

Response to RC3:

Review for manuscript “New insights from fully-integrated surface-subsurface hydrological modeling in high-elevation glaciated environments” by Fan et al., 2026

Comment #1: Overall, I found the manuscript well written and clearly structured. The modelling decisions are generally well supported, and the approach seems appropriate given the complexity of the system and the amount of available data. The model appears to be reasonably well constrained, especially with respect to glacier and snow processes.

I particularly appreciated the comparison between simulations with and without lateral flow. This provides a clear and interesting result: lateral flow appears to need to be inhibited in order to reproduce observed groundwater levels. However, I found the implications of this result somewhat difficult to interpret. Does this suggest that, in this system, most water transfer occurs vertically rather than laterally between cells? Or could this be a structural artefact of the modelling framework? I think this result is important and would benefit from a more in-depth discussion of its physical interpretation, limitations, and broader implications.

Answer: Thank you for your interest and for taking the time to review our manuscript. Regarding the interpretation of the results of limited subsurface lateral flow, as the subsurface lateral flow is constrained to be limited, the dominant flows that contribute to streamflow remain the groundwater baseflow and surface runoff. The surface runoff can be generated via saturation-excess runoff and infiltration-excess runoff; the rest of the water (besides evapotranspiration and water storage changes) goes vertically through the rather thin unsaturated zone (0-1.3 m based on groundwater head observations) and enters the groundwater that flows vertically and horizontally. The subsurface lateral flow, baseflow, and surface runoff are simulated separately in WaSiM and described in Section 4.3. Future works adopting other models with different model structures are welcome to further test the role of subsurface lateral flow in such environments.

We agree that the statement regarding limited subsurface lateral flow requires clearer justification in terms of physical interpretation, limitations, and broader implications. In this study, the inference of reduced subsurface lateral flow contribution is not based on direct measurements, but rather on calibrating an ensemble of hydrological simulations to the observed groundwater level and river discharge (Fig. S9-13 in the supplementary material). The limited subsurface lateral flow can be potentially explained by the landscape characteristics that influence flow partitioning, including steep topography, shallow soil depth, and limited soil water storage capacity in the upper headwater environment. These conditions are generally associated with reduced storage in the soil layers and enhanced rapid runoff generation. Soil erosion may provide additional indirect evidence for surface-dominated flow pathways. High erosion potential has been shown in the Zufallferner within the study site of Martell Valley (Altmann et al., 2025). We will revise the manuscript to clarify that our conclusion refers to the dominant modeled contributions to discharge at the catchment scale, explained with hydrological and landscape characteristics. Soil erosion studies will contribute to understanding the dominant flow pathways.

Comment #2: **Figure 1** Could the meteorological station symbols be made larger or shown in a brighter colour? It took me some time to identify the 12 stations. The piezometer labels are also somewhat difficult to read against the background; adding a light label background or halo might improve readability.

Answer: We will enlarge the meteorological station symbols and adopt a light label background or halo to make the piezometer labels easier to identify and read.

Comment #3: **Line 116** The phrase “are requested” sounds slightly awkward. Would “are obtained” or “were obtained” be more appropriate? Also, because the stations are not labelled in Figure 1, it is difficult for the reader to know where specific stations such as Zufall or Careser are located. It may be helpful either to label them in Figure 1 or to move some of this information to the data availability statement.

Answer: We will use “are obtained” on Line 116. To avoid making Figure 1 too crowded with many labels, we will move some of this information about specific stations to the data availability statement.

Comment #4: **Table 1** Why is wind speed abbreviated as “WG”? “WS” may be more intuitive, unless there is a specific reason for using “WG”.

Answer: We will adopt “WS” as the new abbreviation for wind speed. The “WG” was adopted because speed in German is “Geschwindigkeit”, given that our research consortium and the research area are German-speaking.

Comment #5: **Line 142** I am not sure the reader needs all the details about who installed each station. It may be sufficient to include the years of operation and where the data can be accessed. The installation details could potentially be moved to the data availability statement. Similarly, if the model ultimately uses daily data, is the measurement interval necessary here?

Answer: We agree and will move the installation details to the data availability statement. Our model uses the daily data, but the hydrological measurement interval was mentioned in case the readers may want to use the subdaily measurement data for their study in the future.

Comment #6: **Line 147** November is not technically winter, so “at the onset of the low-flow season” may be more accurate. I would also be cautious about stating that a single water sample collected at the end of fall helps characterize winter interactions. This may be true in a limited sense, but the current wording may overstate what can be inferred from one sampling period.

