This paper used high frequency in situ measurements of DOC and discharge from 2018 to 2022 to examine the seasonal variation in concentration-discharge (C-Q) relationship. Overall, this paper presented a novel data set with comprehensive analyses of the C-Q relationship, including the C-Q slopes, the hysteresis patterns, and DOC load yield. The paper is mostly well written and clearly presented although some clarification of concepts and interpretation of results may need improvements . I hope the comments below help the authors improve the manuscript.

>> Thanks for your helpful evaluation and constructive feedback. We will use your comments to make improvements to the revised manuscript.

In the introduction, the authors used the term "transport process" as a general term for what their research focuses on. This term, however, has a very broad meaning and thus it may not be clear to readers what exactly the authors will investigate. I thus suggest the authors clarify what they mean by transport process. Although the scope and meaning of this term becomes clear in the methods, it would help readers understand the paper better if the term can be explicitly explained in the introduction.

>> We will clarify in the revised manuscript introduction paragraph that focuses on transport processes that by transport processes we are referring to what extent stream DOC in the catchment is source or transport limited, and how these variances are governed by evolving flow paths and sources of DOC.

The introduction section does not have a clear statement on the current knowledge gap and how this paper will address that gap. It is not clear what scientific question this paper tries to address. The analysis essentially computed almost all metrics one can do to a concentration-discharge relationship. I think a more clearly defined research question/hypothesis would make the paper's motivation more clear.

>> We attempted suggesting where research was lacking in the final sentence of each preceding paragraph, but we'd agree the paper would benefit from a more clear overall statement of the research gap at the start of the final paragraph in the introduction. In the revision we will highlight in the revised manuscript our research gap that there is a new for multi-year high resolution datasets to understand seasonal and annual variation, particularly within the Arctic where seasons are experiencing rapid changes.

Furthermore, we will add to the data analysis section in methods a hypothesis for each of the metrics we used in order to better justify their usage. In the manuscript, our justification for using the range of metrics is they all provide differing information, therefore using a metrics allowed us to better inform our interpretation of the results.

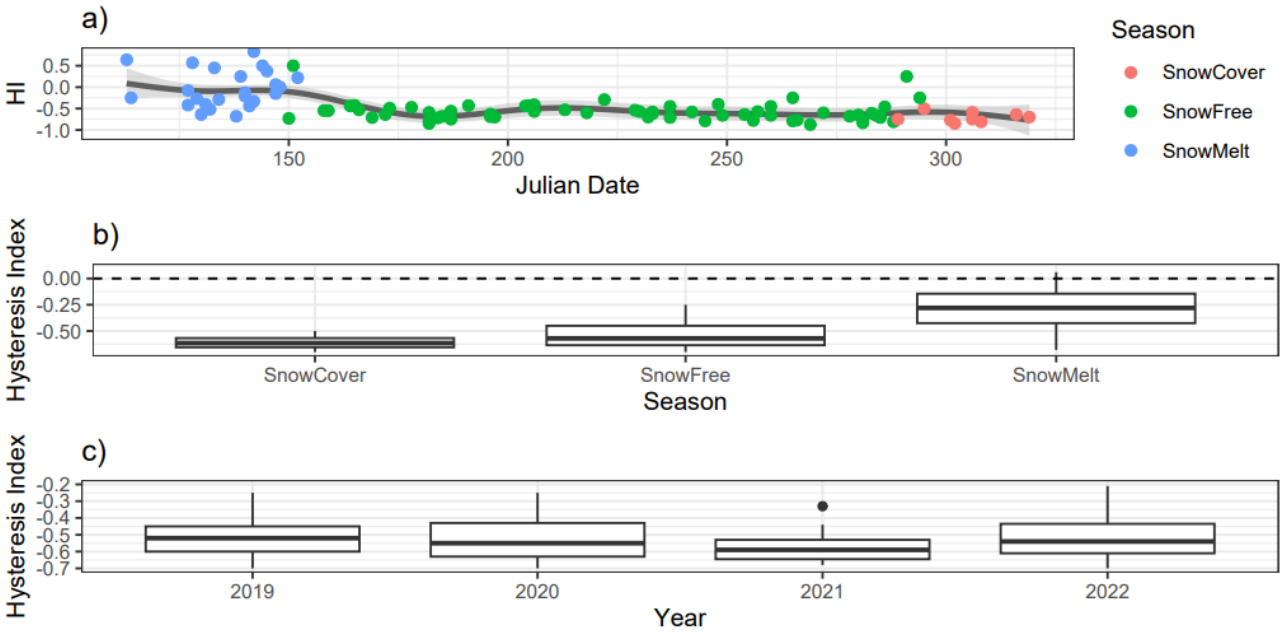
The authors suggest that the variation in C-Q slope between years were generally smaller than seasonal variation (e.g., line 321). Is this really the case? If we look at figure 3(a), it appears that, at least in November, the among-year variation in C-Q slope is larger than

month-to month variation within the same year. I thus think the statement that month-to-month variation is larger is not fully supported by the data.

>> We agree it would be more accurate to say that in most months the month-to-month variation is greater than the year to year variation. The differences in April and November were driven by the fact that they vary year to year between being either in complete snow cover for the entire month (meaning very little variation in flow range), to some snow cover leading to much bigger between year to year differences than other months. Thus we will clarify in the revised paper that the month to month differences is greater than the year to year variation for the May to October period, and explain the reason April and November were different.

