Response letter to the reviewers: Manuscript hess-2022-426

In this response letter, the reviewer's comments are in ***italic bold black***, our responses are in blue and significant new text added to the manuscript are in *italic green*. Changes made in the manuscript are tracked and line number referred to the revised manuscript.

Reviewer #2

The authors conducted a study to examine the relationship between hydrological connectivity and carbon exports in a peatland-dominated watershed. They aimed to achieve several objectives: a) establish the connection between dissolved organic carbon (DOC) exports and peatland hydrology, b) quantify the lateral export of DOC into the stream at the catchment scale, and c) identify patterns of DOC mobilization during high-flow events. They propose a method to estimate carbon exports based on the relative contribution of the peatland in relation to the whole watershed.

The authors deployed a multiparameter probe at the watershed outlet to measure fDOM, turbidity, DO, SpC, water temperature, and pH hourly from June 2018 to May 2020. The relationship between fDOM and DOC was assessed by analyzing temperature-corrected fDOM signals and DOC concentrations obtained from grab samples collected during 5 and 4 sampling events in 2018 and 2019, respectively. Streamflow was estimated from year-round water level measurements, with the relationship calibrated using field streamflow measurements, except for the spring thaw period, where a PHIM model was used. Hidden Markov chains were used to classify the streamflow data into high and low flow periods. Water table elevations were recorded hourly at six wells during the growing season from June 2018 to October 2020.

The design of the study is sound. The paper is well-written, the figures are clear, and the methods section includes good detail. Altogether, the study is a good contribution to the field and improves our understanding of the role of boreal headwater streams in the carbon cycle. I have some recommendations I think will improve the flow of the manuscript and its impact. My major recommendation is to tone down some of the statements regarding WTD and Q relationships since the study does not technically prove lateral flow directionality (see comments below regarding Lines 399 and 515) and to slightly expand the discussion to explain better the results on the context of other studies done in the same site and other boreal streams (see comment below regarding L457).

We thank the reviewers for their comments, and we are pleased that you recognize the overall quality of our manuscript. We are concerned by the recommendations you made and we are convinced that they helped us to improve the quality of the manuscript. You will find responses and changes we made to the manuscript according to your comments.

Specific comments:

L190 "The calculation method was…" replace by "The calculation method for…. was calculated…."

The phrase was changed according to your suggestion and the calculation method of the discharge were precised.

Line 199-201: "The calculation method for the discharge was described by Taillardat et al. (2022). The distance between the surface water and the ultrasonic distance sensor gives the flooded vertical area in the 'V-shaped' weir and the Thomson's triangular-notch equation allow calculating the discharge from water-level measurements (Shein, 1979)."

L191 Please explain how you measured streamflow used in your calibration.

This section of the methodology was rewritten to clarify the method according to this comment and the previous one. A first ultrasonic sensor was installed during the 2018 growing season (see previous comment). But as it was damaged during the 2019 spring freshet, it was replaced by a water-level data logger starting from June 2019.

Line 202-208: "Starting from June 2019, a water-level logger (U201-04, Hobo, Onset, USA) was installed at the stream outlet to replace the ultrasonic distance sensor, damaged during the spring freshet. Water-level-discharge rating curves were calculated following the method described by (Taillardat et al., 2022). Discharge was measured at the stream outlet using a portable flow velocity probe (Flo-mate model 2000, Marsh-McBirney Inc., USA) measuring water velocity in a stream cross-section at subsections of 20 cm with intervals. The cumulative discharge (Q; in m3 s-1) was measured by summing the discharge obtain for each subsection by Equation (1) where V is the water velocity measured by portable flow velocity probe (in m s-1) and A is the flooded vertical area (in m2) and obtain by multiplied depth (in m) to the width of the section (in m)."

L192-193 "daily water discharge was modelled using PHIM" – please clarify if you modelled streamflow only during spring thaws or for all year round.

The modelled period was clarified. Also, we precise reasons why PHIM model was used.

Line 209-214: "Discharge monitored data during the spring thaw was not available due to the absence of monitored data from the ultrasonic distance sensor SR-50A during 2019 spring freshet, because the sensor was damaged during the flood and because of measurements during 2020 spring thaws cannot be measured as the flooded section exceed the stream bed and the Thomson's triangular notch equation cannot be applied. Consequently, daily water discharge was modelled during the whole studied period, using the Peatland Hydrologic Impact Model (PHIM) developed by Guertin et al. (1987) and detailed by Riahi (2021)."

L195 - How deep were the wells?

Wells were installed at 2 meters depth into the peat as it was now clarified in the method section,

Line 216-218: "Water table depth (WTD) was recorded hourly at the six wells (Fig. SI.3) inserted at about two meters depth into the peat and equipped with a water-level data logger (HOBO, Onset, USA) […]."

