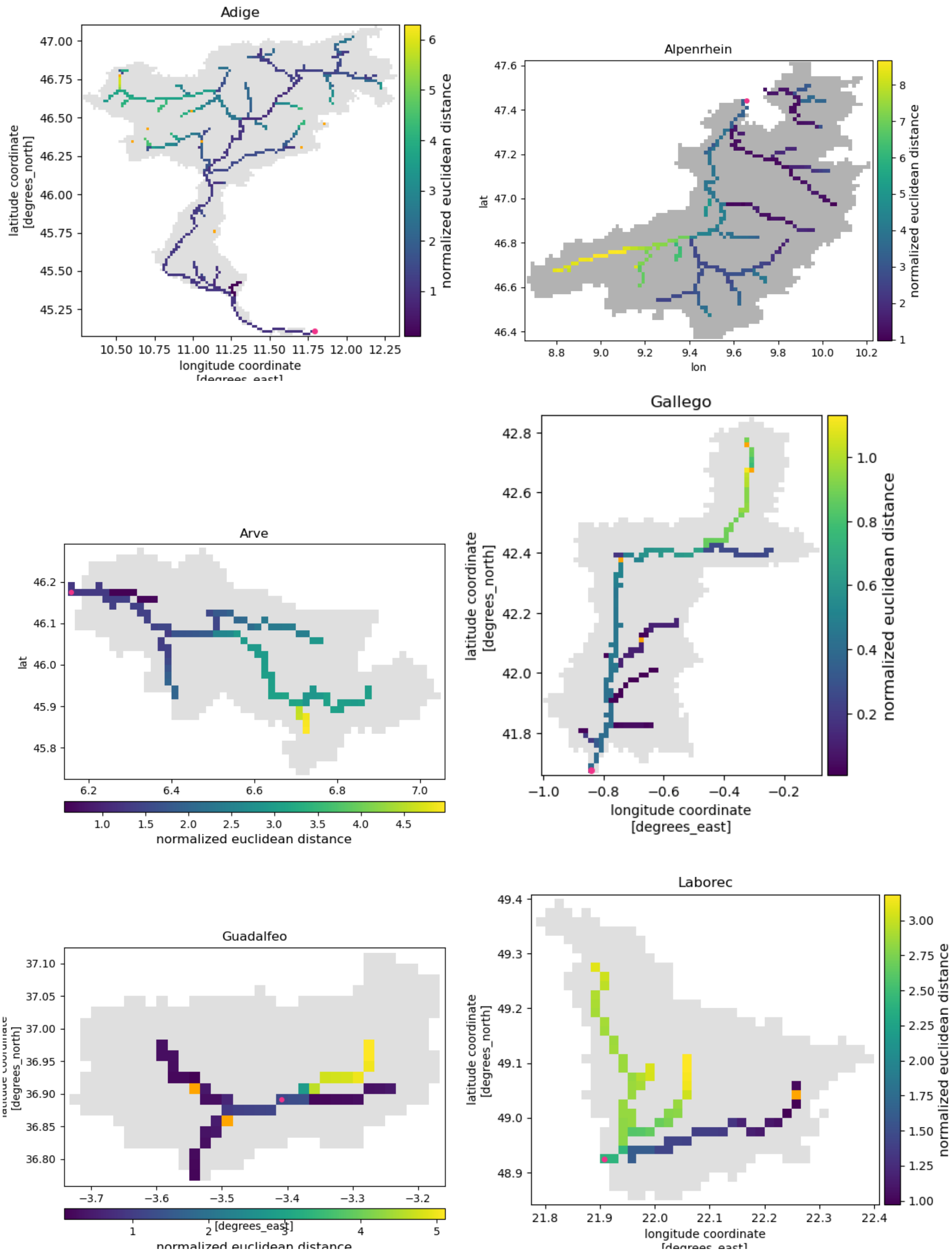
Another possible analysis that can be included in the revised version of the manuscript is to calculate KGE and its metrics grouped by season/month and see if we see improvements or worsening in performance.

- Fig 7: Gallego and Guadelfeo have some reservoirs included. It would be better to use subbasins without too much human interference. From the results, you cannot see if it is the snow or the reservoirs. You explained why Guadalfeo has a bad KGE. One solution could be to use only those years without reservoirs,

The observed discharge for Guadalfeo river starts from 2014, so the bad performance is due the regionalized parameterization, we will specify that in the revised version of the manuscript.

We agree with the reviewer that ideally the less human influenced the better, however LISFLOOD is calibrated on human influenced streamflow we wanted to include catchments with different characteristics.

In the case of the Gallego river, the reservoir (highlighted in orange in Figure) might influence partially a change in discharge as shown in the Figure 14 below.


Adige
latitude coordinate [degrees_north]
longitude coordinate
normalized euclidean distance
47.00
46.75
46.50
46.25
46.00
45.75
45.50
45.25
10.50 10.75 11.00 11.25 11.50 11.75 12.00 12.25
1 2 3 4 5 6
Alpenrhein
lat
lon
normalized euclidean distance
47.6 47.4 47.2 47.0 46.8 46.6 46.4
8.8 9.0 9.2 9.4 9.6 9.8 10.0 10.2
1 2 3 4 5 6 7 8
Arve
lat
46.2 46.1 46.0 45.9 45.8
6.2 6.4 6.6 6.8 7.0
1.0 1.5 2.0 2.5 3.0 3.5 4.0 4.5
normalized euclidean distance
Gallego
latitude coordinate [degrees_north]
longitude coordinate [degrees_east]
normalized euclidean distance
42.8 42.6 42.4 42.2 42.0 41.8
−1.0 −0.8 −0.6 −0.4 −0.2
0.2 0.4 0.6 0.8 1.0
Guadalfeo
[degrees_north]
[degrees_east]
37.10 37.05 37.00 36.95 36.90 36.85 36.80
−3.7 −3.6 −3.5 −3.4 −3.3 −3.2
1 2 3 4 5
Laborec
latitude coordinate [degrees_north]
longitude coordinate
normalized euclidean distance
49.4 49.3 49.2 49.1 49.0 48.9
21.8 21.9 22.0 22.1 22.2 22.3 22.4
1.00 1.25 1.50 1.75 2.00 2.25 2.50 2.75 3.00

