

Answers to Anonymous Referee #1 comments on the paper “Assessing the seasonal compartmentalization of water fluxes in the soil-plant-atmosphere continuum of a high-elevation mountain grassland”

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The study titled “Assessing the seasonal compartmentalization of water fluxes in the soil-plant-atmosphere continuum of a high-elevation mountain grassland” is interesting and investigated seasonal partitioning of water sources supporting plant transpiration and deep drainage using isotopic data and HYDRUS-1D modelling. While the topic is of relevance, the manuscript requires substantial improvements. Below, please find the main and the specific comments regarding the manuscript:

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We thank the Anonymous Referee #1 for the overall assessment of our manuscript and for recognizing both the relevance of the topic and the interest of the investigated theme. All the comments and feedback received are constructive and helped to improve the manuscript. Accordingly, we thoroughly revised the paper to address all main and specific comments. Below, we provide our responses to the comments of Anonymous Referee #1, together with our revisions.

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We hope that the revisions satisfactorily address the Anonymous Referee #1 comments and contribute to improving the quality and clarity of the manuscript, ultimately allowing it to meet the requirements for publication in *Hydrology and Earth System Sciences*.

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1 Main Comments

1.1 Abstract

The abstract clearly motivates the topic and outlines the modelling framework and objectives, but it reads more like an extended introduction and does not provide enough quantitative evidence to support its main claims. In particular, statements that the framework “accurately” simulates VWC/AET/isotope dynamics should be accompanied by performance metrics, and the key ecohydrological-separation findings should be reported quantitatively (e.g., representative SOI ranges or an effect size) rather than described only in qualitative terms such as a “higher degree of separation” or a “greater proportion of winter water” in 2022.

We thank the Anonymous Referee #1 for this comment. We revised the Abstract by including quantitative performance metrics to support the evaluation of the model performance. In addition, qualitative expressions such as “a higher degree of separation” and “a greater proportion of winter water” were replaced or complemented with quantitative information. Below, we report the original sentences from the manuscript, followed by our revised versions.

L28-29 of the old manuscript: “The results reveal the effectiveness of the proposed modelling framework in accurately simulating volumetric water content, actual evapotranspiration, and isotope dynamics at the study site.”

Have been replaced with:

L28-30: “*The results demonstrate the effectiveness of the proposed modelling framework in accurately simulating volumetric water content (mean absolute error, $MAE \approx 0.03 \text{ cm}^3 \text{ cm}^{-3}$ at 10, 20, and 40 cm), actual evapotranspiration ($MAE = 0.62 \text{ mm d}^{-1}$), soil ($MAE = 1.6, 3.6, 3.7 \text{ ‰}$ at 10, 20, and 40 cm) and xylem ($MAE = 1.85 \text{ ‰}$) isotope content at the study site.*”

L29-34 of the old manuscript: “Based on the model outputs, a higher degree of separation between the water used by plants and the water contributing to deep drainage is observed during intense snowmelt periods. Under these conditions, meltwater (winter water) rapidly drains through the lower soil layers, whereas rainfall (summer water), which predominantly occurs after the snowmelt period, remains in the soil longer, sustaining plant transpiration. However, in 2022, we observed a shift in hydrological functioning: a greater proportion of winter water contributed to transpiration fluxes under drought conditions.”

Have been replaced with:

L30-39: *“Based on the model outputs, a separation between the water used by plants and the water contributing to deep drainage is evident during the 2018–2020 growing seasons, when median Seasonal Origin Index (SOI) values of transpiration and deep drainage are positive and negative, respectively. Here, positive and negative SOI values indicate an overrepresentation of summer- and winter-derived water in the considered flux. This separation weakens in 2021, when drainage water exhibits a slightly positive median SOI value. Over intense snowmelt periods, meltwater (winter water) rapidly drains through the lower soil layers, whereas rainfall (summer water), which predominantly occurs after the snowmelt period, remains in the soil longer and sustains plant transpiration. The median SOI of transpiration remains consistently positive during the 2018–2021 growing seasons ($SOI = 0.19–0.69$) but shifts to a negative value in 2022 ($SOI = -0.07$), highlighting that winter-derived water becomes overrepresented in transpiration fluxes under snow drought conditions.”*

1.2 Introduction

The Introduction section is well written; however, it needs the following improvements:

1. It consists of paragraphs that do not have a concluding sentence at the end of each paragraph. Therefore, some of the paragraphs need to be revised as mentioned in the specific comments.

We thank Anonymous Referee #1 for this comment. As also addressed in the specific comments, we revised the Introduction by adding clear concluding sentences to the paragraphs summarizing previous studies. These closing sentences synthesize the key insights from the reviewed literature and explicitly highlight the main message that each paragraph is intended to convey in the context of the present study. Please refer to our responses to the specific comments, where we report the concluding sentences we added at the end of the relevant paragraphs. These concluding sentences are also intended to provide a clear transition to the subsequent paragraphs, thereby improving the overall flow and clarity of the text.

2. The introduction lacks a proper objective paragraph. The introduction section needs an objective paragraph in which the authors should mention the following points (in one single concise paragraph as the last paragraph of the introduction section):
 - a. A sentence summarizing the research need
 - b. Stating the main objective of the research
 - c. Stating the specific objectives of the research
 - d. A sentence regarding the value of the study

Therefore, the last two paragraphs of the introduction section should be revised.

We thank Anonymous Referee #1 for this comment. In response, we revised the last two paragraphs of the Introduction and merged them into a single, concise objective paragraph. The revised paragraph will (i) summarize the research need, (ii) state the main objective of the study, (iii) outline the specific objectives, and (iv) highlight the value of the work by emphasizing its contribution to provide new insights into alpine grassland ecohydrology. Please refer to the corresponding specific comment for further details regarding our revised version of the last two paragraphs of the introduction section.

1.3 Methodology

The methodology section is clearly written in detail, however, a performance evaluation section with corresponding metrics to evaluate the modelling is missing and needs to be added. Moreover, the authors should use past tense and passive sentences throughout the manuscript which needs revision. Further minor comments are available in specific comments.

We thank Anonymous Referee #1 for this comment. In response, we added a dedicated model performance evaluation section (see Section 3.3 *Model performance and Compartment-based calibration/validation approach* of the revised manuscript) to the Results and Discussion section, including appropriate quantitative performance metrics to assess the modelling results. In addition, we revised the manuscript to ensure consistent use of past tense and passive sentences. Further minor issues raised by the reviewer are addressed individually in our responses to the specific comments.

1.4 Results & Discussion

The Results & Discussion section is informative; however, it needs addressing the following points:

1. The authors calibrated the model using one set of variables and validated it using a different set of variables, but both steps were conducted over the same time period. This raises two concerns: (a) the calibration and validation periods are not independent, so the exercise does not represent out-of-sample validation in the usual sense; and (b) because the validation targets differ from the calibration targets, it is unclear whether the model continues to reproduce the calibrated variables correctly during the validation window, or whether good agreement for the validation variables is achieved through compensating errors/equifinality. Please justify the rationale for this approach!

We thank Anonymous Referee #1 for this thoughtful and important comment. We appreciate the opportunity to clarify the rationale behind our calibration and validation strategy.

The choice to calibrate and validate the model over the same time period was driven by the primary objective of this study. Our goal is not to use the model for predictive purposes, which would indeed require a temporally independent out-of-sample validation, but rather to achieve a process-based reconstruction of the

ecohydrological dynamics that occurred during the 2017–2023 period. Given the complexity of high-elevation environments and the temporal resolution of the isotope sampling, the available data alone allow only preliminary insights into system behavior. For example, the seasonal compartmentalization of water fluxes was already suggested by the dual-isotope plot, as discussed in the manuscript. However, a quantitative assessment of the processes leading to this compartmentalization requires a model capable of consistently reproducing the occurring dynamics both temporally (at daily resolution) and spatially (along the soil profile) throughout the entire study period.