Answer: We will adopt the “at the onset of the low-flow season” instead of “winter”. Regarding the water sampling, they were collected in two sampling periods, i.e., in November 2022 and

November 2023. We agree to be cautious with this statement by acknowledging the limitations and uncertainty associated with the limited number of water samples in the revision.

Comment #7: Line 159 Could you clarify how reasonable the soil profile and soil type assumptions are? Were these compared with any field measurements? The 1 km × 1 km resolution seems coarse compared with some of the other spatial inputs, so it would be helpful to briefly justify this choice or discuss its implications.

Answer: The adopted spatial resolution of soil data is rather coarse, which is primarily limited by the sparse field observations available in the high mountain environments. A slightly higher resolution of soil product (e.g., SoliGrids in 250m*250m) may not necessarily improve the simulations significantly because of limited field observations in the high elevations. In this study, we used the land use map (such as the presence of rocks, forests, and grasslands) to evaluate and modify the soil type data, as another layer of validation of the soil data. The HWSD product was adopted as it remains widely used in regional modeling due to harmonized and internally consistent soil attributes across broad spatial extents. We agree to justify this choice and discuss the potential benefits of high-resolution soil products in future work.

Comment #8: Line 165 It sounds like the model is calibrated to glacier area change and evaluated against mass balance. This is useful information, but I wonder whether it belongs in the spatial data section or would fit better in the modelling approach section.

Answer: As there is no observed glacier mass balance data available for the whole catchment, we adopted the observed glacier area changes to calibrate the glacier module in the hydrological model. The observed glacier mass balance data of Langerferner, which is one of the large glaciers in the catchment, are available and are compared with the simulated glacier mass balance in the corresponding subcatchment where the Langerferner is located, as another layer of evaluation of the ice module performance. In the revision, we will mention the availability of spatial glacier coverage in this spatial data section and put the details of calibration and evaluation of the glacier module in the modeling approach section.

Comment #9: Line 192 “Flexibly activated” is sufficient; “activated/deactivated” is probably not necessary, since deactivation is implied.

Answer: We will adopt the “flexibly activated” and remove “deactivated” to avoid repetition.

Comment #10: Line 197 This paragraph seems to mix the general model description with specific parameter choices. For example, why was the range of -2 to +2 selected? Were these values calibrated or assumed? This information may fit better in Section 4.4, where model parameterization and calibration decisions are discussed.

Answer: The parameter “Trans” governs the temperature transition range from snow to rainfall, which is calibrated. A “Trans” calibrated to the value of 2 means a temperature transition range of -2 to +2 °C in the model, when the “TRS” (which is also a calibrated parameter meaning the temperature at which 50% of precipitation falls as snow) is equal to 0 °C. We agree to put this information in the Calibration Parameters subsection in Section 4.4.

Comment #11: **Line 199** If the bucket approach is not used, it may not need to be mentioned here.

Answer: The classic bucket approach is used for calculating liquid and solid precipitation interception by the canopy in the model. We will clarify this point in the revision.

Comment #12: **Line 201** Could the algorithms be named more specifically, if they have formal names? The phrase “state of the art” feels somewhat overstated unless the methods are identified and their status is briefly justified.

Answer: There is indeed no formal name for these algorithms for calculating wind and gravitational redistribution of snow. Therefore, we only mentioned the references associated with the development of these algorithms. We will remove the word “state of the art” to avoid the feeling of overstatement.

Comment #13: **Line 332** The sentence beginning “as global warming continues...” could likely be removed, as it introduces a broader climate-change framing that is not needed for the calibration discussion. If you want to contextualize the simulated value of -126 mm yr^{-1} , it may be more useful to compare it with existing glacier mass-balance estimates, for example from Dussaillant et al. (2025).

Dussaillant, I., Hugonnet, R., Huss, M., Berthier, E., Bannwart, J., Paul, F., and Zemp, M.: Annual mass change of the world's glaciers from 1976 to 2024 by temporal downscaling of satellite data with in situ observations, *Earth System Science Data*, 17, 1977–1991, 2025.
<https://doi.org/10.5194/essd-17-1977-2025>

Answer: We agree to remove this sentence. Instead, we will compare our result with the glacier mass balance decreasing trend in central Europe shown in the suggested literature.

Comment #14: **Line 342** Could you clarify how snowmelt and glacier melt are distinguished in the model? For example, is snowmelt occurring on glacierized cells counted as snowmelt, or is it included in glacier melt? This distinction is important for interpreting the reported melt contributions.

