

Author response to the reviews of
*“Integrating a global glacier model into local hydrological
modeling: Impacts on melt contributions”*
(egusphere-2026-439)

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We thank both reviewers for their constructive and careful reviews, which have substantially improved the manuscript and the editor for handling our paper. Below we reproduce the reviewer comments (grey italics) and our responses verbatim from the open discussion (AC1 and AC2). After each response, a note **“Changes in the manuscript”** describes how the announced changes were implemented in the revised version. A consolidated list of all relevant changes, including changes not directly requested by the reviewers, is provided at the end of this document. Citations in the responses refer to the reference lists of the posted author comments and of the revised manuscript.

Note on figure and section numbering: the revised manuscript contains a new schematic of the snow redistribution scheme (now Fig. 3) and a new dedicated section on precipitation correction (now Sect. 3.4). Figure numbers from the original Fig. 3 onward and section numbers within Sect. 3 have therefore shifted relative to the submitted version. The verbatim responses below use the numbering of the *submitted* version; the “Changes in the manuscript” notes use the numbering of the *revised* version.

Response to Reviewer 1 (RC1)

General comment

This manuscript presents a solid study that integrates the global glacier model GloGEM with the HBV model implemented in the Raven framework, together with a snow redistribution scheme, across 14 glacierized headwater catchments in Switzerland. The study offers a systematic assessment of how glacier constraints, snow redistribution, and precipitation correction affect melt partitioning and parameter behavior at the catchment scale. Overall, the work is carefully conducted and valuable.

Thanks for the overall positive appraisal.

RC1.1 — Novelty and main contribution

That said, I have several concerns regarding the experimental design and the novelty of the main findings. My main comments are as follows: The main contribution of the paper seems stronger as a systematic regional assessment than as a genuinely new methodological advance. The study

is solid and useful, but the novelty of the main findings appears somewhat limited. The main messages of the paper are that (i) good streamflow performance does not necessarily imply realistic cryospheric process representation, (ii) uncoupled HBV can compensate forcing biases through excessive glacier melt, and (iii) introducing a precipitation correction factor can aggravate equifinality and uncertainty. These are important points, but they are not new in cold-region hydrology and glacier-hydrology modeling. In this sense, the main value of the manuscript seems to lie in the systematic multi-catchment comparison across 14 headwater basins, rather than in delivering new methodological or process-level insight.

We thank the reviewer for the summary of our main contributions and we agree that we could better emphasize how our work goes beyond the state-of-the-art. We acknowledge that the individual findings, such as the disconnect between streamflow performance and cryospheric process realism, or equifinality introduced by precipitation correction, are known challenges in cold-region hydrology. However, we would like to clarify and emphasize what we believe constitutes the novel contribution of this work:

1. To the best of our knowledge, no study has yet integrated glacier melt simulations from a global glacier model (GloGEM) directly as forcing input into a conceptual hydrological model (Raven) at the local catchment scale. As shown in our introduction, existing studies coupling global glacier model output with hydrological models are limited to large-scale global applications, and this study explicitly addresses that gap. We will make this clearer in the revised version.
2. We agree with the reviewer that the systematic multi-catchment evaluation across 14 diverse headwater basins represents a core value of this study. However, it should not just be seen as a comparison: rather, this contributes to demonstrating the transferability and limitations of this coupled approach across a range of catchment characteristics. We will emphasize this point in the revised version.

Changes in the manuscript: The introduction now states explicitly that we present, to our knowledge, the first integration of glacier runoff from a global glacier model into a conceptual hydrological model at the local scale, through a mass-conserving, model-agnostic coupling approach, and that the multi-catchment design (glacier coverage 3–60 %) serves to evaluate the transferability and limitations of the approach (Sect. 1, final paragraph).

RC1.2 — Role of the precipitation correction factor

The role of the precipitation correction factor (PCF) needs to be explained more clearly. The manuscript lists `PRECIP_CORR` as a precipitation correction factor in Table 2 and treats it as a separate configuration dimension in Table 3 (e.g., HBV-PCF and HBV-GloGEM-PCF). However, its hydrological meaning and implementation remain insufficiently explained. At present, the manuscript provides a label, but does not clearly explain what exactly is corrected, how the factor operates within the HBV/Raven setup, or what physical bias it is intended to represent. It is also unclear why calibration of this parameter is treated as a dedicated scenario rather than simply as one additional calibration parameter.