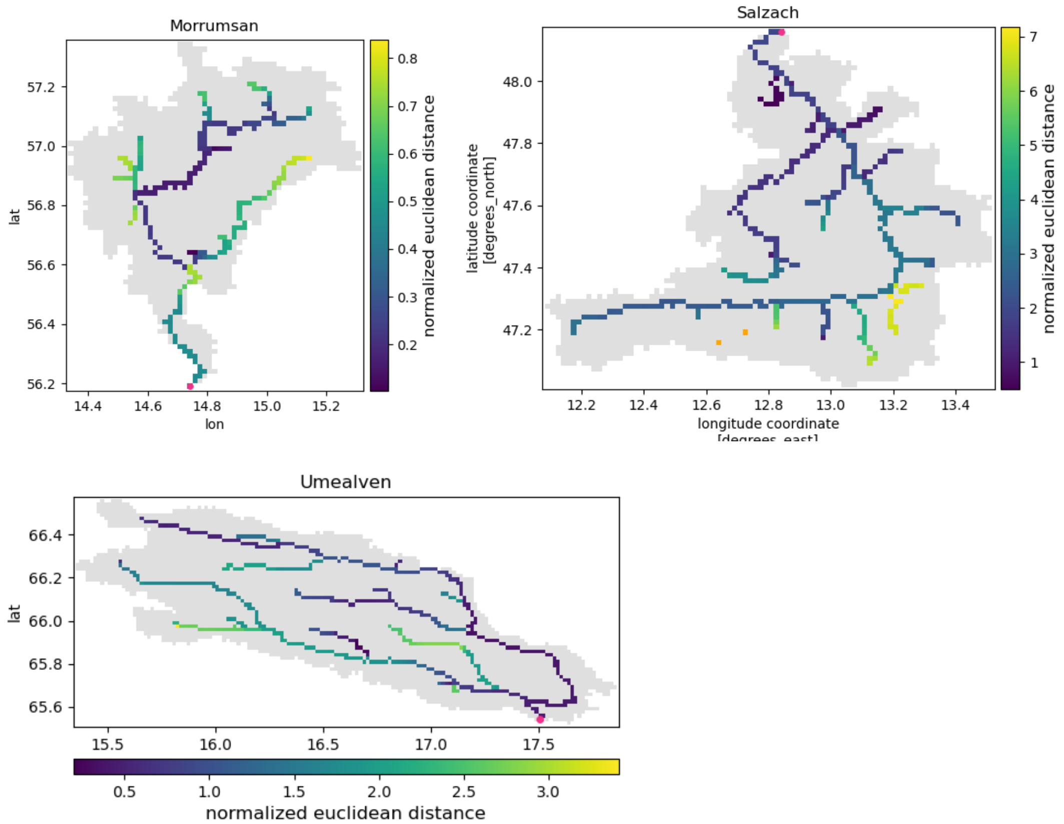


Figure 14 Normalized Euclidean Distance between daily discharge of LISFLOOD model as per in EFAS 5.0 and the LISFLOOD model run with the new snow melt coefficient. Light colours mean that the discharge are different, dark colors mean that discharges are similar. Reservoirs are highlighted in orange in the figure.

- Table 4: This is not suitable for comparison. 1) you keep the other parameter constant (I assume, it is missing in the paper) 2) you compare on monthly values 3) you did not recalibrated the other parameters after setting SMC to your values.

  Thanks for the insightful comment.

  1) Correct. We will highlight that better in the manuscript.
  2) No metrics are calculated on daily values of river discharge
  3) Correct.

- I think it is necessary to re-calibrate for a number of basins (maybe only those where you do not have upstream-downstream calibration) and discuss the effect of your improved SMC e.g. worse KGE but better representation of snow, more exact timing of snow-induced flooding,

  We thank the reviewer for the useful comment. We compared the simulation of streamflow with the new and old SMC with all other parameters unchanged, and the differences are most of the times very small or even negligible, the biggest difference in KGE takes place in the Laborec catchment (difference = 0.05)

Given the fact that the calibration maximises the objective function (KGE), by recalibrating the model we do not expect a lower KGE compared to the one obtained by running LISFLOOD with the new SMC and the current LISFLOOD parameters.

So, this reinforces three arguments that we will stress better in the revised manuscript:

- A possible 2 step calibration (1. Snowmelt coefficient on EO 2. remaining parameters), has the potential to improve the KGE. In the catchments we analysed, the KGE was slightly degraded in some cases, but within a narrow margin.
- This procedure allows the integration of more realism on the snow component of the LISFLOOD model without recalibrating the other parameters. This may be very useful when in need to use this large-scale model for specific purposes in snow-dominated catchments, while preserving consistency with the overall dynamics at the larger scale.
- The LISFLOOD model as currently calibrated and implemented for EFAS 5, can capture the average snow dynamics at the calibration station. Snowmelt dynamics are likely to change (as shown in the Euclidean distance figures) upstream, so river discharge affected by snowmelt dynamics should be treated with cautions upstream the calibrated stations.

Overall, the topic is interesting, and the potential for a good paper is there, but it lacks structure, and fundamental key points are not included yet.

We believe the responses to comments above have cleared the way to improving the manuscript as required by the reviewer.