For this reason, we adopted a compartment-based validation approach. The model was calibrated using observations from the soil compartment and subsequently validated against observations from the soil-plant compartment related to outgoing fluxes toward the atmosphere, leveraging actual evapotranspiration measurements, derived from the eddy-covariance technique, and the xylem water isotopic composition. As shown in the Results, the model reproduces actual evapotranspiration and the xylem water isotopic composition satisfactorily, despite not being calibrated directly against variables related to outgoing fluxes toward the atmosphere. This consistency reduces the likelihood that good performance is achieved for the wrong reasons and supports a good description of the processes occurring in the soil-plant-atmosphere continuum under study. Furthermore, the calibrated parameters were evaluated against ranges reported in the scientific literature to ensure their physical plausibility. Where possible (e.g., for θ_r and θ_s), the estimated values were also directly compared with field measurements, providing additional validation and strengthening confidence in the robustness of the parameterization.

Moreover, the analyzed hydrological years exhibit substantial interannual variability (e.g., reduced snowmelt from 2017–2018 to 2021–2022 hydrologic years, see Fig. 1 of the revised manuscript). Calibrating the model on a limited temporal subset (e.g., 2017–2019) and validating it on a subsequent period would likely bias parameter estimation toward the calibration years and could reduce robustness under contrasting hydroclimatic conditions. In this sense, adopting a traditional split-sample approach might have strengthened the performance during calibration years while weakening the performance during validation years, thus potentially resulting in lower confidence in process-based interpretations during the validation period compared to the calibration period. By calibrating the model across multiple years with highly variable conditions, we aimed to obtain a parameterization representative of a very broad range of system behavior.

We agree that this modeling strategy differs from a predictive validation framework and needs further justifications along the manuscript. In the revised manuscript, we clarified this rationale more explicitly

within the new section 3.3 *Model performance and Compartment-based calibration/validation approach* on model performance, as suggested by the reviewer.

2. Report on model performance consistently for all evaluated variables in one or more standalone tables. See specific comments.

We thank Anonymous Referee #1 for this comment. In line with the reviewer's suggestion, model performances are reported consistently for all evaluated variables in a dedicated standalone table (Table 5 of the revised manuscript) within the new section (Section 3.3) on model performance. Please, refer to the specific comments to further details.

3. Enhance visualization. See specific comments.

We thank Anonymous Referee #1 for this comment. Further details of the changes we implemented are provided in our responses to the specific comments.

4. Make tables and figures standalone. See specific comments.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer's suggestion, all tables and figures were revised to ensure that they are fully standalone. This includes providing complete definitions of symbols and abbreviations and expanding captions where necessary. Further details on the specific modifications are provided in our responses to the corresponding specific comments.

1.5 Conclusion

The conclusion is concise and can be improved with the following revisions:

1. Add sentences on limitations and uncertainty and how these may influence inference on seasonal partitioning.

We thank Anonymous Referee #1 for this valuable suggestion. In response, the Conclusions section was revised to explicitly acknowledge the main sources of uncertainty and their potential influence on the interpretation of seasonal partitioning. We added the following paragraph:

L684-693: "While the adopted modelling framework enabled a process-based interpretation of seasonal compartmentalization of water fluxes, some limitations should be acknowledged. The degree-day model used in this work provides a simplified representation of snowmelt processes: compaction, refreezing, stratification, and snow sublimation are not explicitly included. These simplifying assumptions propagate into the estimation of both

195 *the timing and magnitude of meltwater infiltration. In addition, the adopted snow isotope model does not account for the effects of refreezing or sublimation processes on the isotopic composition of meltwater. Addressing these processes would require dedicated efforts in model development. Further sources of uncertainty arise from the relatively low temporal resolution of isotope sampling, which is largely constrained by the intrinsic challenges of complex high-elevation mountain environments, as well as from potential biases associated with the cryogenic vacuum distillation, both of which may affect the estimation of longitudinal dispersivity and the representation of subsurface transport processes.*

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2. End with a brief statement on implications under changing snow regimes and one concrete recommendation for future work (or improvements).

205 We thank Anonymous Referee #1 for this comment. The Conclusions were expanded to include a brief discussion of the implications of our findings under projected changes in snow regimes. We modified the last two paragraph as follows:

L698-700: *“Given projected changes in snow regimes under climate warming, including an increasing frequency of snow droughts, the variability in the degree of ecohydrological separation observed in this study has important implications for anticipating future shifts in ecohydrological functioning within similar mountain ecosystems worldwide.*

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L701-703: *“In this regard, incorporating climate change projections into the modelling framework of this high-elevation grassland would be crucial to provide new insights into future shifts in the relative contribution of winter- versus summer-derived water to transpiration and groundwater recharge.”*

2 Specific comments

215 L39: “in the subsurface” is vague and generic, use more specific term.

We thank Anonymous Referee #1 for this comment. We agree that the term “subsurface” can be vague. We have clarified its meaning in the text by explicitly stating that it refers to both the vadose zone and the phreatic zone that should be consistent with the framework discussed by Kirchner et al. (2023) where the term “subsurface” has been used.

220 L29-40 of the old manuscript: “Ecohydrological processes occurring in the subsurface have been often conceptualized relying on physical intuition (Kirchner et al., 2023)”

Have been replaced with:

L44-45: *“Ecohydrological processes occurring in the subsurface, encompassing both the vadose zone and the phreatic zone, have been often conceptualized relying on physical intuition (Kirchner et al., 2023)”*

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L46: “available to roots”

We thank Anonymous Referee #1 for this suggestion. Accordingly, we replaced the expression L46 of the old manuscript: “accessed by roots” with L51: “*available to roots*”.

230 L70 – 84 & 85 – 96: These paragraphs are presenting previous studies, individually. However, the paragraphs need to have a concluding sentence at the end. Please revise the paragraphs.

At the end of the paragraph (L70-84 of the old manuscript) we inserted the following sentence:

235 L89-92: “*Overall, these studies highlight that plant water sources and the degree of ecohydrological separation can vary substantially across sites under different climatic and biological conditions. Nevertheless, additional unmodelled factors, including seasonality, may play a key role in modulating ecohydrological separation.*”

At the end of the paragraph (L85 – 96 of the old manuscript) we inserted the following sentence:

240 L105-107: “*Overall, these findings highlight that also the seasonal origin of plant water is highly variable across ecosystems and remains strongly dependent on site-specific climatic, hydrological, and biological conditions.*”

L97: Use the accurate word for evapotranspiration. The use of evapotranspiration can be misleading for the readers without specifying the type. The authors abbreviated evapotranspiration as AET which can be considered actual evapotranspiration by the readers. If this is true, revise the text as follows: “actual evapotranspiration (AET)”.

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We thank Anonymous Referee #1 for this comment and agree that the term “evapotranspiration” should be specified more clearly. Accordingly, we defined the term as “actual evapotranspiration (AET)” at its first occurrence in the manuscript. Thereafter, the abbreviation AET has been used consistently throughout the text whenever specifically referring to actual evapotranspiration.

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L105 – 131: revise these two paragraphs and make an objective paragraph based on what is proposed in the main comments. Also, these two paragraphs include sentences that belong to methodology and need to be removed from the introduction.

255 Following the guidance provided by Anonymous Referee #1 in the main comments on the Introduction, below we report the original sentences from the manuscript, followed by our proposed revised versions, in order to facilitate the review process.

L105-131 of the old manuscript: “In this paper, we aim to determine whether, in regions with pronounced seasonality of water inputs (e.g., mountain regions), the nature of ecohydrological separation may be influenced by the interaction

260 between the timing and magnitude of snowmelt and rainfall events, as well as by the temporal dynamics of root water uptake. Water inputs such as snowmelt and rainfall differ in intensity and frequency, which directly affect soil hydraulic conductivity and, consequently, soil water dynamics. These variations can influence the spatial and temporal availability of water to plants, potentially shaping the degree of separation between mobile and bound water pools. With the aim of pursuing this objective, multidisciplinary approaches, such as the use of conservative tracers and
265 integrated numerical models, have proven valuable, although data scarcity and the inherent complexity of high-elevation sites remain substantial challenges (Gisolo et al., 2025; van Tiel et al., 2024). Among the available models, HYDRUS-1D (Šimůnek et al., 2018) is a widely used and robust software package for simulating water flow and solute transport in variably saturated soils. It incorporates key physical processes (including infiltration, evapotranspiration, root water uptake, and advection–dispersion) offering a comprehensive representation of water and solute dynamics
270 within the soil-plant-atmosphere continuum. Furthermore, HYDRUS-1D enables the simulation of stable water isotope transport in the vadose zone (Nasta et al., 2023; Stumpp et al., 2012).