Thanks for pointing out the lack of details regarding this important aspect of our work. Precipitation in high elevation catchments is notoriously difficult. Two main difficulties arise: i) observed point precipitation can be biased due to gauge undercatch (Goodison et al., 1998), which might lead to up to 50 % bias for solid precipitation (Rasmussen et al., 2012). ii) spatially distributed precipitation in high-elevation catchments remains uncertain, as gauge networks are typically strongly biased towards lower elevations (Frei and Schär, 1998). Our precipitation correction factor (PCF) accounts for both aspects, as in many similar model applications (van Tiel et al., 2020), and this will be made clearer in the revised manuscript.

Practically, we implemented precipitation correction by multiplying all observed precipitation values (i.e. all observed values at all time steps) with a spatially and temporally uniform precipitation multiplier, ranging from 0.8 to 1.8.

Furthermore, we would like to explain why the PCF is introduced as a dedicated scenario. In the uncoupled HBV setup, the model can compensate for precipitation biases by producing excessive glacier melt. In the coupled HBV-GloGEM setup, however, glacier melt is given as an input to the hydrological model, meaning it can no longer compensate for potential precipitation biases in this way. The PCF scenario was therefore introduced to test whether correcting the precipitation input could simultaneously reduce the overestimation of glacier melt in HBV and the streamflow underestimation in HBV-GloGEM. Understanding this compensation effect requires a dedicated configuration rather than simply an additional calibration parameter: each configuration in this study corresponds to a separate calibration run with a distinct parameter set. This will be made clear in the revised version by reformulating parts of the methods section.

Changes in the manuscript: A new dedicated section “Precipitation correction” (Sect. 3.4) explains the sources of precipitation bias, the implementation of the PCF as a uniform multiplier calibrated against streamflow, its relation to GloGEM’s internal correction factor $f_{c,prec}$ (see also RC2.3), and the rationale for dedicated PCF configurations.

RC1.3 — Coupling versus glacier-data constraint

The contrast between the two model setups may not primarily reflect “whether GloGEM is coupled,” but rather whether glacier information is used to constrain the glacier component. In the current design, the coupled setup benefits from glacier-specific calibration against geodetic mass balance, whereas the original HBV setup is calibrated only against streamflow. This makes it difficult to attribute the differences solely to coupling with GloGEM. In practical terms, both the GloGEM-based runoff and the melt generated by the simplified HBV glacier routine ultimately act as inputs to the hydrological simulation. It is therefore plausible that, if the native HBV glacier routine were also constrained using glacier data, it could produce more comparable glacier-melt estimates. The manuscript should discuss this point more explicitly and clarify whether the key distinction is really coupling itself, or the use of glacier observations to constrain the glacier contribution.

Thanks for commenting on this important point that deserves explicit discussion in the manuscript, and we will address it more thoroughly in the revised version in the discussion section.

As rightly summarized above, the two setups differ in how glacier melt is simulated (by GloGEM or by the native HBV glacier routine via Raven), and also in the information used to constrain the models (GloGEM was calibrated against a global geodetic mass balance product, the HBV glacier model only against streamflow).

Calibrating the native HBV glacier routine against geodetic mass balance would face two fundamental obstacles: (i) the semi-distributed model structure lacks glacier-specific resolution: glacier melt is governed by a single set of degree-day parameters applied across all glacier HRUs, whereas geodetic mass balance data (Hugonnet et al., 2021) is resolved per individual glacier. Aggregating per-glacier estimates into a catchment-wide constraint would average out glaciers with different characteristics, providing only a weak calibration target. GloGEM’s advantage lies not in a fundamentally different melt formulation, as both rely on temperature-index methods, but in its ability to calibrate parameters individually for each glacier using its specific area-altitude distribution. (ii) the uncoupled model treats glacier ice as an unlimited reservoir that is never depleted, and lacks a mechanism to convert accumulated snow into glacier ice. The glacier can therefore only lose mass through melt but never gain it through accumulation,

meaning the model cannot close the glacier mass budget. Comparing such an incomplete mass balance against geodetic observations, which integrate both accumulation and ablation as well as geometric change, would not constitute a meaningful calibration target.

