Response to RC2:

Review of the Manuscript

This is a good study that demonstrates significant effort in conducting extensive fieldwork and modeling works. However, I believe the manuscript still needs improvements. I've outlined several comments to assist in this process, as there is potential for enhancement during revision (before publication).

Answer: Thank you for your interest and for taking the time to review our manuscript. We responded to each comment below and planned ways to implement the comments to enhance the manuscript in the revision.

Major Issues

Comment #1: Structure and language: The organization and clarity of the paper should be enhanced to improve comprehension.

Answer: We will focus on improving the organization and clarity of the manuscript by following the reviewer comments below during the revision.

Comment #2: Subsurface flow concerns: I remain unconvinced by the claims regarding limited subsurface flow contributing to river flow.

Answer: We acknowledge the concern of the reviewer, which is also mentioned in the specific comments below at L11-13 (Comment #4) and L657-659 (Comment #18). We agree that the limited contribution of subsurface lateral flow to river discharge needs to be justified more clearly and stated with appropriate caution. In our study, this interpretation is not based on direct subsurface flow measurements. Rather, it emerges from the calibrated ensemble of hydrological simulations, which was evaluated against both groundwater level and river discharge observations. Across this ensemble, subsurface lateral flow was consistently constrained to low values, as shown in Figs. S9-S13 of the Supplementary Material. We will revise the manuscript to make this basis explicit and to avoid overstating that rapid subsurface flow processes are absent.

Our conclusion refers specifically to the dominant modeled contributions to river discharge at the daily catchment scale. Under the simulated conditions, streamflow is primarily explained by groundwater baseflow and surface runoff, while subsurface lateral flow contributes only marginally. This interpretation is also consistent with the landscape characteristics of the upper headwater subbasins, including steep topography, shallow soils, and limited soil water storage capacity, which favor rapid runoff generation and restrict subsurface storage. In addition, recent evidence of high erosion potential near the Zufallferner within the Martell Valley catchment (Altmann et al., 2025) supports the relevance of surface runoff during rainfall and melt events. We agree that erosion indicators provide useful complementary evidence for identifying dominant flow pathways, although erosion was not explicitly quantified in the present study. Accordingly, we will revise the relevant sections, including the abstract and Lines 657-659, to clarify that our interpretation is based on calibrated hydrological modeling rather than direct subsurface flow measurements. We will also identify erosion processes and event-scale runoff generation as complementary future works to further test the role of subsurface lateral flow in high-elevation headwater floodplains.

Specific Comments:

Comment #3: Abstract Lines 9-11: The phrase “highlighting the need for improved subsurface parameterization” is confusing. Did you address this in your manuscript? If so, how? If it's a future recommendation, how do you plan to achieve it?

Answer: Yes, and this is indeed a key improvement in our study. This study is among the first to adopt a fully-distributed and fully-integrated surface-subsurface hydrological modeling approach in a glaciated high Alpine setting. In contrast to the widely adopted simplified representation of subsurface processes, such as by adopting a linear reservoir or by transferring the outflow from unsaturated zone directly to the streams without a groundwater module (Coderre et al., 2026), this study adopts a spatially-distributed hydrological model which solves the hydrological processes in the unsaturated zone with the Richards equation and solves the groundwater flow with Darcy’s equations in two dimensions, which represents an improved subsurface parameterization in hydrological modeling in high Alpine environments. More details of how we address subsurface modeling and parameterization are provided in the manuscript in Section 4.2.

Comment #4: Abstract Lines 11-13: The statement about subsurface lateral flow being constrained to zero and its limited influence on river discharge is questionable. Could this subsurface flow be as rapid as overland flow? What is your model's temporal resolution? If overland flow dominates, can you cite evidence for this, such as land erosion?

Answer: We thank the reviewer for raising this important point. We agree that the role and potential timescale of subsurface lateral flow should be stated more carefully. In our model, subsurface lateral flow was not assumed to be zero a priori; rather, it was constrained to zero after evaluating a large ensemble of hydrological simulations, as shown in Figs. S9-S13 of the Supplementary Material. This indicates that, at the daily temporal resolution and 25 m x 25 m spatial resolution of our model, subsurface lateral flow does not make a significant contribution to simulated river discharge compared with groundwater baseflow and surface runoff.