We follow an approach that integrates meteorological data with field measurements of stable isotopes in precipitation, soil water, plant water, and spring water of a high-elevation mountain grassland. Starting from these data, we employ a modelling framework that integrates a recently introduced snow isotope model and the modified version of
275 HYDRUS-1D (Stumpp et al., 2012) to simulate isotope transport within the soil-plant-atmosphere continuum of this high-elevation ecosystem. Through isotopic composition modeling, we derive the SOI which enables a robust quantification of the seasonal origin of water fluxes (focusing on transpiration and deep drainage fluxes) across multiple years. The study period encompasses years with contrasting hydrological conditions, ranging from very wet to extremely dry. Through the model output, we gain new insights into the hydrological processes shaping the degree
280 of separation between tightly bound soil water and mobile water draining through the soil profile. The integration of isotope measurements with numerical modeling to investigate alpine grassland ecohydrology remains limited, and, according to our knowledge, no study to date has explicitly tested how seasonal variations in water input, typical of high-elevation environments, interact with root water uptake to assess the degree to which the TWW hypothesis holds. Accordingly, we test the following null dichotomous hypothesis (H_0): “Winter precipitation (i.e., snowmelt) rapidly
285 transits the soil profile recharging groundwater and streams, while summer precipitation (i.e., rainfall) remains available to sustain transpiration fluxes”.

Have been replaced with:

290 L117-132: “Thus, understanding how seasonal water inputs and temporal dynamics of root water uptake shape the degree of ecohydrological separation between mobile and bound water pools remains a key challenge in ecohydrology, particularly in high-elevation environments where data scarcity and inherent complexity remain substantial challenges (Gisolo et al., 2025; van Tiel et al., 2024). Accordingly, the main aims of this paper are to:

- i) *investigate the seasonal origin of transpiration and deep drainage fluxes across multiple years with contrasting hydrological conditions ranging from very wet to extremely dry*
- ii) *clarify how contrasting seasonal water inputs, typical of high-elevation environments, interact with root water uptake to assess the degree to which the TWW hypothesis holds. Accordingly, we test the following null hypothesis (H_0): “Winter precipitation (i.e., snowmelt) rapidly transits the soil profile recharging groundwater and streams, while summer precipitation (i.e., rainfall) remains available to sustain transpiration fluxes”.*

To this end, meteorological data, soil-related data and field measurements of stable isotopes in precipitation, soil water, plant water, and spring water of a high-elevation mountain grassland have been used. Starting from these data, a modelling framework that integrates a recently introduced snow isotope model and the modified version of HYDRUS-1D (Nasta et al., 2023; Stumpp et al., 2012) has been employed, thus enabling a process-based assessment of seasonal water flux partitioning and providing new insights into alpine grassland ecohydrology by partially disentangling the processes underlying ecohydrological separation.”

L144: define hydrological year in the body of the manuscript instead of figure 1 caption.

We thank Anonymous Referee #1 for this comment. The definition of the hydrological year has now been included in the main body of the manuscript and, accordingly, removed from the caption and x-axis-label of Figure 1. See L144-145 of the revised manuscript

L156: AET is already defined in the introduction section. No need to redefine it.

Thank you. As indicated in a previous comment, we have defined the term as “actual evapotranspiration (AET)” at its first occurrence in the manuscript. Thereafter, the abbreviation AET has been used consistently throughout the text whenever specifically referring to actual evapotranspiration.

L168: refer to the corresponding figure.

We thank Anonymous Referee #1 for this comment. A reference to the corresponding figure was added at L166 in the revised manuscript.

Table 1: Soil sand, silt, and clay content (%) Is not 100% in aggregate!

We thank Anonymous Referee #1 for pointing out the inconsistency in Table 1. In the first version of the manuscript, soil texture was reported to provide a general pedological characterization of the study site. The values were derived from the Regional Soil Map of the Aosta Valley 1:100000 (https://geoportale.regione.vda.it/ricerche-nuove/scheda-nuova/?id_tipo=3&uuid=r_vda:04296-META:20220121:091600), from which we report an excerpt corresponding to the area of interest (Figure 1 of this document).

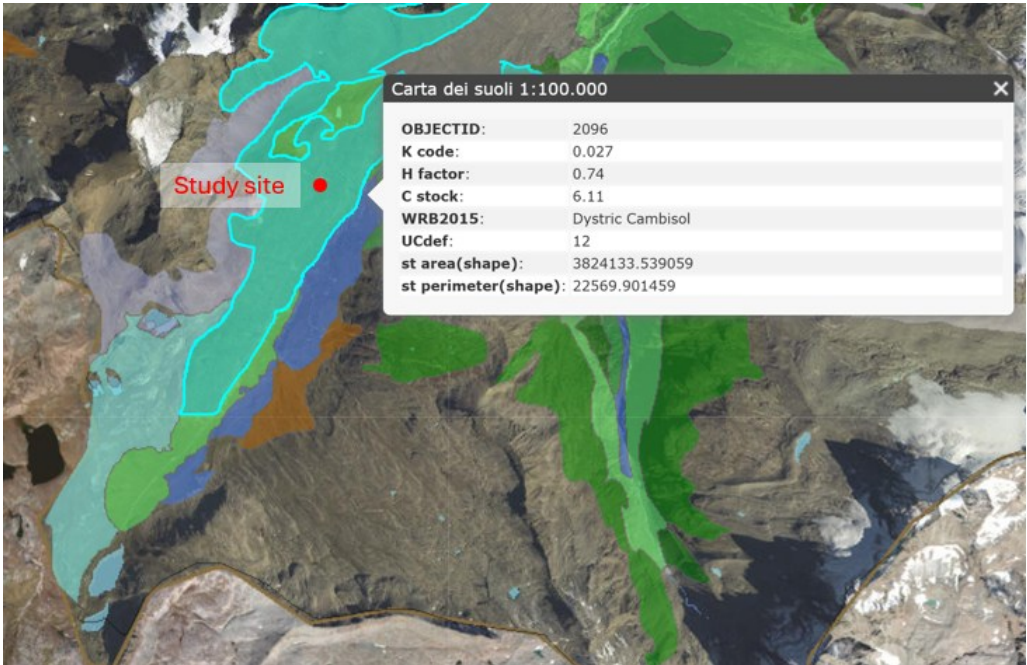


Figure 1 Excerpt of the Regional Soil Map of the Aosta Valley 1:100000 corresponding to the area of interest

Details on soil texture associated with this map are reported in “Carta dei Suoli della Valle d’Aosta – Note illustrative”(https://mappe.regione.vda.it/INVA/Condizioni/CartaDeiSuoli/Note%20Illustrative%20Carta%20dei%20suoli%202020_02_13.pdf). The reported texture corresponds to the average percentages of sand, silt, and clay calculated across all soil profiles attributed to that specific soil type at the regional scale (i.e., over the Aosta Valley Region). We acknowledge that we have not noticed that the reported percentages of sand, silt, and clay do not sum to 100%. Accordingly, it emerged that the regional dataset appears to contain inconsistencies in the reported soil texture values. We therefore conducted additional and time-consuming checks, which required direct contact with colleagues from the Department of Agricultural, Forest and Food Sciences (DISAFA), Agricultural Chemistry and Soil Science Unit, University of Turin, in order to retrieve and verify site-specific soil data. The time needed to identify and confirm these

measurements contributed to the delay in our response. We considered it important to clarify this point, particularly in light of the short time frame within which the reviewer completed the manuscript evaluation.