These limitations are not unique to our setup. Most hydrological models represent glaciers through simplified routines that neither resolve individual glaciers nor simulate the full glacier mass balance, making it inherently difficult to incorporate glacier-specific observations into their calibration. This is exactly one of the gaps this study aims to address, by exploring if glacier runoff from a global glacier model can be used as input for a hydrological model and by providing a mass-conservative, transferable and model-agnostic method to feed the glacier melt into the hydrological model that has a different spatial set-up.

We agree, however, that it would be valuable to test whether the observed differences between the two setups stem from the coupling itself or from the different calibration constraints used. We will highlight this explicitly in the revised manuscript as a direction for future work.

Changes in the manuscript: The discussion now addresses this attribution question explicitly (Sect. 5, paragraph beginning “Constraining the glacier component with an independent observation...”). The limitations section states that the improved realism of the coupled setup cannot be attributed to the coupling alone and that the two effects remain intertwined in our comparison (Sect. 6, first limitation).

RC1.4 — Visibility of the snow redistribution module

The snow redistribution module should be introduced and visualized more clearly. The manuscript explains why snow redistribution is needed and briefly describes how it is implemented. However, its role is not clearly shown in the workflow figure, which makes the overall methodology harder to follow. I suggest either adding this component explicitly to Figure 2 or clarifying its position and function more clearly in the text.

We agree that the snow redistribution module was not sufficiently visible in the workflow. A more detailed description of the module will be added to an appendix to improve the clarity. In brief, the module is based on a parameterization originally developed to simulate avalanche-driven snow transport (SnowSlide, Bernhardt and Schulz (2010)), but is used here not to represent avalanching explicitly, but rather to avoid unrealistic snow accumulation and to redistribute excess snow to lower elevation HRUs where warmer temperatures allow melt to occur. Snow transport is triggered when a slope-dependent snow-holding depth threshold is exceeded, and HRU connectivity is derived during catchment preprocessing using D8 flow routing, with redistribution weights based on upstream contributing area. No additional calibrated parameters are introduced.

Changes in the manuscript: Rather than an appendix, snow redistribution now has a dedicated methods section (Sect. 3.5) and a new two-panel schematic (Fig. 3) showing (a) the derivation of HRU connectivity from D8 flow routing and (b) the redistribution mechanism along the elevation gradient, including the treatment of Masked Glacier HRUs in the coupled configurations.

RC1.5 — Treatment and evaluation of snow-covered area

The treatment of snow-covered area (SCA) remains unclear, and the lack of SCA-based evaluation weakens confidence in the snow and melt partitioning results. I may have overlooked it, but I could not find a clear explanation of how snow-covered area is determined in the model. In addition, the model does not appear to be evaluated against SCA data. This limits confidence in the

snowmelt simulations. Even if catchment-average SWE is reproduced reasonably well, the spatial distribution of snow and the timing and location of melt may still remain uncertain without an independent constraint on snow extent. I therefore recommend that the authors clarify whether SCA is represented or diagnosed in the current workflow, and discuss the implications of not evaluating SCA for the interpretation of snow storage and melt contributions.

Thanks for pointing out that it is not entirely clear in the paper how snow covered area (SCA) is computed. In catchment-scale hydrological modelling, there are two options to compute catchment-average snow accumulation and melt: i) in models without explicit spatial reference for snow simulation (i.e. in fully lumped models), snow accumulation and melt is computed for a reference elevation (from temperature and precipitation) and then linked to SCA via pre-defined “snow height - snow covered area curves” (Martinec et al., 1983). ii) in spatially-explicit models (semi-lumped, semi-)distributed, in particular models with elevation bands, snow accumulation and melt is computed per spatial unit and SCA is obtained directly at each time step from the sum of all spatial units that are snow covered.

