RC2C1: This study presents the use of Unmanned Aircraft System (UAS) snow cover/depth measurements on a fen to complement the interpretation of how the dynamics of source areas and flow paths influence the export of dissolve organic carbon (DOC) from a small, high latitude catchment during snowmelt. Oxygen isotopes, continuous groundwater level measurements and snow surveys, as well as continuous measurement of an optical proxy for DOC are valuable complements to the data used in the analysis. Hysteresis and flushing indices are a key part of the reasoning used in ascribing the role of hydrological connectivity bringing in new source areas and dilution of source areas during events.

Major concerns:

RC2C2: Much emphasis is placed on interpreting the variation in DOC concentrations. However what strikes me more is the overall stability of DOC when the authors say that as much as 50% of the runoff peaks could come from snowmelt/precipitation inputs that are likely to be sources with little DOC.

RC2A2: We appreciate this observation and agree that the relative stability of DOC concentrations, despite substantial inputs of low-DOC snowmelt/precipitation during events, is a central result of our study. In the revision we will make this point explicit and frame the system as exhibiting near-chemostatic, predominantly transport-limited behavior: DOC concentrations vary modestly compared to discharge, so DOC load is largely governed by Q rather than by large concentration changes.

To align the interpretation with this behavior, we will (i) emphasize in the Results that event-scale DOC variability is small relative to discharge variability, (ii) discuss hysteresis direction and flushing indices as indicators of subtle timing/routing differences in transport rather than evidence of pronounced change in sources, and (iii) integrate this perspective in the Discussion as our primary interpretive frame during snowmelt.

In addition, we will include an isotope-based hydrograph separation for the peak melt period to quantify the contribution of recent snowmelt to streamflow. This analysis will allow us to state more precisely that the observed stability of DOC occurs even when event water fractions are elevated, reinforcing a transport-limited interpretation. Together, these revisions will clarify that the value added by the UAS observations lies in constraining where and when the landscape becomes hydrologically active, while the DOC response remains comparatively buffered.

RC2C3: The interpretation of the rich data sources in this paper would be much more convincing if the different catchment DOC sources and pathways alluded to in the discussion were quantified in an approach that keeps track of mass balances of water and carbon. Without that, I am concerned that conclusions may be drawn that are at odds with what the data show. In such a quantification, care should also be taken to explain what the inclusion of the UAS data adds to the interpretation of the other data.

RC2A3: We thank the reviewer for this important comment and agree that a coupled water–carbon mass balance would provide the most robust framework for quantifying DOC sources and pathways. We acknowledge that, with the available data, it is not possible to close a catchment-scale carbon mass balance with sufficient confidence, and we do not aim to do so in this study.

In the revised manuscript, we therefore more clearly delimit the scope of our interpretations to event-scale DOC–Q dynamics and relative changes in hydrological connectivity, rather than absolute source contributions or carbon budgets. Stable water isotopes allow us to quantify the relative contribution of recent snowmelt to runoff through an explicit hydrograph separation, which will be added in the revised manuscript. This strengthens the quantitative basis for interpreting the proportion of low-DOC event water contributing to discharge during peak melt.

The added value of the UAS-derived snow depth data lies not in constraining water or carbon mass balances directly, but in resolving the spatial and temporal heterogeneity of snow accumulation and melt across the fen. This information constrains where and when hydrologically relevant areas become activated during snowmelt, thereby improving interpretation of runoff generation, connectivity, and the timing of low-DOC inputs to the stream. In the revised manuscript, we will clarify this role of the UAS data and avoid interpretations that would require explicit carbon mass balance closure.

Minor comments:

RC2C4: Section 2.1. Is there any information on how well this groundwater level represents the spatial variability in GW level across the fen?

RC2A4: We had 3 similar GW wells on the site during the study, and the GW level dynamics were very similar in all of those, although the GW levels varied by some centimeters. We chose this particular well because it is located closest to the stream gauging station and was the first to thaw, meaning that it more likely represents the first areas where the GW level reaches below the ground surface.

RC2C5: Is there information on how the water from the larger, upslope catchment area moves through the fen? Are subsurface, preferential flowpaths an important feature?

RC2A5: We thank the reviewer for pointing out this important concern. We expect that the SAGA Wetness index provides a reasonable estimate of the main surficial flow paths through the fen, as it is calculated based on the whole upslope catchment area, and only the map extracted for the study site is shown in the manuscript.