Notably, one of these soil profiles was described on 20 September 2017 in close proximity to the Eddy-Covariance monitoring station within the study area.

Given that this point-based measurement provides a more representative characterization of the local soil texture than the regional averaged values reported in the Geoportal of the Aosta Valley Region, we have revised the manuscript accordingly and now report the site-specific soil texture data (see L184–186 and Table 1 of the revised manuscript, also reported hereafter).

Site-specific sand, silt, and clay percentages measured near the Eddy-Covariance station

Sand (%)	Silt (%)	Clay (%)	Soil Texture
84.1	14.7	1.2	Loamy Sand

It is important to emphasize that soil type and soil texture data were included solely to provide a general description of the study site. Therefore, replacing the regional averaged values with site-specific measurements does not affect the results.

L185: mention the accuracy of soil moisture and matric potential sensors.

We thank Anonymous Referee #1 for this comment. In response, we included the accuracy specifications of the sensors. Specifically, the soil moisture sensors have an accuracy of $\pm 0.05 \text{ cm}^3 \text{ cm}^{-3}$, while the matric potential sensors have an accuracy of $\pm 10\%$ of the reading + 2 kPa. See L183-184 in the revised manuscript.

L210 – 215: Use passive sentences.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, the text was revised to consistently adopt passive constructions in the indicated paragraph. The revised version is provided below for clarity.

L214-219: *“Using monthly mean air temperature, vapor pressure, and precipitation $\delta^{18}\text{O}$ as inputs, the monthly evaporation line slopes are computed. These slopes are then used to reproject the isotopic composition of fractionated soil and plant water samples back to the Local Meteoric Water Line (LMWL). Technical details of the Craig and Gordon (1965) model are provided in Benettin et al. (2018), together with the MATLAB implementation adopted in this study. To*

determine whether a water sample is affected by fractionation, the line-conditioned excess* (lc-excess*) is calculated, accounting for uncertainty in the isotopic analysis (Landwehr and Coplen, 2006)”:

Table 2: tables should be standalone, I suggest revising the table and defining each parameter within the table. Also, clearly mention that they are optimized or not in the caption.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, Table 2 was revised to ensure that it is fully standalone. Each parameter was explicitly defined within the table (including a brief description), and the caption was expanded to clearly indicate whether the listed parameters were optimized or not. Please, see the revised version of Table 2 hereafter.

Parameters used in the snow accumulation and melt model. The listed parameters were selected based on values reported in the scientific literature (Ceperley et al., 2020; Jarvis, 1994; Schaepli et al., 2014)

Parameter	Description	Value
$T_S(^{\circ}\text{C})$	Air temperature threshold below which precipitation is assumed to fall as snow	-2
$T_R(^{\circ}\text{C})$	Air temperature threshold above which precipitation is assumed to fall as rain	2
$T_0(^{\circ}\text{C})$	Air temperature threshold for the onset of snowmelt	0
$\zeta(\text{mm } ^{\circ}\text{C}^{-1} \text{ d}^{-1})$	Degree-day melt factor controlling snowmelt rate	4.3

L295: the authors defined ET_0 as potential evapotranspiration while they defined it as ET_P in Figure 3d. Please revise the text to ET_P or PET.

We thank Anonymous Referee #1 for pointing this out and apologize for the inconsistency. The term ET_0 was replaced with ET_P throughout the manuscript to ensure consistency with Figure 3d and to avoid any potential confusion.

L320-321: move to methodology.

L358-362: move to methodology.

We thank Anonymous Referee #1 for these comments. In accordance with the reviewer’s suggestion, the indicated lines (L320–321 and L358–362 of the old manuscript) were moved to Section 2.7, “HYDRUS-1D: main equations and model set up,” to ensure that all methodological details are presented consistently within the body of the manuscript instead of in Figures caption. See L330-333 and L364-368.

L374: use passive sentences.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, the indicated sentences were revised to adopt passive constructions. The revised version is provided below for clarity.

L374-377 of the old manuscript: “In this study, we calculate the SOI by using the isotopic compositions (δ_x) of water fluxes simulated with HYDRUS-1D. We adopt the SOI because, as noted by Allen et al. (2019a), it is specifically designed to assess whether winter or summer precipitation is overrepresented in the considered flux.”

Have been replaced with:

L384-387: “*In this study, the SOI is calculated using the isotopic compositions (δ_x) of water fluxes simulated with HYDRUS-1D. The SOI is adopted because, as noted by Allen et al. (2019), it is specifically designed to assess whether winter or summer precipitation is overrepresented in the considered flux.*”

L429: 3. Ensure consistent terminology for correlation metrics throughout the manuscript (e.g., avoid mixing Spearman’s rank correlation with Pearson notation).

We thank Anonymous Referee #1 for this comment and apologize for the inconsistency in the symbolic notation used for the correlation coefficient. The correlation analysis was performed using Pearson's Linear Correlation Coefficient, and the manuscript was revised to ensure consistent terminology and notation throughout, with Pearson's Linear Correlation Coefficient correctly denoted as r .

Table 3: Add the 95% confidence intervals since the results are achieved through inverse solution approach. Also, I suggest defining parameters in the caption or table to have a standalone table.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, the 95% confidence intervals were added to Table 3, as the parameter estimates were obtained through an inverse solution approach. In addition, to ensure that the table is fully standalone, we included an additional column entitled “Description,” in which each parameter is clearly defined. Please, see the revised version of Table 3 hereafter.

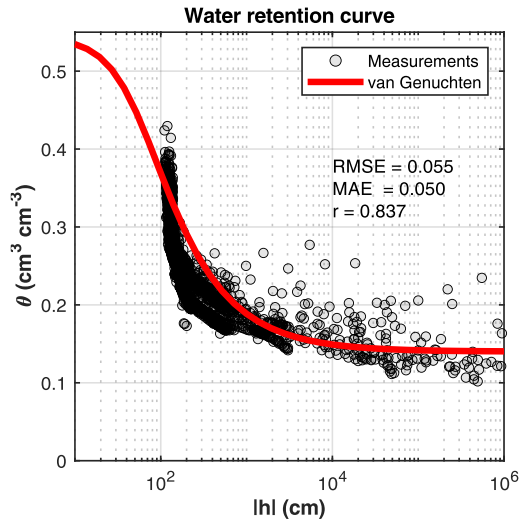
Optimized soil hydraulic and transport parameters

Parameter	Description	Value	95% Confidence limits	
			Lower	Upper
K_S (cm d ⁻¹)	Saturated hydraulic conductivity	277.93	268.02	287.84

α (cm ⁻¹)	shape parameter in the soil water retention function	0.018	0.017	0.019
n (-)	shape parameter in the soil water retention function	1.73	1.697	1.761
θ_s (cm ³ cm ⁻³)	Saturated water content	0.54	0.532	0.555
θ_r (cm ³ cm ⁻³)	Residual water content	0.14	0.138	0.143
λ_L (cm)	longitudinal dispersivity	15.44	14.28	16.61

435 Figure 5: showing a fitted VG model based on measurements without any performance metric does not have any value. So, either add performance metric(s) in the figure 5 or table 3. Also, define θ and h in the figure axes or caption.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, performance metrics were included directly in Figure 5 to quantitatively assess the agreement between measured and fitted van
440 Genuchten curves. In addition, the definitions of θ and h was clarified in the figure caption. Please, see the revised version of Figure 5 hereafter.