Our model setup uses HRUs: these are regrouped “patches” of the catchment with the same land use type and elevation band. Each HRU has its mean elevation and we compute snow accumulation and melt for this mean elevation. In this model setup, SCA is computed internally at each time step as the sum of snow-covered HRUs, but was not used as an evaluation metric. Instead, snow simulations are evaluated against a gridded SWE product (Marty et al., 2025), using catchment-average SWE as the presented metric (Section 4.3, Figure 5). We acknowledge that this approach has two limitations: i) the reference SWE product is itself model-based, although it assimilates in-situ snow depth observations from 350 stations via an Ensemble Kalman Filter (Magnusson et al., 2014), which we consider a sufficiently robust observational constraint for the purposes of our study; ii) as noted by the reviewer, a model could in principle reproduce catchment-average SWE well while misrepresenting SCA. Purely satellite-based snow products would in principle allow for an additional evaluation, but currently available products such as MODIS provide SCA rather than distributed SWE. Moreover, since the hydrological model is HRU-based and only knows the mean elevation of each HRU but not its exact location in the catchment, the spatial extent of snow cover has no direct meaning for the model structure. Given the known hysteresis between SCA and catchment-average SWE (Riboust et al., 2019), evaluation based on SCA alone also gives an incomplete assessment of snow routines. Accordingly, we believe that catchment-average SWE provides a robust measure of snow storage and its seasonal dynamics, capturing the information most relevant to streamflow generation. In the revised manuscript, we will clarify the computation of SCA in the model description and discuss the use of observed SCA for model evaluation as an outlook for settings where no reference SWE product is available.

Changes in the manuscript: The model description now states how SCA is diagnosed (Sect. 3.2.2: “Snow-covered area can be diagnosed at each time step as the areal sum of snow-covered HRUs, where an HRU counts as fully snow-covered whenever it holds non-negligible snow water equivalent.”). The limitations section discusses the SWE-based evaluation, the SWE–SCA hysteresis, and the use of observed SCA as an outlook (Sect. 6, third limitation and future-work paragraph). In addition, the snow evaluation was extended by an elevation-band comparison against SPASS for the Rhone catchment (new Fig. S19; Sect. 4.3).

RC1.6 — Minor comment: use of SWE data

Please clarify explicitly whether SWE was used only for validation or also during calibration. From the current text, SWE appears to have been used for validation rather than calibration. Since SWE results are shown prominently, this point should be stated unambiguously.

No SWE data is used in the calibration procedure. The gridded SWE product is only used to validate simulated SWE outputs from the snow parameters calibrated on streamflow. This will be made more explicit in the Manuscript.

All other minor issues like naming concerns and typos will be addressed in the revision.

Changes in the manuscript: Stated explicitly in Sect. 3.6: “No snow or SWE data are used in the calibration. The gridded SPASS product is reserved for the independent validation of simulated SWE.” All minor issues were addressed.

Response to Reviewer 2 (RC2)

General comment

The manuscript presents a systematic modelling experiment in which the authors couple the global glacier evolution model GloGEM with the HBV model implemented in the Raven framework, and assess the added value of glacier constraints, snow redistribution, and precipitation correction for simulating streamflow and partitioning snowmelt and glacier-melt contributions across 14 glacierized Swiss headwater catchments. The topic is relevant, and the paper addresses the fact that good streamflow performance does not necessarily imply a realistic representation of snow and glacier melt processes. Overall, I find the study valuable, but I think that some methodological choices need to be clarified before publication.

We thank the reviewer for the overall positive assessment and for recognising the relevance of the study. We will address each of the methodological points raised below in the revised version.

RC2.1 — Role and validation of snow redistribution

The major issues in my opinion are: Role and validation of snow redistribution: snow redistribution appears to have an important influence on the partitioning between snowmelt and glacier melt. In particular, it may affect how long glacierized areas remain snow-covered, and therefore how much ice melt is simulated.

Thanks for this important comment. We agree that snow redistribution exerts an important control on melt partitioning, and we will describe the underlying mechanism explicitly in the methods (Section 3.4) and results section (Section 4.6). It operates as follows in the uncoupled HBV configurations: the HBV glacier module produces no ice melt while the glacier surface is snow-covered (Section 3.2.2), so the duration of snow cover directly modulates simulated ice melt.

When redistribution is enabled, snow exceeding the slope-dependent holding capacity is transferred from steep HRUs to lower ones (from higher to lower elevations, no wind-drift is emulated). Since glacier surfaces tend to be less steep than the surrounding ridges, the glacierized HRUs are in many cases recipients rather than sources of redistributed snow. Two effects follow: the added depth prolongs the snow-covered period of the underlying glacier ice, while warmer temperatures at the lower receiving elevation raise melt rates. Both effects shift the melt partitioning from glacier ice toward snow: the prolonged snow cover reduces ice melt, while the higher melt rates allow the redistributed snow to melt and increase snowmelt contributions, rather than accumulating at higher elevations as unrealistic “snow towers”.