Answer: The hydrological model WaSiM calculates and outputs the snowmelt on the non-glacierized cells, snowmelt on the glacierized cells, firn melt on the glacierized cells, and ice melt on the glacierized cells. We calculate the total snowmelt as the sum of snowmelt occurring

both on the glacierized and non-glacierized cells. We will clarify this point in the revision when reporting the melt contributions.

Comment #15: **Line 352** The sentence beginning “Overall…” may not be necessary here. The point seems more appropriate for the introduction or broader framing rather than this section.

Answer: We agree and will remove this sentence in this section.

Comment #16: **Figure 2** “Warm-up period” is understandable, but “spin-up period” is more commonly used in the modelling literature. Consider changing the terminology for consistency.

Answer: We will adopt “Spin-up period” instead of “Warm-up period” in the revision.

Comment #17: **Figure 3** Could you clarify the reference level for river water level? The river appears to be above 0, so it is not immediately clear what 0 represents. Is this water level above the riverbed, with 0 indicating dry conditions? Or is it referenced to ground surface or another datum? A short clarification in the caption or text would help.

Answer: The reference level of 0 is the ground surface, with 0 indicating dry conditions and no water in the riverbed. We will clarify this point in the caption.

Comment #18: **Figure 5** Could the line colours in panels c and d reflect the TWI classes shown in panels a and b? It may be clearer to group all simulations by TWI rather than by selected groundwater ID. At present, it is difficult to identify the IDs in panels a and b, and the groundwater borehole cell colour is similar to the TWI colour scale, which makes the figure harder to interpret. Also, where are the time series for wells 4473 and 4472?

Answer: We agree with the suggestion and will color the lines in (c) and (d) based on the TWI classes. We will also use another color for groundwater boreholes to make them more easily identifiable. The simulations for wells 4473 and 4672 will be provided in the supplementary materials in the revision.

Comment #19: **Line 420** I found this conclusion somewhat vague. What does this result imply about the use of individual grid cells for comparison with groundwater observations? It could suggest that well-location selection needs to be done very carefully, that multiple neighbouring cells should be considered, or that some mismatch between real-world point observations and model grid cells is unavoidable. Since this issue is discussed more fully in Section 6.3, you could either clarify the implication here or leave the interpretation for the discussion section.

Answer: Thanks for pointing this out. Both implications mentioned by the reviewer are indeed correct and will be incorporated into the discussion in the revision. As the reviewer noticed, this issue has been discussed fully in Section 6.3, we would avoid mentioning the implication here in

the Results section. Instead, we will point the reader to Section 6.3 for the detailed discussion, and we will enhance the discussion of the implications in this discussion section.

Comment #20: Figure 6 / Line 432 I think the description of low-flow performance at Pilam needs to be more careful. In Figure 6, there appears to be winter flow at S7, while the model simulates zero flow. Describing this as a “slight underestimate” seems misleading if the model is simulating no flow during periods when observations indicate some winter discharge. It also looks as though some winter periods may have measurement issues, while others, such as 2019–2020, show measurable winter flow. The 2021 winter drop also looks suspicious and may need explanation. Overall, I suggest revising the text to more explicitly acknowledge that the model does not fully capture winter low-flow dynamics at S7.

Answer: We agree and will revise the text about interpreting the low-flow performance at the Plima Station shown in Figure 6(b). We will also be more careful when interpreting this station result by explicitly acknowledging the different model performance in specific years due to modeling and observational issues.

Comment #21: Line 457 The difficulty of the modelling task has already been mentioned several times. This sentence could probably be shortened or removed to avoid repetition.

Answer: We will shorten or remove this sentence to avoid repetition in the revision.

Comment #22: Figure 8 I found the distinction between percolation and groundwater recharge confusing. Percolation is often understood as water draining toward groundwater, while groundwater recharge is commonly interpreted as the amount of water entering the groundwater system. In the figure, groundwater recharge appears much lower than percolation, which makes me wonder whether “groundwater recharge” is actually being used to mean net groundwater storage change or net groundwater volume change. If so, a different term may be clearer. Otherwise, the current terminology may be misleading.

I also wondered whether the model assumes a constant head beneath the glacier. If so, was this described earlier in the manuscript? Is that assumption based on model structure, observations, or another constraint?

Answer: In the WaSiM model, the “Percolation” output is defined as the vertical flux into the groundwater system, while the “groundwater recharge” output is defined as the net change of groundwater storage after calculating both vertical and horizontal inflow and outflow. We have adopted the same terminology as the WaSiM model and its user manual when showing this result. To avoid confusion, we will clarify the definition of “percolation” and “groundwater recharge” in the caption of the figure.