The model represents subsurface lateral flow, groundwater baseflow, and surface runoff as separate flow components (Section 4.3). Therefore, rapid runoff responses are primarily attributed to surface runoff in the calibrated simulations, while the groundwater component represents slower baseflow contributions in the headwater floodplain. We acknowledge, however, that our model resolution does not allow us to rule out very rapid preferential subsurface flow processes at shorter, event-based timescales. We will therefore revise the manuscript to clarify that our conclusion refers to the dominant modeled contributions to discharge at the daily catchment scale, rather than to the complete absence of fast subsurface flow processes.

As supporting evidence for the importance of surface processes, a recent high-temporal-resolution soil erosion study in the catchment reported high erosion potential near the Zufallferner during rainfall and melt events (Altmann et al., 2025). This is consistent with the interpretation that overland flow can be an important process in the upper headwater floodplains. In the revision, we will add this reference and highlight erosion and event-scale runoff generation as relevant future works to further test the role of subsurface lateral flow in high-elevation headwater floodplains.

Comment #5: Abstract Lines 14-16: Please clarify how point scale groundwater observations were incorporated in your modeling. Or did you just use them for model validation or calibration?

Answer: We have used the point-scale time-series groundwater level observations to calibrate the subsurface module parameters in the model. Specifically, we introduced methods regarding how to use point-scale groundwater observations to calibrate the spatially fully-distributed hydrological model. This is done by extracting an ensemble of time-series groundwater level simulated at the grid cells around the groundwater borehole locations in the model domain → evaluating them with the observed groundwater level at the borehole locations → further calibrating the subsurface module parameters to improve model simulations. Furthermore, we have shown in Figure 5 the challenges of using point-scale groundwater borehole observations to calibrate a fully-distributed hydrological model and discussed this issue in detail at L412-422 in Section 5.3. In summary, it is crucial to use an ensemble of groundwater simulations extracted from the modeling domain to calibrate the groundwater module, in contrast to the intuitive approach of only looking at a single groundwater level simulation at the borehole location. Additionally, we suggest calculating the Topographic Wetness Index (TWI) beforehand to make wise decisions on where to install groundwater boreholes, given that the locations with high TWI values are challenging to use for calibrating subsurface parameters. Such implementations are fully discussed in Section 6.3.

Comment #6: Research Questions: The connection between research questions and the main text is unclear. Specifically, what do you mean by "the commonly adopted more simplified" in research question (iii)? How does your study differ from existing researches (literature)? Please clarify them. Clarification on how point-scale groundwater observations address your earlier identified research gaps is essential.

Answer: Here "the commonly adopted more simplified" approach points to the overly simplified representation of the subsurface (i.e., the unsaturated zone and the groundwater) in many hydrological models, such as the lumped-parameter hydrological models (e.g. HBV (Seibert and Bergström, 2022)) and semi-distributed hydrological models (e.g. mHM (Kumar et al., 2013), THREW-T (Nan et al., 2021)). In these models, the subsurface is generally simulated with a single linear reservoir. In another more simplified manner, the percolation (leaked water) from the bottom layer of the soil is even directly transferred to the stream to fulfill the water balance due to the lack of a groundwater module (Coderre et al, 2026). Therefore, in our study, we have made efforts to fill in this knowledge gap by improving subsurface parameterization in the hydrological model for high Alpine environments: the unsaturated zone is solved with the Richards equation and the groundwater model is solved with the Darcy's equation in two dimensions in detail

(Schulla, 2024). The point-scale groundwater level observations were used to calibrate these subsurface modules, as explained in the answer to Comment #5. We have described the calibration of the subsurface module in Section 4.2, the results were shown in Section 5.3, and their implication for future modeling and field installation of piezometers was discussed in detail in Section 6.3. We will further improve the clarification of this aspect in the revision.