Based on the hydraulic conductivity measurements done at the site, there is a layer with higher conductivity at approximately 30-50 cm depth, which can act as a preferential flow path. However, surficial flow paths likely dominate, especially during the peak melt period, due to the quick melting of snow and soil frost. We also took pore water samples from the peat profile during the study period, but the function of the preferential flow path could not be clearly identified from those. We will include the discussion of this potential preferential flow path in the revised manuscript.

RC2C6: Section 2.2 – Were the daily stream isotope samples taken at the same time each day. During spring, the runoff can have strong diurnal variation, so where one samples in that diurnal variation is important.

RC2A6: This is an important concern which we acknowledge. During the surveys on the peak melt period, samples were collected with a maximum 2 hours difference. However, most of the isotope samples were collected by the research station staff and taken within a few hours' difference, although, unfortunately, the exact sampling time cannot be confirmed.

RC2C7: Section 2.3 – While there is good coverage of snow depth in the open fen, is there any information about the progress of snowmelt in the larger, upslope forested area which is presumably a large source of the water measured at the flume?

RC2A7: Due to challenges of applying the UAS survey method to larger and forested areas, we restricted this study to the open peatland area and thus have no data from the snowmelt in the upper catchment. However, the research station has a forest measurement site with point snow depth monitoring located approximately 600 m downhill from the Puukkosuo study site. Average snow depth in this forest site was 28 cm on 14 May and 12 cm on 17 May. Corresponding snow depths could be assumed in the forested upslope catchment of the study area. This information will be included in the revised manuscript.

RC2C8: How well does the 10 cm resolution in elevation measurement of each cell compare to the variation of the topography across the fen? Is this important for assessing how well TWI can be identified.

RC2A8: SAGA wetness index was calculated on 50 cm DEM, and snow depth maps were resampled to a 50 cm resolution for the SWI-snow depth comparisons. The 50 cm resolution for SWI calculation was selected because it was fine enough to capture the influence of microtopography. Also, SAGA algorithm has been successfully used to represent soil moisture at fine resolutions (≤2 m, Riihimäki et al. 2021). Based on visual assessment and observations on site, 50 cm resolution provided the most accurate representation of flow paths. Finer resolutions are typically unsuitable for topographical analysis, and coarser resolutions did not capture the smaller flow paths (which have been observed on site) correctly.

RC2C9: Snow water equivalent was apparently measured at 5 points. What is the variation between points? This should be of relevance for the assumption of a uniform SWE across the fen. While an average SWE may be relevant for the water balance of the catchment, if one wants to consider local inputs of water to the catchment, then that variation in SWE will be important. A relationship between degree of change in snow depth and SWE from your point measurements might be a way to improve precision on where water is being input to the fen surface. The use of a reference from alpine areas (Lopez-Moreno) to argue that SWE is relatively uniform may be problematic. It would be better to know how SWE varies in a high-latitude fen as opposed to an alpine area with much more relief and different diurnal patterns of insolation.

RC2A9: We thank the reviewer for this insightful comment. SWE was measured at a single point, which will be clarified in the manuscript, and the measurement location will be added to the study site map (Fig. 1) for clarity. Results of the manual SWE measurement are reported in Table 2.

In calculating SWE for each cell of the study area, we used the snow depth retrieved from UAS data and a one-point measurement of snow density and thus assumed snow density to be uniform. Although we do acknowledge the limitations of one-point measurement of snow density, the study site is rather homogeneous in terms of vegetation cover and topography, and thus, minor variation in snow density can be expected. Topography and especially slope likely have some impact, but again, we expect this impact to be higher on snow depth than density, as shown by e.g. López-Moreno et al. (2013). Similarly, the study of Whittington et al. (2012) in James Bay Lowlands, considering different peatland types, also showed that no significant differences in snow density across landscape types were found. The reference will be added to the revised manuscript.

RC2C10: Section 2.4.1 – is there any test of how well the processing of UAS images was able to capture the variation of snow depth and SWI across the fen?

RC2A10: The processing method for UAS images aimed for the best model accuracy (XY and Z accuracy), which are presented in Table 1. We calculated the error metrics for each survey by comparing UAS snow depth to the manual snow depth measurements done at 7 points on the snow transect. This resulted in a mean RMSE of 9.79 cm for all surveys. Error metrics for each survey can be found in Table S1. SWI was calculated from LiDAR data, and no ground truth data was available for accuracy assessment.

RC2C11: Line 251: Data is plural, so “data was” should read “data were”.