The groundwater heads are assumed to be constant beneath the glacierized cells, and this assumption is a built-in feature in the WaSiM model. Given that there is rare continuous observation of groundwater level beneath the glacier, it is challenging to estimate how

groundwater may (or may not) fluctuate beneath the glacier covered region. We will clarify this assumption in the model description in Section 4.2 in the revision.

Comment #23: Figure 9 When discussing this figure in the text, could you also include the terms “gaining stream” and “losing stream”? These terms are widely used to describe groundwater–surface-water exchange and would help make the interpretation clearer.

Answer: Thanks for the suggestion. We will use the terms “gaining stream” and “losing stream” when interpreting the results of this figure in the text in the revision.

Comment #24: Line 515 The direct quotation feels somewhat unnecessary. The same point could likely be paraphrased and cited instead.

Answer: We will paraphrase this point instead of a direct quotation in the revision.

Line 518 I would like to see a more detailed discussion of the implications of lateral flow. Why does this result matter? What physical conditions could explain the need to inhibit lateral flow? Alternatively, could this reflect limitations of the model structure or spatial discretization?

Answer: Thanks for raising this important point, and we agree to enhance the discussion of the implications of subsurface lateral flow. In our model, subsurface lateral flow was not assumed to be zero a priori; rather, it was constrained to zero after evaluating an ensemble of hydrological simulations, as shown in Figs. S9-S13 of the Supplementary Material. The limited subsurface lateral flow can be potentially explained by the landscape characteristics that influence flow partitioning, including steep topography, shallow soil depth, and limited soil water storage capacity in the upper headwater subbasins in the study area. These physical conditions are generally associated with reduced subsurface storage in the soil layers and enhanced rapid runoff generation. Soil erosion processes may provide additional indirect evidence for surface-dominated flow pathways when the subsurface lateral flow is limited. High erosion potential has been shown in the Zufallferner within the study site of Martell Valley (Altmann et al., 2025).

In the WaSiM model, the subsurface lateral flow in each soil layer and each grid cell is generated based on the water content of the soil layer, hydraulic conductivity, and local slope. The generated subsurface lateral flow at the grid cells is then transformed to the stream with a single reservoir cascade approach (see details in the work of Schulla, 2024). Future research is welcome to adopt other models with different model structures to further test the role of subsurface lateral flow in the high-elevation headwater environment. Regarding spatial discretization, we have adopted a fine spatial resolution of 25m * 25m after testing the 50m*50m and 100m*100m resolutions for this high-elevation setting. The currently adopted spatial resolution is considered sufficient given the available spatial information of the study site and the field conditions. We will discuss the implications of limited subsurface lateral flow in more detail in the revision.

Comment #25: **Line 525** You could also cite Aubry-Wake et al. (2024), who used shallow groundwater head observations in a fully integrated hydrological modelling study of glacier–groundwater interactions. That study was HRU-based rather than fully distributed, but it is still relevant to the use of groundwater observations for constraining glacierized basin models:

Aubry-Wake, C., McNamara, G., Somers, L. D., McKenzie, J. M., Pomeroy, J. W., & Hellström, R. (2024). *Sensitivity of surface water and groundwater contributions to streamflow in a tropical glacierized basin under climate change scenarios*. *Environmental Research Letters*, 19, 114036. <https://doi.org/10.1088/1748-9326/ad7c68>

Answer: Thanks for mentioning this relevant literature. We will discuss this work in our manuscript in the revision.

Comment #26: **Line 567** “Outrageous” feels too strong and informal. Possible alternatives include “unlikely,” “physically improbable,” “counterintuitive,” or “inconsistent with established understanding of flow behaviour.”

Answer: We will adopt “counter-intuitive” or similar to replace “outrageous” in the revision.

Comment #27: **Line 779** I really liked this section. It was clear and helpful.

Answer: Thanks for the appraisal of the value of our work. Glad to hear that.

References

Altmann, M., Fischer, P., Haas, F., Pfeiffer, M., Ramskogler, K., Rom, J., Kara-Timmermann, D., Himmelstoß, T., Heckmann, T., and Becht, M.: Simulation of soil erosion in the Little Ice Age glacier foreland of the Zufallferner (South Tyrol, Italy) using Erosion-3D, EGU General Assembly 2025, Vienna, Austria, 27 Apr–2 May 2025, EGU25-5734, <https://doi.org/10.5194/egusphere-egu25-5734>, 2025.

Schulla, J.: Model Description WaSiM (Water balance Simulation Model), http://www.wasim.ch/downloads/doku/wasim/wasim_2024_en.pdf, 2024.