RC1C1: Main comments
I was reviewer in the first round already and am pleased to see my comments carefully considered. I still struggle with 1) a tendency for over-reporting in the result section (esp. on the snow depth assessment) and 2) a tendency for over-interpreting minor changes in the (more or less static) DOC concentrations. I see point 2) more critically. See my comments below for more details.

RC1A1: We thank the reviewer for their continued engagement with our manuscript and for the positive feedback on the revisions. We appreciate the remaining concerns raised and have revised the text carefully according to the main concerns and more detailed comments.

Regarding point 1), we agree that parts of the Results section, particularly the snow depth assessment, were overly detailed. In the revised manuscript, we have updated this section by reducing the number of figures and integrating text regarding specific results or figures to avoid over-reporting. Please refer to the comment RC1A6 for the detailed description of the changes made.

Regarding point 2), we acknowledge the concern about potential over-interpretation of relatively stable DOC concentrations. We have revised the text to moderate the interpretation and avoid overemphasizing minor variations. We further refined the language to emphasize connectivity-driven transport processes rather than implying strong changes in DOC sources. Please refer to the comment RC1A9 for the detailed description of the changes made.

Detailed comments
RC1C2: L39: Consider to add how much % of time (of a year) snowmelt covers to underline the strong temporal inequality in DOC export.

RC1A2: Added in the revised manuscript, L40.

RC1C3: Fig. 1: Is there a stream/ stream network that can be shown here?

RC1A3: Thank you for the suggestion. The main waterways were added to Fig. 1 in the revised manuscript. These do not include the small surficial flow paths within the study catchment, which are represented in the SWI index map (Fig. 3)

RC1C4: L245f: This is the point I struggled with the last time. TWI applied to snow free area only is justified by the statement that snow free areas contribute to connectivity. Is there a literature reference or field evidence that can underline this assumption? I don't feel confident to judge on that but can imagine that there are also water flow paths on top of the snow or below the snow that may contribute to connectivity.

RC1A4: Thank you for the comment. It is true that not only snow-free areas contribute to runoff, as meltwater is also transported through intra-snowpack flow paths and on top of the impermeable snow layers, although this process can be expected to be less prevalent in flat areas (e.g., Lundberg et al., 2016; Webb et al., 2018). Although these processes affect water movements, connectivity in DOC sources is linked to the lateral connectivity of the organic soils, which is affected by the snow cover and soil frost (e.g., Eskelinen et al., 2016; Laudon et al., 2007, 2011), and this process is discussed later in the manuscript. To link this more clearly to the relevant process, 'hydrological connectivity' was rephrased to 'the connectivity of DOC transporting flow paths' at L246 in the revised manuscript

RC1C5: Fig. 2: Would it make sense to have the same y-axis for all plots? Just consider this.

RC1A5: We thank the reviewer for this suggestion. For Fig. 2, which presents spatial snow depth maps, the y-axis represents geographic coordinates and is therefore already consistent across all panels.

However, if the reviewer was referring to Fig. 3, we reconsidered the arrangement and agree that the shared y-axis could improve the interpretability of this figure. Thus, Fig. 3 was edited so that all panels share the same y-axis. The previous Fig. 3 was also moved to the supplementary materials (see next comment).

RC1C6: Fig. 2-5: This paper is focused on DOC pathways during snowmelt. I see that the snow depth assessment is relevant for that but I am convinced, that four figures on that are too much. Look into your main messages is the abstract – snow depth is not a main result - rather the snow-free areas and their connectivity/ SWI. Snow depth is sufficiently displayed in fig. 2. Fig. 3 is not needed for me. Fig. 4 is not needed as numbers are given in table 2 and spatial patterns can be also seen in fig. 2. Fig. 5 does not need to repeat snow depth in panel a and b. What is more relevant to be seen in a main figure is the SWI map that is displaying the actual connectivity. This could be combined with Fig. 5c.

RC1A6: We thank the reviewer for the good suggestions, and motivated by this comment, we decided to reduce the number of figures in the main text. Specifically, we moved the previous Figs. 3 and 4 to the supplementary material and integrated previous Fig. 5 showing snow depth and snow-covered area in SWI classes with a map of SWI classes of melted areas. We also reorganized the text linked to the figures that were

moved to supplementary material or removed. Changes and the new edited Fig. 3 can be found in L341-366 and L375 in the revised manuscript.

RC1C7: L401: Can you give SD of DOC concentration as well?

RC1A7: Standard deviation of DOC concentration was added to the revised manuscript.

RC1C8: L400ff: Do you think, analytically, and based on the calibration with rather low R2, the precision of DOC concentration with two digits is justified? The same also applies for discharge measurements.

RC1A8: Thank you for pointing this out. We agree that concerning the accuracy of DOC and discharge measurements, one-digit precision is sufficient. Thus, DOC and discharge measurements were rounded to one digit in the results section of the revised manuscript.

RC1C9: L550f: For me one of the most telling take-aways. Despite nearly exclusive snowmelt-water the DOC concentration is invariant and therefore the entire system does not act as a source limited system. For me it is not fully clear at this part of the manuscript why the intense flushing of DOC sources can sustain the same concentration over the snowmelt period. However, this is touched later in L609-610. Still there is a bit of a discrepancy between what you write here, 610 and around fig. 10.

RC1A9: We thank the reviewer for this insightful comment. We agree that the stability of DOC concentrations despite the high snowmelt water contributions is an important takeaway, indicating that the DOC sources are sufficient to offset the large dilution effects. As noted by the reviewer, this mechanism is discussed later in the manuscript; however, it was not sufficiently clear at this earlier stage, and we recognize that a discrepancy arose due to inconsistent use of the terms “sources” and “flow path activation.”

In the revised manuscript, we have clarified this by refining the wording throughout. In particular, we now emphasize the activation and increasing connectivity of DOC-rich flow paths rather than referring to changes in sources. This better reflects that DOC export during peak snowmelt is not source-limited, but is primarily controlled by the hydrological connectivity of organic-rich areas, which enables sustained DOC supply and offsets potential dilution from meltwater.

We have revised the relevant sections to ensure a consistent description of the processes and to more clearly link the observed invariant DOC concentrations to connectivity-driven transport dynamics. Changes can be found at L534, L540-542, L634-635, L639 and L634.

RC1C10: L585ff: This chapter leaves me puzzled. You state that DOC may stem from upland catchment area but the entire chapter is about snow depth there but not about DOC sources and pathways. This is not really helpful for the reader.

RC1A10: We thank the reviewer for pointing this out. We agree that the link between sources and flow paths was not clearly presented. Although progression of snow melt is closely tied to catchment connectivity and hydrological activation, this was not explicitly stated. In the revised manuscript, we have addressed this by adopting a clearer catchment-scale perspective, highlighting how delayed snowmelt alters the connectivity of upslope forested areas in relation to the peatland. Changes can be seen in L574-577 and L581-584 in the revised manuscript.