General comments:

RC1C1: This is an interesting setup touching a weak spot in hydrology and solute mobilization: While connectivity is used to explain solute export dynamics, it is hard to actually map and measure. This study can go a step in this direction by a drone-based assessment of snow cover changes during a major snowmelt event combined with concentration dynamics in the receiving stream. This topic is of great interest to readers of HESS.

While the data and effort are impressive, I am not totally convinced about the way results are described and interpreted. From my point of view the result section can be more concise and the discussion section much more integrative. I see a tendency to overinterpret patterns. My suggestion is a careful revision following the points raised below. I moreover encourage the authors to look for a measure of connectivity in the snow cover data that can be used as an explanatory variable in the event C-Q analysis. Just fraction of area covered by snow or snow depths is not telling a story of connectivity of source zones to the stream. However, measures of spatial connectivity of snow-free area changing over time may provide that.

RC1A1: We thank the reviewer for the detailed comments and feedback, and the manuscript will be carefully revised following the comments. We fully agree that hydrological connectivity is a key concept for interpreting solute export dynamics, and that simple metrics such as total snow-free area or mean snow depth alone do not adequately describe connectivity of source areas to the stream.

Motivated by this comment, we explored whether the UAS data could be used to derive a connectivity-relevant descriptor from the spatial pattern of snowmelt. While snow cover limits direct observation of surface wetness or flow paths during spring, the progressive exposure of the peat surface provides information on where potential source areas become hydrologically active. We therefore use the SAGA Wetness Index (SWI), calculated from a snow-free DEM, to characterize the potential hydrological connectivity of those areas as they become snow-free.

Specifically, we calculate the mean SWI of snow-free pixels for each UAS survey as a first-order proxy for the wetness state and potential connectivity of activated areas. We emphasize that SWI is a static, topography-based index and does not describe connectivity dynamically; rather, connectivity emerges through the interaction between static topographic controls and the temporally evolving snow cover.

Despite the limited number of surveys (five during rapid snowmelt), we find that mean SWI of snow-free areas is significantly positively correlated with event maximum discharge and hourly DOC load, and negatively correlated with the flushing index. These relationships suggest that not only the extent but also the topographic position of snow-free areas influences event-scale hydrological and biogeochemical responses. This additional analysis and a new figure will be included in the revised manuscript, with appropriate caution regarding its exploratory nature.

Specific comments:

Title

RC1C2: For me UAS is not self-explaining and I would even avoid DOC as an abbreviation in the title.

RC1A2: Abbreviations in the title will be written in full in the revised manuscript.

Abstract

RC1C3: L9: I miss a reference to the location and size of the catchment/area to help the reader understanding what dimension you are talking about.

RC1C3: Reference to the location and size of the catchment will be included in the revised version.

Introduction

RC1C4: The introduction reads very well – not much to revise from my point of view. However, I found the isotope analysis as not at all motivated in the introduction. You should mention at some point here and / or in the methods what this analysis is used for.

RC1A4: The isotopes are used as supporting data for interpretations on the flow paths and snowmelt inputs. The rationale will be included in the introduction or methods for the revised manuscript.

RC1C5: L56: “water table” or better “water levels” is enough.

RC1A5: Will be changed in the manuscript.

RC1C6: L60: These studies use the TWI at catchment scale or in riparian zones but not in peatlands. Are there also examples from peatlands pointing a dominant topographic control of discharge generation?

RC1A6: We agree that flow paths in peatlands are complex and not solely controlled by surface topography, particularly under dry conditions or when vertical gradients dominate. However, during snowmelt and other wet periods when the water table is close to or above the peat surface, lateral flow near the surface becomes increasingly important, and topographic controls on flow convergence can play a meaningful role.

