We thank the reviewer for their constructive comments, which will help improve the manuscript. Below we provide a point-by-point response to each comment. Reviewer comments are shown in *grey italics*, followed by our responses.

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.

Main Comments

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 mm °C^{-1} day^{-1} and a mean temperature of +3 °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.

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.

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 fc,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 catcment 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.

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.

"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.

References

Marty, C., Michel, A., Jonas, T., Steijn, C., Muelchi, R., and Kotlarski, S.: SPASS – new gridded climatological snow datasets for Switzerland: potential and limitations, The Cryosphere, 19, 4391–4407, https://doi.org/10.5194/tc-19-4391-2025, 2025.