The authors only used single peak data for analysis of event C-Q relationship. How many events have multiple peaks? If multi-peak events were not excluded, would the results remain the same? Some additional analysis including the multi-peak event would be helpful.

>> We initially used single peak events for consistency across the analysis, but we would agree it is a good idea to examine multiple peak events. We subsequently calculated multiple peak events (by calculating HI for each peak) which we show in the figure below. This added 37 new events to the analysis. The new analysis does not change any interpretation of the results. The snowmelt season still has the highest average (and most variable) HI, the snowfree and snowcover season are still consistently very negative in their HI values. Hence, we will include this new figure in the revised manuscript, but it will not necessitate further changes to the text or interpretation thereafter.



Below, I listed detailed line-to-line comments.

Line 187: please provide citation information for equation 1.

>> The equation is derived from our own calibrations of the instrument thus there is no reference, though we will make this clear in the revised manuscript.

Line 209-214: Could you please provide numeric criteria you used to define snow cover, snow melt and snow free season. From figure 2, snow depth fluctuates even in what you classified as snow cover season. Wouldn't that cause some period of time to be classified as snow melt season within the currently defined snow cover season?

>> We gave the following criteria in the methods:

"The snowmelt season was defined as the period starting from the onset of snowmelt, indicated by a decline in snow depth concurrent with an increase in flow, until there was no remaining snow cover at the Kenttärova site. The snow cover season referred to the period when snow cover was present and persisted until the subsequent snowmelt season. The snow free season referred to the period between the snowmelt and snow cover seasons."

As such, snow free is the period from which the snow measurement at meteorological station is 0 cm, to the period when the permanent snow cover of the year begins. The snow cover season is the period beyond which the meteorological station refers to the period where snow depth is greater > 0 till the following spring snowmelt. The snow melt season is defined as the point in spring where a decrease in snow depth occurred alongside flow increase. The logic for classifying snowmelt in this way was that snow depth often fluctuates for reasons that are not due to melting (for example through snowpack consolidation), so a pure snow-based definition of snowmelt would not be helpful. We did not classify events in early snow cover season as "snowmelt season" as in this study we are interested in seasonal differences, and October/November have very different seasonal characteristics to the snowmelt in spring snowmelt season, which was borne out in the analysis (for example in the hysteresis data).

In the revised manuscript, we will make clear the distinction that the snowmelt season is in specific reference to the spring snowmelt season, and better clarify the numerical definitions.

Line 225: is it each season or each month? From the figure, it seems C-Q analysis was done to data in each month.

>> We will change this to say "each month" in the revised manuscript.

Line 237: A brief explanation on how HI index was calculated could be helpful.

>> Indeed, we will add a brief description of how the HI index is calculated to better inform the reader in the revised manuscript.

Line 248: the term "dynamics" is a bit vague here. Could you please be more specific and explicit about the meaning of this term here?

>> We will change this line to be clear that the yield analysis will reflect changes in transport limitation and source activation and capacity.

Table 1: Is the unit of flow wrong? Shouldn't it be L s-1 km-2?

>> Yes, we will change this in the revised manuscript.

Figure 3: how was coefficient of variation calculated? What is the standard deviation used in the calculation of CV? If I understand correctly, the C-Q slope here is derived by linear regression using data in each month. Is the standard deviation used in CV calculation obtained from the standard error of slope from the regression? If so, the CV calculated here is not meaningful because the SE of slope from regression shows the uncertainty of estimation, not true variation in slope over time within a month.

>> The coefficient of variation was calculated for the DOC data, not for the C-Q slope data. Thus it shows the CV for DOC by month. The reasoning for including it was to see how much the DOC data varied month to month in comparison to the C-Q data. We will change the wording of the figure caption to make this clearer.

Figure 6: similar to my comment to table 1, shouldn't the unit of DOC load yield be kg DOC km-2?

>> Yes, we will change this in the revised manuscript.

Line 364: please give exact p-value, not just a range.

>> We will add this to the revised manuscript.

Line 375: please give the degrees of freedom of the F test statistic.

>> We will add this to the revised manuscript.

Line 382: what's tested here is that the slope is statistically different from zero assuming there is a linear relationship. Whether the relationship is linear or not is not tested. Thus, the term "remained strongly linear" is a bit misleading.

>> We will change from "remained strongly linear" to "were strongly linear". While we test slopes, we do also feature individual linear regressions on the graphs to show that in all cases the relationships were linear.

Line 420-432: the explanation here may need further consideration. The trend in C-Q slope seen using data within each month (figure 3) is not evident when analyzing event C-Q behavior. Thus to say that the patterns seen in figure 3 suggests limited source does not reconcile with what is shown in figure 4(a), particular considering that ~60% of flow occurs in events.

>> Figure 3 is all flows, which includes event flow, therefore across the entire range of flow the catchment is more limited in source (or less transported limited) in the snowmelt

period compared to other months. In Figure 4 we compare only event flows. I think the important distinction to add would be that at the kind of flows that constitute event flow, there is a tendency for all events to become much more similar levels of source limited, likely due to exhaustion effects in sources at the highest flows (as shown by the lower C-Q slopes for all months for events compared to the entirety of flow). Thus, we will discuss the difference with event flow, although keep the assertion that snowmelt was more source limited across the entire flow-range.