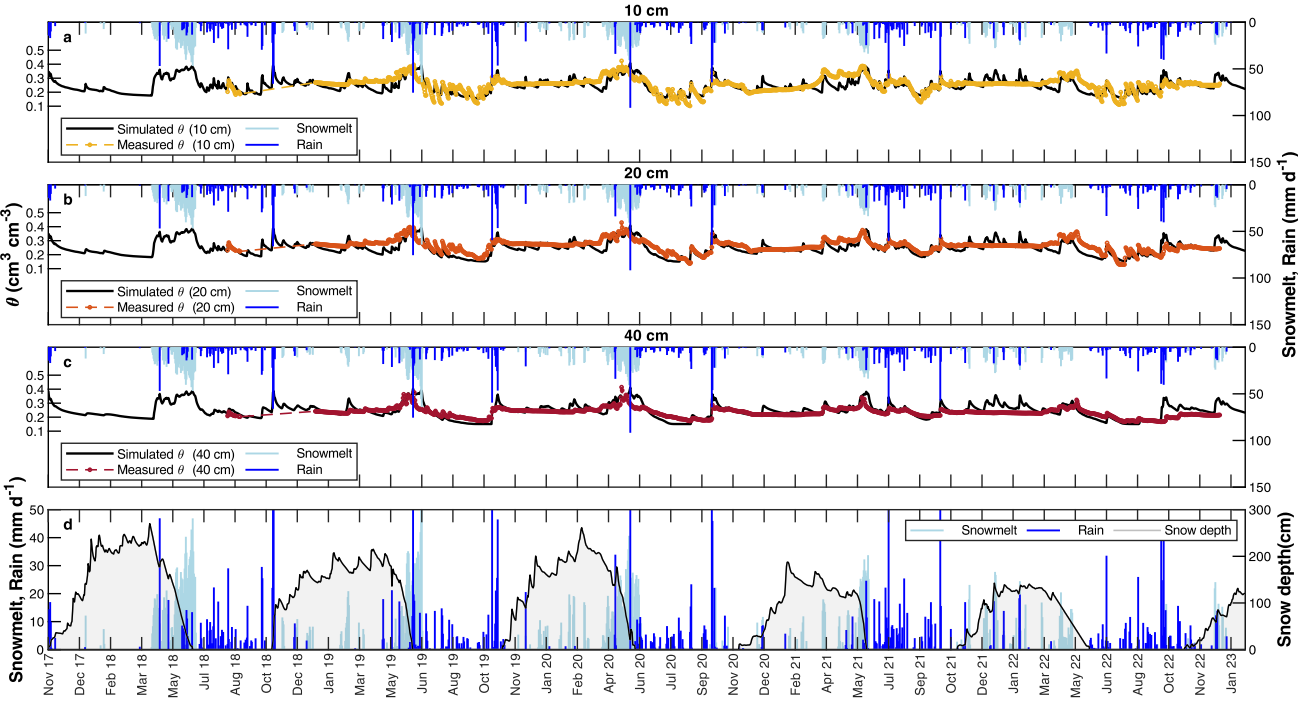


Volumetric water content (θ) as a function of pressure head ($|h|$). Points represent measurements, while the solid red line denotes the optimized van Genuchten water retention function. Performance metrics (RMSE, MAE, and Pearson’s correlation coefficient, r) are reported.
445

Figure 7: I suggest using “Sim” than “Mod” since it is widely used in the literature. Also, define the abbreviations in the caption. Moreover, the measurement lines interfere with the simulated lines and they are too congested, please try to use contrasting colors, and size adjustments in order to distinctly visualize the simulation and observation. I suggest using the full words for “Sim” and “Meas” as there is enough space in the graphs.

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We thank Anonymous Referee #1 for these suggestions. In accordance with the reviewer’s comments, the term “Mod” was replaced with “Simulated” in Figure 7, and all abbreviations were clearly defined in the revised figure caption (L527-528). In addition, the labels “Meas” were replaced with their full terms (“Measured”) to improve clarity. Furthermore, the visualization was revised to enhance readability by adopting contrasting colors and adjusting line styles and widths to clearly distinguish simulated and measured data. Below we provide the revised version of Figure 7:



Measured (line with markers) and simulated (line) volumetric water content at (a) 10 cm, (b) 20 cm and (c) 40 cm. Snowmelt and rain have been also indicated. (d) Snow depth, snowmelt and rain. θ indicates the volumetric water content.

L428-431 & 466-469: It is better to report the simulation performance in table(s) rather than within the body of the manuscript which makes it harder to find for the readers.

We thank Anonymous Referee #1 for this comment. In accordance with the reviewer’s suggestion, the simulation performance metrics reported in L428–431 and L466–469 of the old manuscript were moved from the main text to dedicated table in the new Model performance section (see Table 5 in Section 3.3 *Model performance and Compartment-based calibration/validation approach* and also reported hereafter).

Model performance metrics for all evaluated variables. The adopted performance metrics are Pearson’s linear correlation coefficient (r), the mean absolute error (MAE) and the Root Mean Square Error (RMSE).

	Variable	Depth (cm)	Compartment	Description	r	MAE	RMSE
CAL	θ (cm ³ cm ⁻³)	10	Soil	Volumetric water content measured at 10 cm	0.700	0.031	0.040
	θ (cm ³ cm ⁻³)	20	Soil	Volumetric water content measured at 20 cm	0.720	0.031	0.039
	θ (cm ³ cm ⁻³)	40	Soil	Volumetric water content measured at 40 cm	0.723	0.028	0.037
	$\delta^{18}\text{O}$ (‰)	10	Soil	Isotopic composition measured at 10 cm	0.871	1.550	1.820
	$\delta^{18}\text{O}$ (‰)	20	Soil	Isotopic composition measured at 20 cm	0.359	3.600	4.230
	$\delta^{18}\text{O}$ (‰)	40	Soil	Isotopic composition measured at 40 cm	0.328	3.700	4.090
VAL	$\delta^{18}\text{O}$ (‰)	-	Plant	Isotopic composition of xylem water	0.652	1.850	2.350
	AET (mm d ⁻¹)	-	Soil-Plant	Actual evapotranspiration measured with Eddy-covariance technique	0.610	0.620	0.880

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490 **Answers to Dr. Judith Eeckman comments on the paper “Assessing
the seasonal compartmentalization of water fluxes in the soil-plant-
atmosphere continuum of a high-elevation mountain grassland”**

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This work investigates the highly relevant question of the seasonal dynamics of water transfers in shallow soils (<60 cm depth) in an alpine grassland context. In particular, this work aims to distinguish between the seasonality of water available for vegetation and the water that percolates vertically to recharge groundwater. Isotopic transfers are modeled using the HYDRUS-1D model. Meteorological data are measured locally, and samples for isotopic analyses are collected monthly.

510 I mainly have a few substantive comments on this work, which is overall comprehensive.

We thank Dr. Judith Eeckman for the assessment of our work. We appreciate the recognition of the relevance of the research question and the overall comprehensiveness of the study. All the comments and feedback received are constructive and helped to improve the manuscript. Accordingly, we thoroughly revised the paper to address all main and specific comments. Below, we provide our responses to the comments of Dr. Judith Eeckman, together with our revisions.

515 We hope that the revisions satisfactorily address the Dr. Judith Eeckman comments and contribute to improving the quality and clarity of the manuscript, ultimately allowing it to meet the requirements for publication in *Hydrology and Earth System Sciences*.

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3 Main comments

The use of snow depth measurements to estimate melt flux is very simplistic. Indeed, this does not account for processes such as compaction, refreezing, stratification, etc. Snow sublimation is not mentioned. The degree-day model is simplistic and poorly represents these fluxes. Is sublimation accounted for in the calculation of AET in winter? There are instrumental methods for directly calculating melt flux (see Eeckman et al., 2025).

This point could be specified in the “dataset” paragraph or in the discussion.

We thank Dr. Judith Eeckman for this constructive comment and for pointing out the relevant reference.

We agree that the degree-day approach represents a simplified description of snowmelt processes and does not explicitly account for mechanisms such as compaction, refreezing, stratification, or snow sublimation. Regarding sublimation, we would like to clarify that, in terms of the objectives of this study, distinguishing the partitioning of potential evaporation into sublimation and soil evaporation is not a key aspect. Our main interest is to adequately represent the outgoing flux (evaporation + transpiration) toward the atmosphere. This is also because we compare the AET obtained from the model with AET derived from eddy covariance measurements, which do not distinguish between sublimation, evaporation and transpiration. Given how HYDRUS-1D operates, we simply partition potential evapotranspiration into potential evaporation and potential transpiration using Eqs. 2.7 and 2.8. Clearly, during winter, potential evaporation mainly corresponds to sublimation. Moreover, explicitly accounting for the isotopic composition of sublimation would require additional model developments and dedicated investigations, as this process remains subject to ongoing research (Beria et al., 2018).