In this context, we use the SAGA Wetness Index (SWI) as a first-order indicator of the potential for surface and near-surface flow accumulation rather than as a mechanistic predictor of hydrological processes within the peat profile. Similar topography-based indices have been applied in peatland settings to estimate lateral flow paths, soil moisture patterns, or contributing areas, particularly under wet conditions. Examples include applications in restored peatlands (Ikkala et al., 2022), permafrost-affected peatlands (Persson et al., 2012), and riparian and near-stream contributing areas in peatland complexes (Richardson et al., 2012).

We acknowledge that SWI does not capture key peat-specific controls such as hydraulic conductivity contrasts, macroporosity, or vertical preferential flow paths. For this reason, SWI is not used to infer flow mechanisms, but rather to describe the spatial distribution of areas that are more likely to contribute to hydrological connectivity once snow cover disappears. This clarification will be added to the revised manuscript, together with the references.

Methods

RC1C7: L109: To what is the abbreviation “FMI” pointing to?

RC1A7: FMI refers to the Finnish Meteorological Institute, which is the source of the open weather data used. This will be clarified in the revised manuscript.

RC1C8: L115: Try to be more precise here. Is this the pH in the surface water or in the soil water?

RC1A8: Both in the surface water and pore water, at least in the upper peat layers, based on our unpublished data and other studies in the site (e.g., Järvi-Laturi et al. 2025). This will be clarified in the revised manuscript.

RC1C9: Fig. 1: I find it rather unusual to use a copy of a topographic map as a background. All shown features are useless unless explained in a legend. But are all shown features necessary to know? Isolines lack numbers! Potentially use the right map to show the location of the research station/ precipitation sampler.

RC1A9: We agree that the map features can be changed to better serve the purpose. The figure will be edited in the revised manuscript by changing the background topographic map only to the most relevant features (contours and peatlands) and including the location of the research station.

RC1C10: L177: Explain GCPs.

RC1A10: GCPs refer to ground control points. This will be added in the revised manuscript.

RC1C11: Fig. S2: This relationship looks rather weak. Can you report on the R2 and bias and have you used the confidence interval later on as shown (but not described) here?

RC1A11: We agree with the reviewer that the initial calibration shown in Fig. S2 is associated with considerable uncertainty, and we appreciate the opportunity to clarify and improve this aspect of the manuscript. The online DOC sensor was initially calibrated following the manufacturer's recommendation for local site calibration, using a linear model with zero intercept. Due to the limited number of grab samples available at the time and occasional interference by particulate material during high-flow conditions, the resulting relationship showed substantial scatter, which motivated the reviewer's concern.

For the revised manuscript, we therefore recalibrated the DOC time series using an improved method with an expanded dataset that includes a longer monitoring period and a substantially larger number of grab samples. This revised calibration resulted in a marked improvement in the sensor–laboratory relationship (updated R^2 and regression statistics will be reported in Fig. S2), and reduced systematic bias across the observed concentration range.

As a result of the recalibration, the absolute range of DOC concentrations increased (revised range 4.3–11.3 mg L^{-1}, previously 4.6–6.5 mg L^{-1}), while the temporal dynamics and relative event-scale patterns remained consistent. All analyses in the revised manuscript are performed using the updated DOC dataset. Importantly, the main conclusions of the study rely primarily on relative changes in DOC concentration and DOC–discharge relationships rather than absolute concentration values. We therefore consider the interpretations to be robust despite remaining uncertainties in sensor-based concentration estimates. The revised manuscript will explicitly acknowledge calibration uncertainty and clarify how it affects, and does not affect, the interpretation.

Results

RC1C12: I find the description of snowmelt with 5 figures and 2 tables as too extensive. Information can be transferred in a more condensed way focusing on the information that is really needed in the subsequent analysis.

RC1A12: We agree that the information can be presented more concisely. This section will be revised by integrating some of the figures and tables and moving some of the figures to supplementary materials.

RC1C13: L291: What caused the rapid snowmelt? Temperature? Rainfall on snow?

RC1A13: The rapid melting of snow cover was mainly driven by air temperatures (mean 7.9°C between 12 and 18 May as stated in section 3.2, L360). However, there was a small rain event on 16 May, which might have further accelerated the melt.