RC2A11: Will be corrected in the revised manuscript.

RC2C12: Line 270-280 –MAJOR It appears that different source areas are being assumed AND that the concentration of the source areas is changing. How can one distinguish between changes in source area concentration and changes in source areas? Please be more clear about the assumptions being used in the hysteresis and flushing analyses.

RC2A12: We thank the reviewer for this important comment. Hysteretic behavior can be used to associate with different source areas and/or flow paths, while flushing behavior is associated with transport/source limitation of the solute. Direct quantification of source-area concentrations is not possible with the available data and hysteresis analysis, instead, inferences are made based on the relationship between concentration and discharge. In this study, hysteresis patterns are used to characterize the relative timing of changes in concentration with respect to discharge during individual events, which may reflect differences in flow routing, connectivity, or transport pathways, but do not uniquely identify specific source areas or their concentrations. Similarly, the flushing index is used to indicate whether solute export during an event appears transport-limited or supply-limited, but it does not provide direct information on absolute source concentrations.

In the revised manuscript, we will clarify these assumptions and explicitly distinguish between what is inferred from hysteresis and flushing patterns and what cannot be resolved from the data.

RC2C13: Section 3.1, Line 299 – should RMSE have the unit of cm?

RC2A13: Yes, RMSE is reported in centimeters, consistent with the units of snow depth.

RC2C14: Fig. 3. Please include uncertainty in this. It is worry ing that such a large portion of the predicted data is excluded as unreasonable. What does the non-excluded data actually say about the snow depth, especially when the ambition is to map the spatial variability of the snow depth.

RC2A14: We estimated the accuracy of UAS snow depth maps by calculating the error metrics by comparing UAS snow depth to manual snow measurements (Table S1). Negative values are more likely to represent shallow or zero snow depth within the measurement uncertainty. Thus, we decided to exclude them in Fig. 3 to ease the interpretation of snow depth, particularly in cells that still confidentially have snow cover. Uncertainty will be visualized in the revised manuscript by adding RMSE uncertainty bounds in Fig 3.

RC2C15: Lines 330-335. The relation of SWI and snowdepth is discussed. Does the SWI get influences by snowdepth?. I presume SWI is calculated during the snow-free period, but it would be good to clarify this in the methods.

RC2A15: We agree that the relation of SWI and snow depth can be explained more clearly in the manuscript. The SWI index is calculated from LiDAR data collected during snow-free period. SWI is invariable, but we discuss the mean SWI of snow-free areas, which are progressing through the study period. In this study, we do not intend to imply a direct causal link between SWI and snow depth but use SWI to describe the potential hydrological connectivity of these areas as they become snow-free. We will clarify this rationale in the methods section in the revised manuscript.

RC2C16: Line 384-388 The change in groundwater levels from being above the soil surface until the final runoff peak, after which the water table is below the soil surface seems to indicated a very important shift in flow paths and hydrological connectivity in the fen. Yet this major change gets little mention and does not seem to have a major impact on the DOC concentration. This seems to deserve more attention in the discussion where groundwater does not get much mention.

RC2A16: We thank the reviewer for pointing this out and agree that the position of GW level has significant implications for flow paths and hydrological connectivity, and this aspect could be strengthened in the discussion. The GW level remains less than 5 cm below the surface until the end of the study period, and these surficial layers of peat can be efficiently flushed during the snowmelt or can potentially have limited DOC due to the freeze-out effect, which could explain the minor impact on DOC concentration. We will improve the discussion of GW level by adding this interpretation in the revised manuscript.

RC2C17: Figure 7 – The "snow" value of the oxygen isotopes: is that sampled in the snow pack I assume? Please make that clear. If it is the isotope signature remaining in the snowpack, this means the snowpack is contributing meltwater with a less depleted isotope ratio. This complicates the interpretation. A quantitative hydrograph separation would be a much more satisfactory basis for the interpretation of the runoff sources. I would recommend an explicit hydrograph separation. There will of course need to be assumptions, but then the assumptions will be explicit.

RC2A17: We thank the reviewer for this important concern and suggestions. Yes, the snow value is measured from snowpack, sampled as described in methods: "During the field campaigns, snowpack samples were collected for stable water isotope analysis. The sample was collected by coring the whole snow profile from the top to the base of the snowpack with a snow tube corer (diameter 3.5 cm)". For clarity, the "snow" will be renamed as "snowpack" in the legend in the revised manuscript.