In line with the reviewer’s suggestion, we explicitly highlighted in the Discussion that one of the main limitations of our approach is the use of a simplified model to estimate the melt flux. We also acknowledged that this flux could be directly calculated by using instrumental methods as reported in Eeckman et al. (2025).

At the same time, we would like to emphasize that the degree-day model is coupled with an isotope-enabled framework that simulates the isotopic composition of equivalent precipitation (i.e., rainfall + snowmelt) following the approach proposed by Ceperley et al. (2020). To the best of our knowledge, this represents one of the most recent and suitable approaches for simulating isotopic content in snow-dominated sites.

Notably, the method developed by Ceperley et al. (2020), despite the simplified assumptions, has been successfully applied in the Vallon de Nant catchment (i.e., the same site investigated in Eeckman et al., 2025), where it showed agreement with results of Eeckman et al., (2025) as also reported by the authors in the manuscript. Moreover, our results indicate that the degree-day approach provides overall reliable estimates of snowmelt dynamics at the Alpine study site where the melt is largely driven by latent heat transfers (Ceperley et al., 2020; Ohmura, 2001).

555 We added the following paragraph in section 3.4 L501-515: “As in our study, also Stumpp et al. (2012), by assessing the effects of land cover and fertilization on water flow and solute transport of five lysimeters using HYDRUS-1D, found the main discrepancies between simulated and measured values because of the uncertainties related to infiltration during snowmelt. Therefore, for future studies, a more accurate estimation of these fluxes could be achieved by directly using instrumental methods, as reported in Eeckman et al. (2025).

560 Overall, the degree-day approach used in this work has the limitation of providing a simplified representation of snowmelt processes, in which processes such as compaction, refreezing, stratification, and snow sublimation are not explicitly included. Regarding the latter, a further partitioning of potential evaporation into soil evaporation and snow sublimation is beyond the scope of the present study. The main interest here is to adequately represent the outgoing flux toward the atmosphere. Moreover, explicitly accounting for the isotopic composition of sublimation would require additional model
565 developments, as this process remains subject to ongoing research (Beria et al., 2018). Therefore, no further distinction is made between soil evaporation and snow sublimation, as both processes are included within the potential evaporation term computed using Eq. 2.7. During winter, this term predominantly reflects sublimation due to the presence of snow cover. Accordingly, we compared the simulated AET with eddy-covariance measurements, which do not distinguish between sublimation, evaporation, and transpiration.”

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We also added at the end of the paragraph 3.4:

L524-526: “Despite these limitations, our results indicate that the degree-day approach provides overall reliable estimates of snowmelt dynamics at the Alpine study site where the melt is largely driven by latent heat transfers (Ceperley et al.,
575 2020; Ohmura, 2001)”

We added the following sentence at the end of section 2.6 “Potential evapotranspiration (ET_p), evaporation (E_p) and transpiration (T_p)”:

L317-320: “No further partitioning of potential evaporation into soil evaporation and snow sublimation is made: this
580 partitioning is beyond the scope of the present study. During winter, this term predominantly reflects sublimation due to the presence of snow cover at the study site”

The issue of rain-on-snow is not addressed: how does the degree-day model respond in this case? There could perhaps be specific thresholds for such conditions. Have you observed this situation in your meteorological data? This is a very important
585 point for estimating melt flux, especially in spring.

We thank Dr. Judith Eeckman for this insightful comment. We acknowledge that, from a methodological perspective, the approach proposed by Ceperley et al. (2020) and adopted in this study does not explicitly account for rain-on-snow

events. We therefore further emphasized in the revised manuscript that the lack of an explicit representation of rain-on-snow dynamics constitutes a limitation of the adopted modelling approach that weakens the representation of meltwater generation during such events.

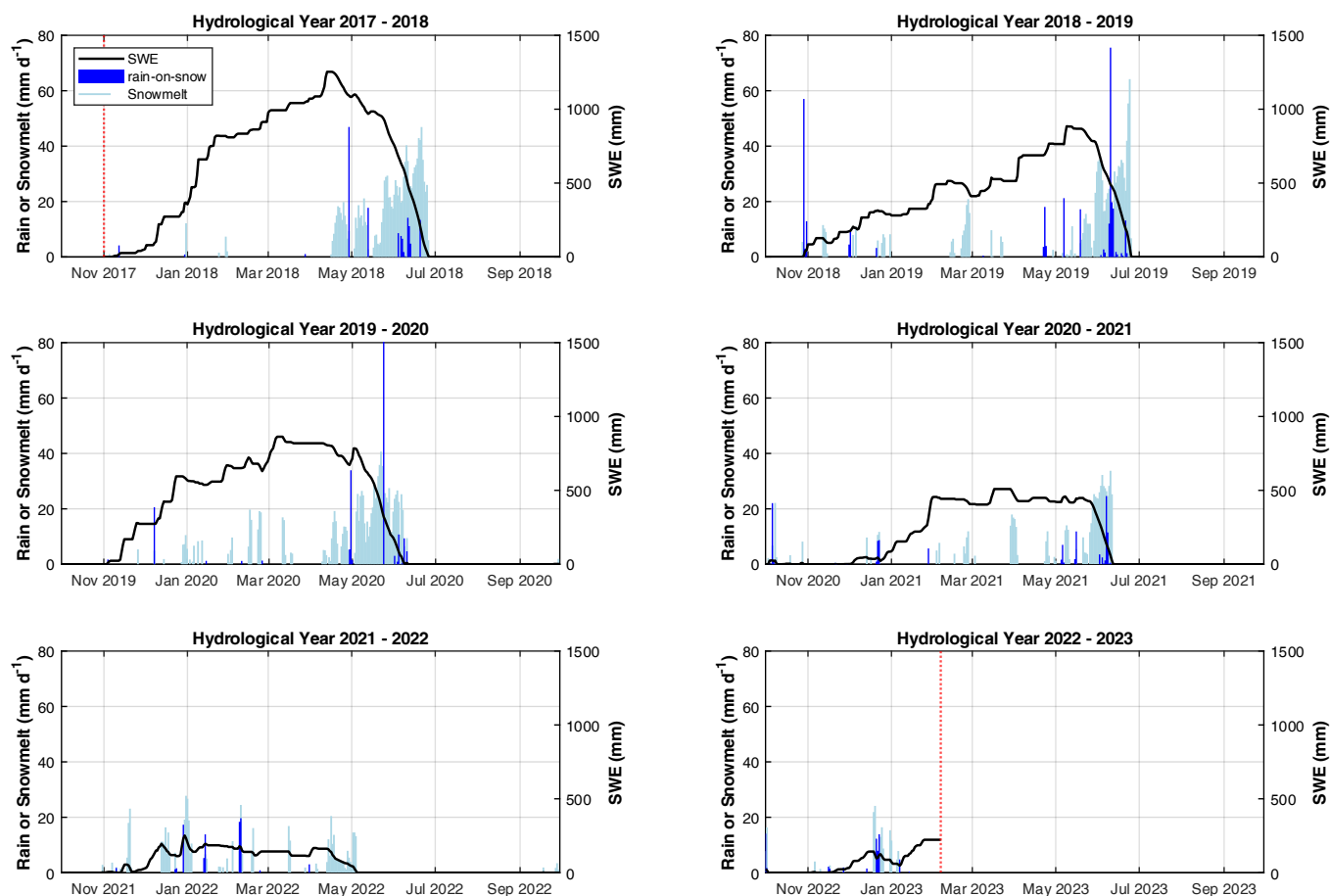
We would like to point out, however, that this aspect has been addressed in the manuscript, although it may not have been discussed in sufficient detail. Specifically, at L456 of the old manuscript we note that some discrepancies between modelled and observed data may be related to the occurrence of rain-on-snow events. In addition, we quantified in aggregated manner the occurrence of such conditions compared to total rainfall by defining rain-on-snow as precipitation falling as rain while snow water equivalent (SWE) is greater than zero.