RC1C14: L292: To what statistic measure “variation” is referring to? Can you be more quantitative here?

RC1A14: This refers to Figure 3, which shows the snow depth histograms for each survey. Variation refers to the distribution of snow depth in all pixels, which represents the spatial variation of snow depth. Will be clarified in the revised manuscript.

RC1C15: L331: For me it is a surprise that the SWI is dynamic. The description in the method does not point to SWI being used as a dynamic surface feature. I interpreted it as a static topography feature. So, for me this is hard to understand. Do you describe snow melt in different classes of the snow-free topography or temporally dynamic SWI of the snow surface?

RC1A15: It is correct that SWI is a static topography-based measure, and the dynamic aspect here is the changing snow cover. At L331, mean SWI refers to SWI of the snow-free areas, where the mean SWI is calculated for all pixels where snow depth has reached zero, giving an estimate of the potential wetness of melted areas. Similarly, SWI classes are calculated based on snow-free topography. We will explain this more clearly in the methods in the revised manuscript.

RC1C16: L334f: Can you state if difference were statistically significant?

RC1A16: Based on pairwise Wilcoxon test, differences in snow depth between SWI classes were statistically significant ($p < 0.01$), except for snow depth in low and medium classes on 16 May. Similarly, differences in snow depth change between SWI classes were statistically significant, except for low and medium classes on 14–15 May. This information will be added to the revised manuscript.

RC1C17: Table 3: For me it would be a better option to show the content of that table in Fig. 6 as a third panel.

RC1A17: We thank the reviewer for the good suggestion, which would also help to reduce the number of tables. Information in Table 3 will be added to Fig. 6 in the revised manuscript.

RC1C18: Chapter 3.2: The title implies a description of DOC only but the chapter contains much more.

RC1A18: Thank you for pointing this out. We agree that the current title does not fully reflect the content of the chapter. The title will be changed to "High frequency hydrological and dissolved organic carbon (DOC) time series" in the revised manuscript.

RC1C19: Fig. 7: Use TSSeq concentrations in the axis as well. Is this mg/L as a unit or rather unitless? What is the data source for snow depths here? Is this the same data as described above (UAS)? I am not sure if there is a reason to display load of DOC and TSS concentrations in the same plot. Same for WTD and water temperature.

RC1A19: The sensor gives TSS value in mg/L, but without local calibration, the values should be interpreted primarily in terms of their temporal dynamics rather than absolute concentrations. In Fig. 7, TSS is shown on a log scale for the ease of interpretation.

The snow depth shown in Fig. 7 is derived from a point snow depth measurement at the location indicated in Fig. 1. This dataset is shown instead of the UAS-derived snow depth to provide a continuous reference over the longer monitoring period. This distinction will be clarified in the figure caption.

DOC load and TSS are shown together to limit the size and number of panels, however we agree that other options could be considered and will reconsider this in the revised manuscript. WTD and (ground)water temperature are monitored in the same GW well using the same logger, and we therefore consider their joint presentation to be meaningful.

RC1C20: L355: Consider a different wording as “followed by” implies that first discharge increased and then air temperature and rainfall increased (that actually triggered the discharge?).

RC1A20: Will be changed in the revised manuscript.

RC1C21: L362-374: Concentration of DOC hardly change over time so that the discharge dynamics are the overwhelmingly dominant driver of the load, right? This stark differences in the variation of both could be mentioned here.

RC1A21: We agree that the changes in DOC concentrations are minor compared to Q and that it is discharge that drives the DOC load, and this remark will be added in the revised manuscript.

RC1C22: L398: This statement puzzles me as the relative position of DOC and Q in the plot (Fig. 8) is matter of the scaling of the two different Y-axes.

RC1A22: We thank the reviewer for pointing this out. We agree that the relative position of DOC and Q in a dual-axis plot is inherently dependent on the scaling of the two y-axes. The intention of Fig. 8 was to compare the timing and general shape of DOC and discharge responses during the event. However, we acknowledge that the current wording and figure presentation may invite misinterpretation. In the revised manuscript, we will remove or rephrase text that implies inference based on the relative position of the two curves and revise the figure to focus explicitly on temporal co-variation rather than visual comparison of magnitudes.