Comment #7: Line 75: The term "alone" needs clarification.
Answer: Here the “alone” means an integrated surface-subsurface hydrological model is adopted, which allows to model both surface and subsurface processes temporally and spatially continuously within WaSiM itself. This is opposed to the approach of adopting a hydrological model (e.g. SWAT) and coupling it externally with another groundwater model (e.g. MODFLOW).

Comment #8: Line 76: Could you explain “external coupling framework”?

Answer: The “external coupling framework” means, for example, to couple a hydrological model (e.g. SWAT) with an ice model or a groundwater model (e.g. MODFLOW) (Ice model + hydrological model + groundwater model). This happens when a hydrological model does not have these specific modules, but it needs to be applied to model the environment with ice cover or for improved groundwater simulations. To apply this type of hydrological model in the glaciated environment, the researchers need to first run the ice model, then give the outputs of the ice model as the inputs to the hydrological model, and finally give the outputs of the hydrological model as inputs to the groundwater model. The external coupling approach often disconnects the interactions and processes spatially between the surface and subsurface, which are simulated separately with different models.

Comment #9: Line 81: The phrase “with all its limits” should be reconsidered for clarity. “A missing piece in the literature”: This phrasing may imply you’re the first; please revise, as while you may be the first to implement WaSim in high-elevation glaciated environments, you may not be the first to model in this setting?
Answer: No model is perfect with pros and cons, especially given the challenges of modeling complex topography and processes in a high-elevation glaciated catchment. The WaSiM model, as one of the most important hydrological models for simulating high Alpine environments, also has its limits besides the numerous advantages. We have discussed the limits of applying WaSiM in detail in Section 6.4 Limitations. Regarding the novelty, it is not only because of implementing WaSiM in such a complex environment, but it is more because a spatially-distributed and fully-integrated surface-subsurface hydrological modeling approach in general has been rarely applied to modeling such an environment. We will revise this phrase as “rarely studied”.

Comment #10: Table 4: There appear to be no spatial variations for the parameters presented. Could you clarify this assumption? If no spatial variation exists, your claim of an “improved” subsurface schematization is, perhaps, questionable?

Answer: The subsurface parameters are not fully spatially homogeneous. We applied the key subsurface heterogeneity by calibrating the hydraulic conductivity for each soil layer and for the soil in each land cover category (rocks, grassland, forest). The “ksat” in Table 4 is provided for the soil in each land cover, and its range indicates the variations of hydraulic conductivity between the soil layers. We will clarify this point in the revision. Additionally, given that all installed five groundwater piezometers are located in the same hydrogeology (sedimentary aquifer, see Fig. 1a), other parameters are thus assumed to be the same. The modeling results are with high confidence in the upper headwater floodplain where the piezometers are installed, and the results for the whole catchment provide an overview despite lower confidence (L105-110 in the manuscript). Compared with the widely adopted simplified representation of subsurface in hydrological models, such as a single linear reservoir or no groundwater model at all (Coderre et al, 2026), our implemented spatially-distributed subsurface parameterization with key variations in hydraulic conductivity for different soil layers and land cover represents a key step forward for understanding surface-subsurface hydrological interactions and for improving water partitioning between surface and subsurface.

Besides that, more detailed heterogeneity of the subsurface characteristics is challenging to apply due to the rarely available soil content mapping in the glaciated high-elevation catchments. Many studies on the topic of stochastic hydrogeology show the challenges of estimating the spatially variable subsurface parameters even within an extremely small experimental area. Assigning heterogeneous subsurface properties in the ungauged areas involves numerous assumptions. We therefore applied a conservative approach to ensure the key subsurface heterogeneity. Exploring the impact of even high-level spatially variable subsurface parameters is out of the scope of this study.

Comment #11: Chapter 4: What is the temporal resolution of your model? I couldn’t locate this information in the manuscript.
Answer: The temporal resolution of the model is daily, which is mentioned in Section 3.1 L113 when we introduce the daily meteorological data as model inputs.