Nevertheless, following the reviewer’s comment, we further investigated this aspect to better assess the potential influence of rain-on-snow events on the results.

Over the study period, most of these events occur in June (Table 1, Figure 2 of this document), toward the end of the snowmelt season (Figure 2 of this document). During these events, the model quantifies melt fluxes (Figure 2 of this document), but no enhanced melt rates are used during rain-on-snow events. Indeed, Myers et al. (2023), highlighted that such events can increase melt rates. Accordingly, one potential implication about the use of Ceperley et al. (2020) model is that snowmelt is underestimated by the degree-day approach during rain-on-snow conditions.

Table 1 Number of rain-on-snow days (i.e., precipitation falling as rain while SWE is greater than zero) for each month. The intensity of the blue shading in each cell is proportional to the number of rain-on-snow days recorded during the corresponding month. Hatched cells indicate months that fall outside the study period.

Hydrologic Year	Oct	Nov	Dec	Jan	Feb	Mar	Apr	May	Jun	Sep	Aug	Sep
2017-2018		2	1	0	0	1	1	1	9	0	0	0
2018-2019	3	0	3	0	0	1	3	2	14	0	0	0
2019-2020	0	1	1	1	3	0	2	2	7	0	0	0
2020-2021	4	4	3	1	0	0	0	6	8	0	0	0
2021-2022	0	3	3	2	3	1	0	0	0	0	0	0
2022-2023	2	4	4	2								



610 **Figure 2** Daily snow water equivalent (SWE, right axis), rainfall occurring under rain-on-snow conditions (bars, left axis), and snowmelt (SM, left axis) derived from the Ceperley et al. (2020) model for each hydrological year. Rainfall is shown only for days when both rainfall and SWE are greater than zero (rain-on-snow conditions). Red dashed vertical lines indicate the start (1 November 2017) and end (6 February 2023) of the observation period.

In the revised version of the manuscript, we reported the previous analysis in the Supplementary Material.

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L455-460 of the old manuscript: “Notably, at the end of snowmelt periods, measured volumetric water content is often slightly higher than modelled values. This discrepancy may be explained by the impact of rain-on-snow events, which are typically more intense and short-lived than melt events driven solely by temperature (Myers et al., 2023). Indeed, within the considered elevation range, approximately 26% of the total rainfall between November 2017 and February 2023 occurred under rain-on-snow conditions, that is, precipitation falling as rain while the SWE is greater than 0 (Ceperley et al., 2020).”

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Have been replaced with:

L516-523: “Notably, at the end of snowmelt periods (June), measured volumetric water content is often slightly higher than simulated values. This discrepancy may be explained by the impact of rain-on-snow events. Indeed, most of these conditions (i.e., precipitation falling as rain while the SWE is greater than 0) occur in June (Table S1, Figure S1). During these events, the Ceperley et al. (2020) model simulates melt fluxes (Figure S1). However, the magnitude of snowmelt is likely underestimated, as rain-on-snow events are known to generate more intense and short-lived melt pulses compared to melt driven solely by temperature (Myers et al., 2023). The lack of an explicit representation of rain-on-snow processes therefore constitutes an additional limitation of the modelling approach. Nevertheless, it is worth noting that such conditions account for only 5.6% of the total study period, mainly occurring at the end of the melting season.”

How do you consider lateral transfers? In steep slope contexts, infiltration upstream can be transferred laterally at shallow depths (<60 cm) and re-emerge downslope to supply the root zone again. In these specific contexts, infiltration at the bottom of the soil column does not necessarily lead to groundwater recharge or streamflow contribution.

We thank Dr. Judith Eeckman for this pertinent comment, which allows us to better clarify the role of lateral water transfers in our study area.

- i) Although the hillslope hosting the monitoring station has an average slope of approximately 32° (Gisolo et al., 2022), the instruments (soil moisture and matric potential probes) are installed on a small, relatively flat plateau, as described in the “Study site and datasets” section. This local topographic configuration supports the assumption that water fluxes within the monitored soil profile are predominantly vertical, and thus reasonably represented by a 1D modelling approach.
- ii) Independent evidence at the catchment scale suggests that vertical processes dominate over lateral transfers. In Gentile et al. (2023), the fraction of young water (water younger than 2-3 months) in the main streamflow and spring of the study area was estimated to be very low, 0.18 and 0.11, respectively. These results are consistent with findings from mountainous catchments worldwide (Jasechko et al., 2016). These authors show that steeper catchments tend to exhibit lower fractions of young streamflow, stating that “...the reduced prevalence of young streamflow in steeper terrain suggests that steeper landscapes tend to favour deeper vertical infiltration rather than shallow lateral flow. A tendency for greater infiltration in mountainous watersheds may seem counterintuitive but is consistent with conceptual models of runoff generation and groundwater flow that suggest that topographic roughness drives long groundwater flow pathways that bypass first-order streams. Smaller young streamflow fractions in steep regions may also reflect the tendency for rock stresses in steep landscapes to fracture bedrock, enhance permeability, and promote deep infiltration”.

iii) the comparison between the isotopic composition of the modelled bottom flux and that of spring water (Fig. 11d–e of the revised manuscript) further supports that vertical processes dominate over lateral transfers, suggesting that snowmelt predominantly contributes to subsurface storage that feeds the spring, rather than being rapidly transferred laterally. This interpretation is consistent with isotope-based analyses presented in Gentile et al. (2023), and with global-scale findings (Jasechko, 2019), which show that event water typically represents only a minor fraction of streamflow, while discharge is largely dominated by pre-event water. These results suggest that equivalent precipitation (rainfall + snowmelt) primarily infiltrates into the subsurface, displacing pre-event water that is subsequently released to the stream. This appears to be the dominant process in the study area.

In general, we agree that infiltration occurring upslope could be redistributed laterally at shallow depths (<60 cm) and subsequently re-emerge downslope, contributing to the root zone. Indeed, we cannot entirely exclude the presence of such process. However, based on the combined topographic, hydrological, and isotopic evidence, deep vertical infiltration is the dominant process at the site. In the revised manuscript, we explicitly included this aspect in the Discussion section.

We added the following paragraph L604-623: *“It should be noted that, in steep slope contexts, infiltration occurring upslope could be redistributed laterally at shallow depths (<60 cm) and subsequently re-emerge downslope supplying again the root zone. In these specific contexts, infiltration at the bottom of the soil column does not necessarily lead to groundwater recharge or streamflow contribution. Accordingly, we cannot entirely exclude the presence of such process. Nevertheless, this mechanism can reasonably be considered less relevant at the study site for three main reasons.*

First, although the hillslope hosting the monitoring station has an average slope of approximately 32° (Gisolo et al., 2022), the instruments are installed on a small, relatively flat plateau. This local topographic configuration supports the assumption that water fluxes within the monitored soil profile are predominantly vertical and can thus be reasonably represented using a 1D modeling approach.

Second, independent evidence at the catchment scale suggests that vertical processes dominate over lateral transfers. In Gentile et al. (2023), the fraction of young water (i.e., water younger than 2–3 months) in both streamflow and spring water was estimated to be low (0.18 and 0.11, respectively). These values are consistent with findings from mountainous catchments worldwide (Jasechko et al., 2016), suggesting that, although seemingly counterintuitive, steeper catchments tend to favour deeper vertical infiltration rather than shallow lateral flow.

Third, the comparison between the isotopic composition of the modeled bottom flux and that of spring water (Figure 11 d–e) further supports the dominance of vertical processes over lateral transfers. This interpretation is consistent with isotope-based conceptualizations presented in Gentile et al. (2023) and with global-scale findings (Jasechko et al., 2019),

which show that event water generally represents only a minor fraction of streamflow, while discharge is largely dominated by pre-event water. These results suggest that equivalent precipitation (rainfall + snowmelt) primarily infiltrates into the subsurface, displacing pre-event water that is subsequently released to the stream. This appears to be the dominant process in the study area.”