RC1C23: Fig. 8: Why is cumulative Q and SWE loss with the same unit referring to different Y-axes. This should be on the same axis. I have troubles understanding the SWE loss in the figure. Majority happens at the 9th May – I see that this is due to a gap in the data. However, the way to display this is not helpful. Consider to leave out the vertical line starting from 0.

RC1A23: Cumulative Q and SWE loss are shown on separate y-axes because their magnitudes differ substantially. SWE loss is estimated for the entire study area based on UAS-derived snow depth maps, whereas cumulative Q is measured at the stream gauging station, resulting in much smaller values. We agree, however, that displaying variables with the same units on separate axes can be confusing and complicates interpretation, and thus, Fig. 8 will be edited in the revised manuscript.

SWE loss is calculated based on subsequent UAS surveys, and the gap between the first survey on 1 May and the next on 14 May causes the major increase. The step direction is set to the center to represent the midpoint between the surveys, and this is why the majority of changes seem to happen on 9 May. We agree that the current visualization is not optimal. In the revised manuscript, we will revise Fig. 8 to improve clarity, including reconsidering the step representation and removing the vertical line starting from zero, as suggested.

RC1C24: L433-435: Some of the information are redundant here as anticlockwise and HI<0 is the same thing.

RC1A24: We thank the reviewer for pointing this out. We agree that describing anticlockwise hysteresis and HI < 0 is redundant, and the text will be revised to remove this redundancy in the revised manuscript.

RC1C25: Fig. 9: Use TSSeq on the axis.

RC1A25: Will be changed in the revised manuscript.

Discussion

RC1C26: I have issues with the cut between chapter 4.1, 4.2 and 4.3. For me the separation is not clear but redundancies are large. Discussion circles around the same processes that are explained by different data in the different chapters. The idea of a discussion should be more integrative and less along the steps of the result section, especially when the same processes are discussed.

RC1A26: We thank the reviewer for this comment and agree that the current separation between Sections 4.1-4.3 is not sufficiently clear and leads to redundancy. In the revised manuscript, we will reorganize the Discussion to be more explicitly process-oriented and integrative. Instead of structuring the Discussion along individual result sections, we will synthesize snow cover evolution, groundwater dynamics, isotope information, and DOC–Q relationships within a smaller number of conceptually focused subsections. This revised structure will reduce repetition and place greater emphasis on linking multiple observations to common hydrological and biogeochemical mechanisms.

RC1C27: L462: Again, I have issues to make the link of snowmelt and SWI. A high SWI marks areas in the landscape that tends to be wetter as flow paths converge (large upstream area, low slope) while low SWI values mark areas that are steeper and have smaller upstream area. How does that come together with the snow melt? In a direct causative way? Or because both are a function of topography? Steeper hillslopes do not allow for snowpack accumulation and are more exposed to radiation… So how does that link to connectivity in the landscape?

RC1A27: We thank the reviewer for raising this important conceptual point. We agree that the SAGA Wetness Index (SWI) does not causally control snow accumulation or snowmelt, and that both snow distribution and SWI are influenced by topography and associated factors such as slope, contributing area, and radiation exposure. We do not intend to imply a direct causal link between SWI and snowmelt processes. In this study, SWI is not used to explain where or why snow melts, but rather to characterize the hydrological relevance of areas once they become snow-free. Snowmelt acts as a temporal trigger that progressively activates different parts of the landscape, whereas SWI describes the potential for lateral flow convergence and hydrological connectivity in those activated areas.