To illustrate the magnitude of this effect, consider 100 mm of snow water equivalent added to a flatter glacier surface at 3000 m a.s.l. With a spring snow melt factor of about $3 \text{ mm } ^\circ\text{C}^{-1} \text{ day}^{-1}$ and a mean temperature of $+3^\circ\text{C}$ at this elevation, the snow melts at roughly 9 mm day^{-1} and

persists for about 11 days. Had it remained 500 m higher at its source, the lapse rate of 0.6°C per 100 m would give a mean temperature of 0°C , at which no melt would be generated. Over the 11 days that the redistributed snow keeps the glacier HRU snow-covered, the underlying ice does not melt. As a result and as the numerical example above shows, the increased snow depth prolongs the snow-covered period of glacier ice and thereby delays ice melt. The net result is a melt re-partitioning from glacier melt toward snowmelt.

Given this importance, the snow redistribution scheme should be described more clearly, including how it operates within the semi-distributed HRU structure. I also think that an independent evaluation would be useful, for example using maps snow-covered area from satellite observations. Catchment-average SWE alone may not be sufficient to verify whether the spatial distribution and timing of snowmelt are realistic.

These two comments mirror comments by reviewer 1 (see, RC1: 'Comment on egusphere-2026-439', doi: 10.5194/egusphere-2026-439-AC1). We will add a more detailed description of the snow redistribution scheme, albeit in the appendix to not cut the flow of the manuscript (and because it is not our own development). We will also make it explicit in the workflow figure (Fig. 2). We will additionally clarify how it operates within the semi-distributed HRU structure (Section 3.4): snow exceeding the slope-dependent holding depth is transferred laterally to lower-elevation HRUs, with connectivity derived during preprocessing via D8 flow routing and transport weighted by HRU area. No additional parameters are calibrated. Full details are given in our response to RC1 doi: 10.5194/egusphere-2026-439-AC1.

Furthermore, Reviewer 1 raised the same concern regarding snow-covered area (SCA), and we address it in detail there (doi: 10.5194/egusphere-2026-439-AC1). In brief: SCA is computed, but not used as an evaluation metric. We instead evaluate against the gridded, observation-assimilated SWE product of Marty et al. (2025). We agree that catchment-average SWE does not fully constrain the spatial pattern of snow, and we will clarify the SCA computation in the methods section and discuss the implications of not evaluating against SCA, as detailed in our response to Reviewer 1.

Changes in the manuscript: Snow redistribution is now described in a dedicated methods section (Sect. 3.5, rather than an appendix) with a new schematic (Fig. 3), including how it operates within the semi-distributed HRU structure (D8-derived connectivity, slope-dependent holding capacity, no additional calibrated parameters, and the removal of snow redistributed onto Masked Glacier HRUs in the coupled configurations). The mechanism by which redistribution shifts melt from glacier ice toward snow is described in the results and discussion (Sect. 4.6 and Sect. 5). For SCA, see the changes under RC1.5; in addition, the snow evaluation was extended by an elevation-band comparison against SPASS (new Fig. S19, discussed in Sect. 4.3).

RC2.2 — Routing of GloGEM meltwater

Routing of GloGEM meltwater: the treatment of glacier runoff in the coupled configuration is not fully clear to me. The manuscript states that GloGEM runoff bypasses intermediate storage components and is directly added to the runoff-streamflow transfer function. However, the native HBV glacier module seems to include a glacier storage coefficient controlling meltwater release. Are glacier melt contributions routed in a comparable way in the HBV and HBV-GloGEM configurations, or does the coupling remove part of the glacier storage/routing representation? Since this may affect the timing of simulated discharge, especially during the melt season, this point should be clarified and justified.

We thank the reviewer for the careful reading; the observation is correct, and we will clarify this in Section 3.3 (Methods Section). In the uncoupled HBV configurations, glacier melt is released

through a linear storage reservoir governed by the calibrated coefficient `GLAC_STORAGE_COEFF`, before entering the catchment transfer function. In the coupled HBV-GloGEM configurations, the GloGEM-derived runoff is added directly to the catchment transfer function and the linear glacier storage reservoir is bypassed (the coupled configurations calibrate one parameter less; Table 3). Both configurations use the same triangular transfer function `TIME_CONC`, so the catchment-scale routing is identical.