We agree with the reviewer that the isotope signature of the snowpack and associated fractionation during melt complicate the interpretation of runoff sources. In the revised manuscript, we will therefore add isotope-based hydrograph separation for the peak melt period to provide a more quantitative assessment of runoff source contributions. The underlying assumptions of hydrograph separation will be clearly stated and discussed.

Results:

RC2C18: Much is made about the variation in the DOC concentration, but what strikes me is how stable the DOC is, especially when the precipitation and snowmelt entering the catchment on top of frozen fens soils are potential sources of runoff with almost no DOC until they interact with the subsurface. But that interpretation depends on quantifying the inputs of "recent precipitation" along flow paths that stay out of the soil/peat. The fact that the groundwater well is frozen, with water tables above the peat surface during most of the study period (Fig. 7e) would indicate that the input of precipitation/snowmelt on the mire that makes it to the stream would be a source without much DOC at all.

RC2A18: We thank the reviewer for this insightful comment. We agree that, despite event-scale dynamics, DOC concentrations remain relatively stable throughout much of the study period, even when a large fraction of snowmelt inputs with low DOC are expected to contribute to runoff. This suggests that a fraction of streamflow continues to interact with DOC-rich zones, rather than being dominated by flow paths that bypass the soil or peat entirely.

In the revised manuscript, we clarify this interpretation and explicitly acknowledge that stronger dilution might be expected under dominant overland flow conditions on frozen peat. The persistence of relatively stable DOC concentrations therefore implies that subsurface interaction is occurring, likely due to spatially and temporally heterogeneous thaw conditions and flow routing. As indicated by the snow depth maps, snow cover, and by inference soil frost, is not spatially uniform. For example, in the immediate adjacency of the spring, no soil frost was observed, allowing infiltrating water to access peat layers and acquire DOC even during periods when frozen conditions dominate elsewhere in the fen.

Additionally, for the revised manuscript, we decided to recalibrate the DOC data, as a higher number of grab samples and longer time series significantly improved the calibration. This will lead to a slightly increased deviation in DOC concentrations (4.3–11.3 mg L, when previously 4.6–6.47 mg L), but the overall dynamics remain consistent. We will also improve the interpretation by considering the variation (or lack of it) in DOC concentration during the events.

RC2C19: Figure 8 – please find a way to help the reader better compare panes (a) and (b) of the figure. Stacking one on top of the other, rather than side by side would be one easy way, even if it would take more space.

RC2A19: We thank the reviewer for these suggestions. We agree that this figure can be improved for better readability, and it will be edited in the revised manuscript.

RC2C20: Line 480. The role of ice is mentioned, with the UAS snow depth analysis interpreting surficial ice as snow. Please say more about "formation of ice in low lying areas". My experience is that the upper centimetres of mires are often frozen during the winter, whereas forested upland areas often do not have much ice in the soil profile. Is the entire fen frozen to a certain depth (some centimetres), or is it a crust of ice on the snow and or soil surface that is referred to here. This is good to clarify since the depth of soil frost has a bearing on the interpretation of flow paths and where the DOC sources are that keep DOC from diluting more as snow melts on the fen, creating large inputs of low-DOC water close to the flume.

RC2A20: The ice layer here refers to the ice cover formed in wet depressions, where the water table is above the peat surface and easily freezes, forming a thick ice cover. This will be explained more clearly in the revised manuscript. Soil frost is likely present across much of the fen but is spatially heterogeneous. Unfortunately, we did not have data on the depth of soil frost, but this is supported by the field observations, where in some locations, soil frost was not present (a metallic stick could be pressed into the peat), and in some peat was frozen throughout the study period.

This is an important concern and refers to the central insight of the study: snowmelt and soil thaw are not spatially uniform across the peatland, leading to dynamic generation of flow paths and influencing stream DOC response.

RC2C21: Speaking of the flume, does it capture all the water running off from the upslope areas? With water tables above the ground surface on a relatively flat landscape, there might be a possibility of water bypassing the flume.

RC2A21: The flume location ensures that it captures only water that is passing through the peatland, but some water bypassing is expected. Based on the flow accumulation analysis and wetness index, a total of three outlets can be identified in the fen. We assume that the bypassing water is not significantly different in terms of volume or water quality, and thus does not impact on our analysis results. However, we acknowledge that this is an assumption and the possible limitations of it. We will include mentioning this source of uncertainty in the discussion of the revised manuscript.