L196 – "water-level data logger… from June 2018 to October 2020" – is that right? Or were they only deployed during the growing season?

As it was presented in Fig.3.b, water-level data logger monitored WTD from June to October 2018 and from June to October 2019 only. Harsh climatic conditions have emptied WL data loggers batteries and as we cannot collect data outside of the growing season, WTD were only available for these periods. The manuscript was adjusted.

Line 216-218: "[…] for continuous hourly measurement of WTD and temperature, from June to October 2018 and from June to October 2019 as described in Prijac et al. (2022)."

L203 – please include at how many sites.

The phrase was removed (line 215) as those data were not used afterwards. Is the water table temperature measured with water level logger which was used.

L220 – "Gap filling (..) could not be performed during (…) the non-growing season due to the bad quality of the model (i.e., low linear relationships between the predicted and measured values" however, in L146-147 you indicate that grab samples were only collected during the growing season. Which one is correct?

There was an error in the previsouly submitted version of the manuscript. Indeed, the gap filling correctly performed for the complete daily time series (from June 2018 to May 2020) which allowed us to model daily DOC concentrations during this period (i.e., non-growing season). The last version of the random forest method description was included in the manuscript. We apologies for the confusion.

Line 240-246: "Gap filling of the DOC concentration was also performed during the rest of the time series (i.e., non-growing season). The same method was applied on the daily-interval data set to model the missing DOC concentrations (51.3% of the data set). The data set contained the PHIM simulated discharge, water temperature, pH, dissolved oxygen saturation and specific conductivity. The training data set corresponded to 26% of the data set and validation data set corresponded to 22.7% of the data set. The validation test of the random forest model gave a relatively good fit with a strong positive correlation between observed and modelled DOC concentration (cor = 0.84; p-value < 0.0001), the mean root-square residuals was 2.15 and the percentage of variance explained by the model was 71% (p-value < 0.0001; Fig. SI.1.b)."

L222 – "the 10th quantile of the DOC concentration was used to fill the gaps": Please explain the rationale behind using the 10th quantile.

This phrase was removed as the gap filling method was rightly applied to the daily-interval dataset.

L232 – "HMM was used to classify the time series": Specify which time series – flow with PHIM outputs? Or original water level?

We specify in the manuscript that HMM was used in both daily and hourly interval datasets.

Line 270-271: "The HMM was applied on both 1h-interval data set and on PHIM modelized daily-interval datasets."

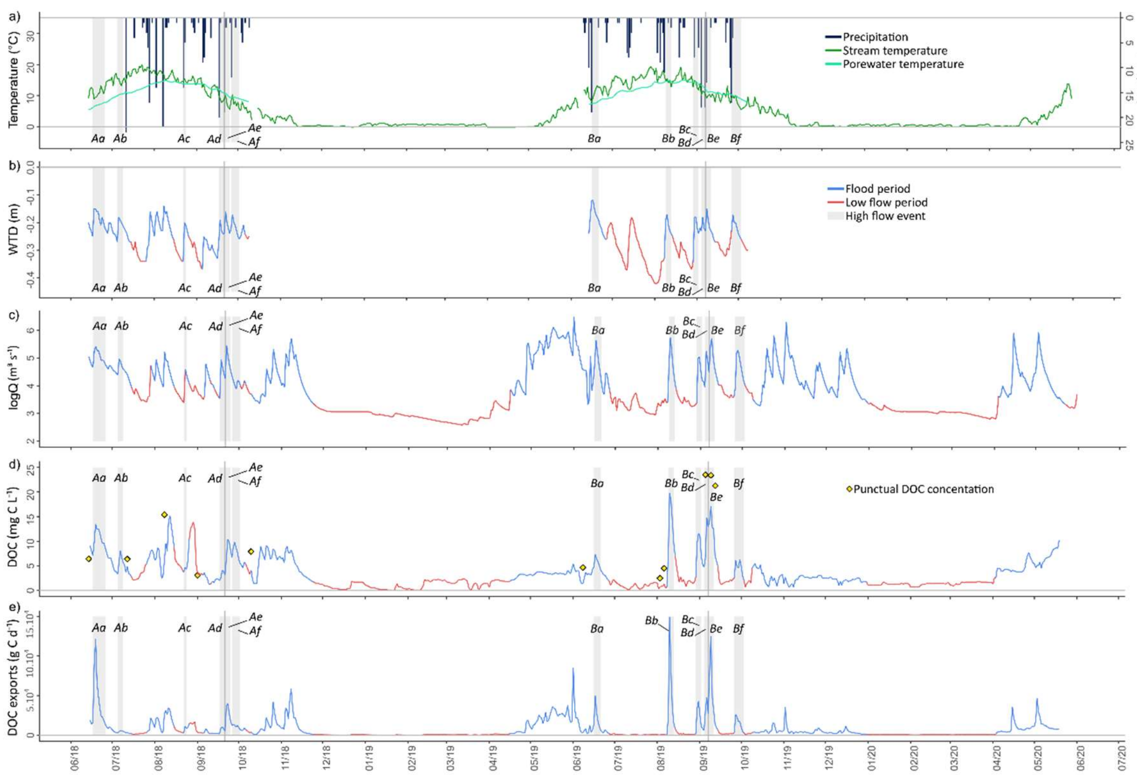
L244 "For each flow event" – Replace by "For each of the 12 flood events"

The manuscript was corrected following your suggestions.