This setting is due to the structure of the coupling and the composition of the simulated GloGEM glacier runoff. In the native HBV module, the glacier reservoir is parametrised by a single release coefficient for ice melt alone (`GLAC_STORAGE_COEFF`): snowmelt and rainfall do not pass through it. In our one-way coupling on the other hand, glacier runoff is injected as precipitation onto the Masked Glacier HRUs. The simulated GloGEM ice melt, snowmelt, and rainfall are supplied to Raven as one aggregated flux and cannot be routed selectively. Since this combined flux is mostly composed of snowmelt and rainfall, passing all of it through the glacier reservoir would delay water that never enters that reservoir in the uncoupled configuration. We therefore pass the combined GloGEM runoff directly to the catchment transfer function.

We acknowledge that this removes the conceptual linear storage that, in the uncoupled configurations, delays the release of glacier melt before it enters the catchment transfer function; this additional sub-catchment delay is not reproduced in the coupled configurations. A possible improvement for future work would be to add GloGEM’s components separately and pass it through a glacier storage reservoir. We will add both this limitation and the possible improvement to the revised manuscript.

Changes in the manuscript: Sect. 3.3 now explains the routing difference and its rationale in full (paragraphs beginning “This also changes how glacier meltwater is routed...”). The missing ice-melt storage delay is stated as a limitation, together with the future option of passing GloGEM’s runoff components separately so the ice-melt fraction can be routed through a glacier release storage (Sect. 6, second limitation).

RC2.3 — Precipitation correction factor and relation to GloGEM’s $f_{c,prec}$

Precipitation correction factor: the hydrological meaning and calibration of the precipitation correction factor should be explained more clearly (i.e., is this factor intended to correct precipitation undercatch, orographic effects, winter precipitation bias, etc.). Is this PCF different from the $f_{c,prec}$ (line 205) calibrated for the GloGEM? How do these two parameters are linked? If they are different, does this means that there is a precipitation correction in all the HBV-GloGEM coupled configuration? Would this change the volumes reaching the catchment between configurations?

As also noted by Reviewer 1, we will expand the methods to clarify what the PCF represents. In short, it is not a correction for a specific process (undercatch or orographic effects), but a lumped, spatially, and temporally uniform multiplier (0.8–1.8) applied to the observed precipitation field. Full details and references are given in our response to Reviewer 1 doi: 10.5194/egusphere-2026-439-AC1.

In fact, there are two distinct and independent precipitation correction factors, one for GloGEM and one for HBV, constrained by different observations:

1. $f_{c,prec}$ is a parameter of GloGEM. It is calibrated against geodetic mass balance, independently of and prior to our hydrological calibration. It is therefore fixed before we calibrate the hydrological model.
2. The PCF is internal to the HBV hydrological model and is calibrated against streamflow. In the coupled version it corrects the precipitation forcing over the non-glacierized area only, whereas in the uncoupled version it corrects precipitation over the entire catchment.

As a consequence, a precipitation correction is present in every coupled HBV-GloGEM configuration: the glacier runoff that GloGEM passes to the hydrological model already embeds its mass-balance-constrained correction $f_{c,prec}$. In the coupled version, the hydrological PCF is applied only in the dedicated PCF configurations and only to the precipitation driving the non-glacierized part of the catchment. Coupled configurations without the PCF therefore still carry GloGEM’s internal correction in the glacier runoff, but apply no correction to precipitation on non-glaciated areas. We will state this explicitly in the revised methods.

We emphasize that the two factors are not interchangeable, because each is only valid over the region against which it was constrained. GloGEM’s $f_{c,prec}$ is calibrated against geodetic mass balance and is therefore only meaningful over the high-elevation glacierized area. Its meaning differs in that it accounts for additional processes such as snow accumulation on the glacier originating from surrounding slopes through avalanches. In addition, each glacier has a different correction factor, which is not compatible with a catchment-wise factor. For these reasons, it cannot be applied to the precipitation driving the non-glacierized part of the catchment. Conversely, the PCF is calibrated against streamflow and corrects precipitation over the non-glacierized area in coupled configurations. The two corrections thus act on spatially distinct parts of the catchment.