Comment #12: For soil analysis, I'm just wondering why not using SoliGrids (250 m resolution) for accuracy.
Answer: We acknowledge that the SoliGrids provides a finer resolution of 250m compared with the one (HWSD) that we adopted of 1km. However, given the uncertainty of global soil datasets in high mountain regions, using a slightly finer global soil product would not necessarily improve the simulations significantly. In the high mountainous regions, a finer-resolution global soil product such as SoliGrids still contains high uncertainty due to sparse field observations. Besides that, the HWSD product remains widely used in regional modeling because of harmonized and internally consistent soil attributes across broad spatial extents. We agree to strengthen the discussion of this limitation and the potential benefits of high-resolution soil products in future work.

Comment #13: Section 6.1: I think many paragraphs of this section should be moved to the Introduction. This part showed what has been done in previous researches and how your study cover the research gaps.

Answer: This section aims to discuss what has been achieved in this study based on the adopted modeling approach and the results in comparison with those in the existing literature. We will improve the clarity and structure of this section in the revision.

Comment #14: Lines 350-354: The flow of these sentences could be improved; the conclusion about declining water availability seems unsupported by the previous sentences. While glacier melt (10 mm/year) may offset declining rainfall (-4.5 mm/year), could there be times when glacier melt ceases?

Answer: We will improve the flow of these sentences and clarity. Note that these results are provided as annual trends. Besides the rainfall shows a trend of -4.5 mm/year (high uncertainty, p-value=0.50) and the glacier melt +10mm/year (p-value=0.16), we also show the snowmelt trend of -26 mm/year (p-value=0.10), which is a more likely trend for future projections, indicating declining water availability on an annual scale. With global warming and accelerating glacier retreats, the glacier melts in many high Alpine catchments with small glaciers in Europe are projected to cease before 2090 (Rounce et al., 2023; Huss and Hock, 2018).

Comment #15: Figure 8: The groundwater heads appear to represent groundwater depth (or depth to the water table).

Answer: The groundwater heads represent the depth to the water table, i.e. the distance between the ground surface and the groundwater table.

Comment #16: Lines 474-476: Clarification is needed regarding recharge. are you implying that some of it is released outside your study area? How significant is this recharge?

Answer: We will improve the clarity of these sentences. Here the “recharge” refers to the water flowing from groundwater to the river (i.e., the river is recharged by the groundwater). Regarding the magnitude, our results show that shallow groundwater contributes significantly (up to 40%) to the streamflow in winter. Deep groundwater was not measured and out of scope of the modeling task, but it is unlikely to contribute significantly to the streamflow in the upper headwater floodplain and more likely in the valley floor (see Fig.1b in the work of van Tiel et al. (2024)).

Comment #17: Lines 645-646: The reasoning for being unable to model permafrost due to a lack of soil temperature data is unclear. If such data are not existing, can we just simulate these temperature?

Answer: It would be possible to simulate the freezing and thawing processes in the WaSiM model if the observed soil temperature data are available. However, without the measured soil temperature data, it would not be possible for WaSiM to calibrate the related parameters in the corresponding Permafrost module and to calculate the freezing and thawing depth in the soil.

Comment #18: Lines 657-659: What evidence supports the claim of limited subsurface flow? If fast flow is primarily overland, then soil erosion may also provide insights.

Answer: Thanks for the insightful comment. We have answered this specific comment together with the major comment #2 and specific comment #4 on this topic. To restate it here, we agree that the statement regarding limited subsurface flow requires clearer justification. In this study, the inference of reduced subsurface flow contribution is not based on direct measurements, but rather on hydrological simulations and landscape characteristics that influence flow partitioning, including steep topography, shallow soil depth, and limited soil water storage capacity in the study area. These conditions are generally associated with reduced subsurface storage and enhanced rapid runoff generation. We also agree that soil erosion processes can provide additional indirect evidence for surface-dominated flow pathways. High erosion potential has been shown in the Zufallferner within the study site (Altmann et al., 2025). While erosion was not explicitly quantified in the current analysis, we acknowledge that integrating erosion indicators would strengthen the interpretation of dominant flow pathways. We will revise the manuscript to clarify that our interpretation is based on hydrological modeling rather than direct subsurface flow measurements, and we will highlight erosion and event-scale runoff generation as relevant future works to further test the role of subsurface lateral flow in high-elevation headwater floodplains.

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