4 Minor remarks

l. 99 “In Swiss catchment”: please provide references and/or describe which catchments.

We thank Dr. Judith Eeckman for this comment. The relevant references were added in the revised manuscript.

L99-100 of the old manuscript: “In Swiss catchments, for example, SOI in streamflow (SOI_Q) was found to be ≈ 0 , reflecting that streams are sustained by nearly equal fractions of summer and winter precipitation.”

Have been replaced with:

L110-113: “For example, in Swiss catchments having at least four years of streamwater isotope measurements previously analyzed by Seeger and Weiler (2014), von Freyberg et al. (2018) and Bovier et al., (2025), SOI in streamflow (SOI_Q) was found to be ≈ 0 (Allen et al., 2019), reflecting that streams are sustained by nearly equal fractions of summer and winter precipitation.”

Figure 1: how is the AET presented in this figure calculated?

We thank Dr. Judith Eeckman for this comment. Actual evapotranspiration (AET) shown in Figure 1 is derived from eddy-covariance measurements. This was clearly specified the Figure 1 caption of the revised manuscript.

l. 245: do you have a reference for this thermal lapse rate in the region?

We thank Dr. Judith Eeckman for this comment. The thermal lapse rate was not taken from literature but was directly estimated from observational data. Specifically, it was calculated using air temperature records from our meteorological station together with data from additional stations managed by the Functional Centre of the Aosta Valley Region, all located within the same valley (Valsavarenche Valley). Overall, the used stations cover an elevation range from 1651 to 2555 m a.s.l.. This is described in L247-252 of the section 2.4 “Snow accumulation and melt model”.

However, we added a relevant reference (see L252) regarding local site effects previously discussed in Eeckman et al. (2022).

I. 310: eq. 2.7, 2.8: please provide references for these equations.

We thank Dr. Judith Eeckman for this comment. We reported the reference in the revised manuscript.

L309 of the old manuscript: “ $ET_{P,HSM}$ is then partitioned into potential evaporation (E_P) and potential transpiration (T_P) using the following expressions:”

Have been replaced with:

L309-310: “ ET_P is then partitioned into potential evaporation (E_P) and potential transpiration (T_P) using Beer’s law that partitions the solar radiation component of the energy budget via interception by the canopy (Ritchie, 1972) as follows:”

Figure 7: the measured and simulated data are not distinguishable in the figure.

We thank Dr. Judith Eeckman for this comment which is similar to a comment reported by Anonymous Referee #1.

Accordingly, the visualization was revised to enhance readability by adopting contrasting colors and adjusting line

styles and widths to clearly distinguish simulated and measured data. Below we provide the revised version of Figure

7:

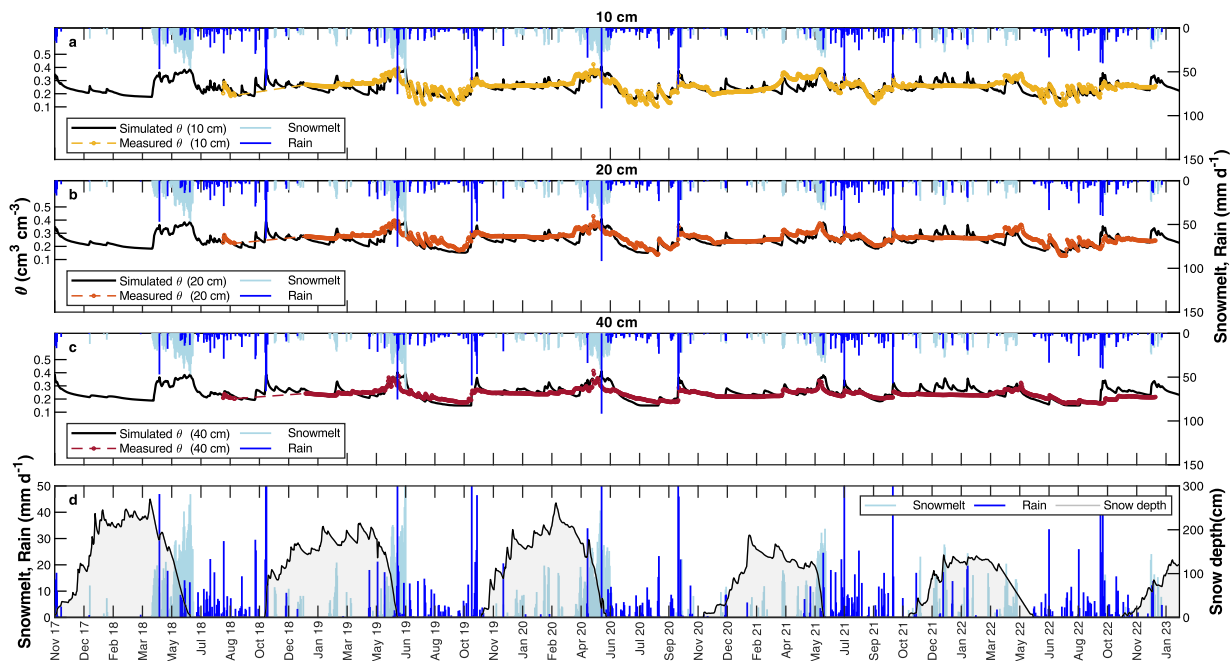


Figure 8: there are many overlapping curves. Could these be separated into multiple panels for better readability?

We thank Dr. Judith Eeckman for this comment. Below we provide the revised version of Figure 8:

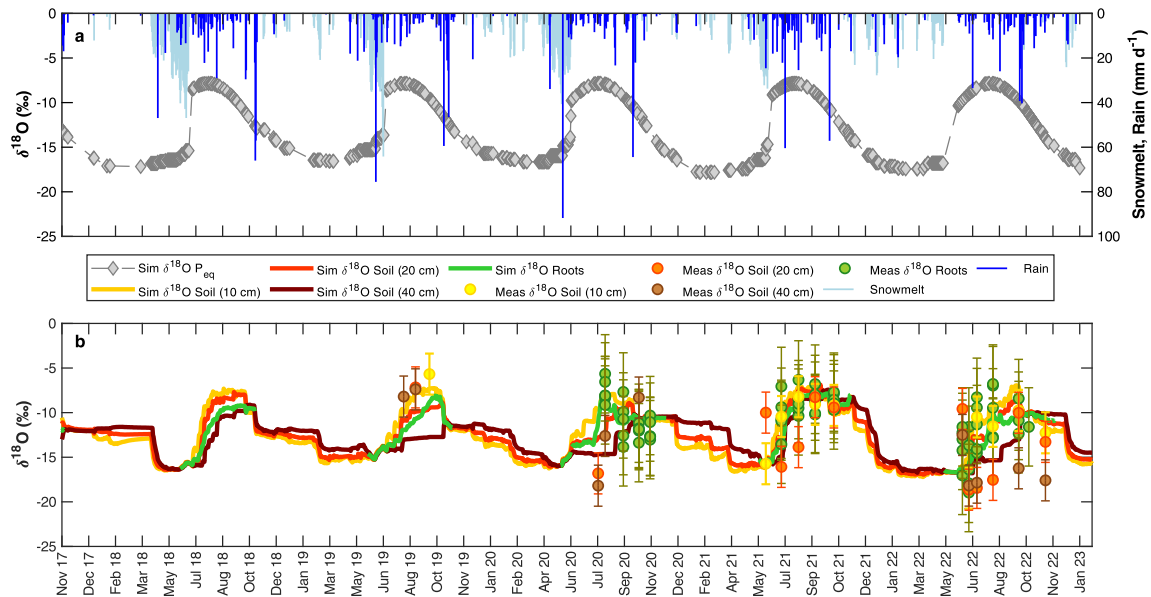


Figure 12: specify the variables simulated with HYDRUS-1D.

We thank Dr. Judith Eeckman for this comment. All the variables reported in Figure 12 have been simulated with HYDRUS-1D. We specified this at the end of Figure 12 caption in the revised manuscript.

L670: “All the variables over time and soil depth have been simulated with HYDRUS-1D.”

5 References

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