It follows that the total water volume entering the catchment differs between configurations. In the coupled version, calibrating the PCF rescales the precipitation over the non-glacierized areas, while the glacier runoff supplied by GloGEM is unchanged. In the uncoupled version, it rescales the precipitation over the entire catchment, including the glacierized area. This difference is intentional. It is precisely the compensation effect we set out to test, namely whether correcting the hydrological precipitation input can simultaneously reduce the overestimation of glacier melt in the uncoupled HBV setup and the underestimation of streamflow in HBV-GloGEM.

Changes in the manuscript: All of the above is now stated in the new Sect. 3.4 (“Precipitation correction”), including the distinction and independence of the two factors, their spatial domains, and the consequence for water volumes. See also the note under RC1.2 on the corrected PCF range (0.5–1.8).

RC2.4 — Initialization of model states

Initialization of model states: the initialization and spin-up of the different configurations should be clarified. Are all configurations initialized with the same SWE and storage conditions? Is SPASS used only for validation or also for initialization? Why not for calibration? In Figure 5b, the different configurations already seem to show different SWE values at the beginning of the validation period. This initial offset should be explained, as it may affect the interpretation of SWE evolution and “snow towers” development.

We thank the reviewer for these questions and clarify the initialization and use of SPASS. The simulation is split into an 11-year calibration period followed by a 10-year validation period; Figure 5b shows only the validation period. All configurations are initialized identically with all storage states set to 0. Although all configurations are initialized identically at the start of the calibration period, each setup has evolved its own snow storage state by the start of the validation period, depending on its own parameter set. So, the SWE at the start of the validation window already differs between configurations. We will state this explicitly in the caption and text, to avoid the misunderstanding the reviewer rightly flags.

SPASS is used for validation only, not for initialization or calibration. SPASS (Marty et al., 2025) is a daily 1 km gridded SWE product over Switzerland, produced by the OSHD temperature-index model with assimilation of in situ snow depth observations. We deliberately reserve SPASS for

validation rather than calibration because it is itself a model product that is structurally similar to our own temperature-index snow routine. We therefore calibrate against measured streamflow and retain SPASS as an independent dataset to evaluate whether the simulated snow dynamics are realistic.

Changes in the manuscript: Sect. 3.6 now describes the initialization scheme, states that reported metrics are computed on full periods without a dedicated warm-up, and explains why: initialization effects are confined to the first year of the calibration period, they cannot be removed consistently by a common warm-up window (the configurations without snow redistribution never reach a stationary high-elevation snow state), and the validation period starts from model states carried over from ten years of continuous simulation. The caption of Fig. 6 (SWE time series, formerly Fig. 5b) now explains the differing SWE at the start of the validation period. The use of SPASS for validation only is stated in Sect. 3.6.

RC2.5 — Extended presentation of model parameters

“Model parameters” section: this section is very short and the entire paper could benefit from a more extended description (and discussion in the “discussion” section) of the changes in model parameters (not only the melt factor) among the different configurations and how such differences are related to model overcompensation to fit the streamflow calibration objective. I see the authors have included figures in the supplement but I would appreciate a more extended presentation in the text.

We thank the reviewer for this constructive suggestion. We agree that the parameter analysis deserves a more extended treatment, and that connecting the parameter shifts to the compensation mechanism strengthens the paper’s central argument. We will expand Section 4.4 to discuss the calibrated snow parameters beyond the melt factor and connect them to the compensation mechanism. We note that the response differs strongly between parameters. Two parameters show clear and consistent behavior across catchments: the melt factor (Fig. 6), which drops markedly when glacier melt is constrained by GloGEM, and the precipitation correction factor (Fig. S22), which increases in the coupled HBV-GloGEM-PCF configuration relative to HBV-PCF. Together, these illustrate that, when the model can no longer overestimate glacier melt to close the water balance, it increases the precipitation correction instead. We will therefore include part of Fig. S22 into the main text alongside Fig. 6 and discuss this trade-off explicitly. In contrast, the remaining snow parameters (rain/snow transition temperature, refreeze factor, snow water-holding capacity; Figs. S19–S21) do not show a consistent directional response to the configuration changes; their shifts are catchment-specific and lack a clear cross-catchment pattern. We will state this explicitly and continue to refer to their distributions in the supplement (Figs. S19–S21), rather than reproducing them in the main text.