Within this framework, snowmelt occurring in low-SWI areas is less likely to result in rapid stream connectivity due to limited flow convergence or greater infiltration potential, whereas snowmelt in high-SWI areas is more likely to enhance connectivity and contribute disproportionately to discharge and solute transport. The observed co-variation between snow-free area weighted by SWI and event-scale hydrological and biogeochemical responses therefore reflects an interaction between static topographic controls and the dynamic progression of snowmelt, rather than a direct control of SWI on snowmelt itself. We will revise the manuscript text to clarify this distinction and avoid any implication of causality between SWI and snowmelt

RC1C28: L478f: The ice cover is a new result brought up here. For me it does not really explain why ice is forming here especially.

RC1A28: We agree that this interpretation can be stated more clearly. The low-lying areas refer to microtopographical depressions where the water level is above the ground surface, which is easily frozen during the winter. The depressions can also capture more snow due to wind trapping, further increasing the persistence of the ice cover. This process will be clarified in the revised manuscript.

RC1C29: L503: This relative increase was not convincingly shown nor quantified in the result. So, it is a bit hard to follow that argument here.

RC1A29: We thank the reviewer for this comment and agree that the wording of this argument is ambiguous. The statement refers to changes in the C-Q dynamics rather than to concentration increases alone, specifically the lack of clear dilution as snow-free areas expanded, particularly in high-wetness zones. In the revised manuscript, we will clarify this interpretation and explicitly link it to the relevant results (e.g., C-Q behavior and hysteresis patterns) or revise the wording to avoid implying a quantified increase where this is not directly shown.

RC1C30: L509f: However, consider that the dilution effect is very small with concentration hardly changing during the event. This speaks rather for a transport and not a source limitation of DOC. So, from my observation I see very mild dilution effects only and therefore nearly every flow path loaded with DOC and therefore no major changes in sources of flowpath. This is basically a chemostatic system.

RC1A30: We thank the reviewer for this insightful interpretation and agree that the observed DOC dynamics are consistent with near-chemostatic behavior and predominantly transport-limited DOC export. DOC concentrations vary only modestly during the snowmelt events compared to the large changes in discharge, indicating that most mobilized flow paths are DOC-rich and that dilution effects are generally weak.

We agree that this behavior does not support major shifts in DOC sources or flow paths, but rather suggests that event-scale dynamics primarily reflect changes in transport efficiency and hydrological connectivity within an otherwise well-buffered system. Hysteresis direction and flushing indices therefore indicate subtle differences in timing and routing of DOC transport, rather than fundamental changes in source contributions. Despite the subtle changes, understanding these processes has great importance in understanding the DOC transport mechanisms from peatlands to downstream water bodies on a wider catchment scale.

In the revised manuscript, we will strengthen this interpretation by explicitly framing the system as near-chemostatic, reducing language that implies strong dilution or rapid source depletion, and clarifying that DOC–Q dynamics mainly reflect transport-related processes during snowmelt.

RC1C31: L538-553: For me this discussion repeats former statement but add some TSS data. I suggest to strongly reduce redundancies and combine with the discussion above.

RC1A31: We agree that the organization of the discussion section can be improved. Discussion will be revised for clearer separation for the revised manuscript.

RC1C32: L555ff: This is a long statement for a rather simple fact. Nearly invariant concentrations multiplied with highly variant discharge will result in a load that is exactly the same as the discharge.

RC1A32: We agree that the statement can be formed more concisely to express the point. Will be edited in the revised manuscript.

RC1C33: L592ff: Again, I would be careful in interpreting the mild concentration changes too much. Yes, the described processes are meaningful but I don't think we see a fundamental change of flow paths and sources but rather slight changes. So, phrases such as "quick depletion" or "sudden depletion" are a bit too much for me.

RC1A33: We thank the reviewer for pointing this out and agree that the wording can be improved. With 'quick' and 'sudden', we refer to shifts in behavior in a short (daily) timescale, rather than absolute concentration values. This will be stated more clearly in the revised manuscript. For the revised manuscript, we will strengthen the discussion regarding the variation of concentrations (or lack of it) to avoid over-interpretation. As pointed out by the reviewer, the processes and dynamics are meaningful, but the concentration changes can be better discussed.