Line 280: "For each of the 12 flood events, several descriptive and quantitative indicators were calculated; they are described in Table 1."

Figure 3 – add units to discharge

The unit was added to the panel (c) of figure 3.



L373 – "Although the events of cluster 1 had the highest ΔDOC" but in L369 "but the events in cluster 1 presented the lowest ΔDOC".

There was typo here as the highest ΔDOC was measured for cluster 3 rather than for cluster 1. The manuscript was corrected.

Line 414-416: "Although the events of cluster 3 had the highest ΔDOC, the events of cluster 2 had the highest Qmax and ΔQ, namely 0.086 ± 0.018 and 0.065 ± 0.022 m3 s-1, respectively."

L399 – "The increase in WTD led to an increase in Q" – here and in general, I would tone it down, maybe "the increase in WTD coincided with an increase in Q". I don't believe you are strictly proving causation.

It is a good point you mentioned here. Even if we agree to soften this statement, in a peatland-dominated watershed, we could reasonably hypothesize that the peatland significantly contributes to the stream discharge through subsurface runoff, following the increase of the WTD (Bishop et al., 2004; Tunaley et al., 2016, 2017).

Line 440-441: "The increase in WTD coincide to an increase in Q and DOC concentrations at the outlet and, consequently, to an increase in DOC exports (Fig. 2)."

L457-459 – "DOC only accounts for 13.6%-18.8% of the total aquatic carbon" – it would be worth expanding this paragraph since it can provide a comprehensive context for your research, for example, how did you account for total carbon – is that including particulate exports? Can you use the data from Taillardat et al 2022, your data, and bibliography to provide evidence that the lower-than-expected DOC exports are due to higher rates of transformation to GHG?

Figures (13.6-18.8%) were not good as the wrong GHG flux was considered. The GHG flux is 1.08 g m^{-2} y^{-1} (Taillardat et al., 2022) and DOC fluxes were 1.87 and 1.27 g m^{-2} y^{-1} for 2018-2019 and 2019-2020 respectively. Given that change, the proportion of DOC exports is coherent with data from the literature that ranged this contribution between 46 and 95% (Roulet et al., 2007; Nilsson et al., 2008; Worrall et al., 2008; Dyson et al., 2011; Holden et al., 2012; Huotari et al., 2013; Dinsmore et al., 2013; Leach et al., 2016). The paragraph was modified according to this change. Also, it was precise in these changes that aquatic exports only include GHG (CO2+CH4) and DOC.

Line 525-530: "In terms of total carbon flux in our studied peatland, Taillardat et al. (2022) estimated the stream carbon GHG (CO2 and CH4) aquatic exports as 1.08 g GHG-C m-2 y-1. It gives a total aquatic carbon exports (GHG + DOC) ranged between 2.35 and 2.95 g C m-2 y-1 and a contribution of DOC exports accounting for 54-63% of the total aquatic carbon exports. This is in line with previous studies which observed a DOC contribution to aquatic carbon flux ranged between 46 and 95% (Roulet et al., 2007; Nilsson et al., 2008; Worrall et al., 2008; Dyson et al., 2011; Holden et al., 2012; Huotari et al., 2013; Dinsmore et al., 2013; Leach et al., 2016)."

L515 – "given the lack of a direct link between peat porewater discharge and DOC exports from the stream" – Maybe I missed that detail, but my understanding is that you did not measure WTD during the non-growing season, when the majority of low flow occurs. If that is the case, I would tone down the statement and acknowledge the limitations of the study in this regard.

You raised an interesting point, then we precise "during the growing season" in the phrase to avoid any confusion. However, if we agree that the absence of WTD measurements during some low-flow periods could be a limitation, it is important to consider that those periods also correspond to the ice-covered period. In this context, we hypothesized that DOC exports are not only limited by the hydrological connectivity but also by the stream freeze that leads to very low discharge (Fig. 3.c).

Line 605-607: "By contrast, during the low-flow periods, the catchment area is considered the conservative surface reference in the calculation given the lack of a direct link between peat porewater discharge and DOC exports from the stream observed during the growing season."