Changes in the manuscript: Sect. 4.4 was expanded and the parameter figure redesigned as announced: the new two-panel Fig. 7 shows (a) the melt factor for all six configurations and (b) the precipitation correction factor for the two PCF configurations, side by side for the nine catchments with more than 10% glacier cover, ordered by glacier area. The text now explicitly notes that the two parameters with the most consistent cross-catchment response shift in opposite directions under coupling, and states that the remaining snow parameters lack a consistent directional response, with their distributions provided in the supplement (now Figs. S20–S22). The compensation mechanism is discussed in Sect. 5.

List of all relevant changes

New material and structural changes

- New Sect. 3.4 “Precipitation correction”: sources of precipitation bias, implementation and calibration of the PCF, its distinction from GloGEM’s $f_{c,prec}$, and the rationale for dedicated PCF configurations (RC1.2, RC2.3).
- New Sect. 3.5 “Snow redistribution” with a new schematic figure (Fig. 3): HRU connectivity from D8 flow routing and the redistribution mechanism, including Masked Glacier HRU treatment (RC1.4, RC2.1). Subsequent figures renumbered accordingly.
- Expanded Sect. 3.3: routing of the aggregated GloGEM runoff, bypass of the linear glacier reservoir, and identical catchment-scale routing across configurations (RC2.2).
- Expanded Sect. 3.6: calibration setup, explicit statement that no snow/SWE data enter the calibration, initialization scheme, and the treatment and justification of the no-warm-up choice (RC1.6, RC2.4).
- Sect. 3.2.2: added the diagnosis of snow-covered area as the areal sum of snow-covered HRUs (RC1.5).
- Introduction: sharpened statement of the novel contribution (first local-scale integration of global glacier model output into a conceptual hydrological model; transferability across 14 catchments with 3–60 % glacier cover) (RC1.1).
- Discussion (Sect. 5): new paragraphs on the attribution of improvements to coupling versus glacier-data constraint, and on the snow redistribution mechanism and its simplifications (RC1.3, RC2.1).
- Limitations (Sect. 6): new limitations on (i) the entanglement of coupling and constraint, (ii) the missing ice-melt storage delay in the coupled routing, and (iii) the SWE-based snow evaluation and the role of SCA, with observed SCA and seasonal snowline observations as an outlook (RC1.3, RC1.5, RC2.2).

Results and figures

- New two-panel parameter figure (Fig. 7): melt factor and precipitation correction factor for the nine catchments with more than 10 % glacier cover; Sect. 4.4 expanded accordingly (RC2.5).
- New supplementary elevation-band SWE evaluation against SPASS for the Rhone catchment (Fig. S19), discussed in Sect. 4.3 (RC1.5, RC2.1).
- Fig. 6 caption now explains the differing SWE between configurations at the start of the validation period (RC2.4).

Corrections and consistency

- PCF calibration range: verification of the calibration setup during revision showed that the range actually used is 0.5–1.8; Table 2 and Sect. 3.4 now report this range (our discussion response stated 0.8–1.8).
- Sect. 4.1: the KGE values reported in the submitted version were mean values, although the text referred to them as medians; the revised text now reports the median values, consistent with the boxplots in Fig. 4a and Tables S2–S3.

- Abstract: the equifinality statement is now precise about scope (“Within a single model configuration, glacier melt contributions can vary by more than a factor of three among the best-performing parameter sets, and by up to an order of magnitude in weakly glacierized catchments”).
- Units and terminology standardized (melt and refreeze factors in $\text{mm d}^{-1} \text{ } ^\circ\text{C}^{-1}$; configuration naming; degree-day wording).
- References updated to published versions: Marty et al. (The Cryosphere, 19, 4391–4407, 2025) and van Tiel et al. (HESS, 30, 23–43, 2026).
- Numerous typographic and grammatical corrections throughout the manuscript and supplement.

Code and data availability

- The full modelling workflow, including the scripts that produce all figures, is now published and archived (<https://github.com/bergjus94/Raven-Switzerland>; <https://doi.org/10.5281/zenodo.21788830>).
- The modified Raven version is archived at <https://doi.org/10.5281/zenodo.21786366>; the model configurations and full simulation results are published as a data record at <https://doi.org/10.5281/zenodo.21774506>.
- The data availability statement was updated accordingly.