Anonymous Reviewer #3

In this study the authors investigate how permafrost thaw affects mineral weathering sources of inorganic carbon (IC), and how the fluvial IC is cycled across different scales. Specific focus is on retrogressive thaw slumps (RTS) and their major contribution to IC yields and biogeochemical processes across fluvial networks draining permafrost regions. The study is based on one synoptic summer sampling campaign of three different fluvial transects covering different scales, and where samples were taken for a comprehensive set of chemical and isotopic variables. The authors conclude that rapid weathering in the RTS runoff enhance both atmospheric CO2 emission and downstream DIC transport. They further show that the IC signal from RTS have a major downstream impact across large scales although the RTS impacted area covered less than a 1% of the total catchment area.

The manuscript focus on an important topic that is very suitable for publication in Bio-geosciences. The current thaw of permafrost regions is of major concern and the response in the landscape C cycling is a central issue. Much of the literature is focusing on the mobilization of organic C stocks and the subsequent mineralization into CO2 and CH4. In comparison, relatively little focus is given to the inorganic C mobilization and to what degree mineral weathering upon permafrost act as a source or sink for atmospheric C, and how it affects biogeochemical processes in aquatic systems.

Thank you for the encouraging comments. We appreciate it!

General comments:

With this background the manuscript is an important contribution to the research field. The authors present a comprehensive and neat data set from a data scarce region, and where they disentangle different sources and processes affecting the fluvial IC in a (mostly) very convincing way. The manuscript is very well written but I have some points that need to be clarified prior to a publication. These issues are mostly to strengthen the argumentation by the authors but also to fully capitalize on their findings.

Many thanks for your helpful feedback. Please find our replies below.

Detailed comments:

Ln 15-18, a very long sentence with plenty of information. I suggest to split it.

Revised.

Ln 153-160, it is hard to grasp the uncertainty of the stream flow section. i.e. how certain the Q estimates are. On the other hand, the water or solute yields are a relatively minor part of the ms.

This is a fair point. We added a figure to the appendix which shows the relationship between our discharge measurements and the estimates from our model. As the figure shows, the 95% confidence interval is relatively larger at higher discharge levels. The strong, linear relationship provides some confidence in our estimates of discharge.

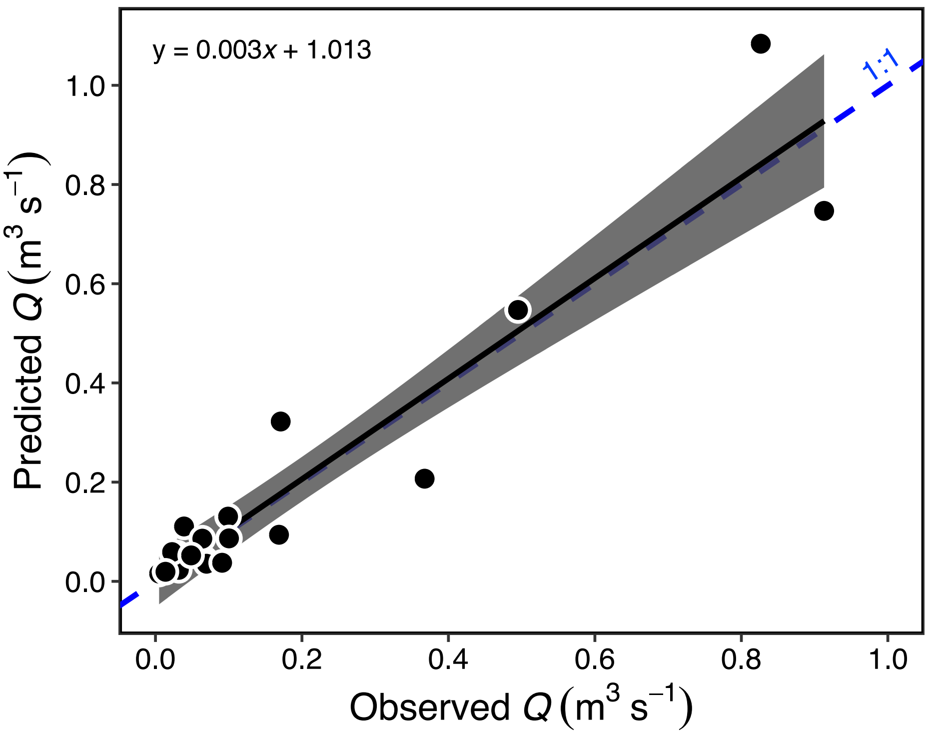

Figure A2. *Estimated vs. measured discharge (Q) ($p < 0.001$, $R^2 = 0.89$, $F_{1,18} = 150$) for 20 streams in the Stony Creek watershed. Grey band represents the 95% confidence interval around the regression. Estimates were made using measurements of stream width, Q, and a hydraulic geometry model (Gordon et al. 2004) (see Sec. 2.6). The model (Eq. 1) was used to estimate Q in the Stony Creek tributaries.*

Gordon, N. D., McMahon, T. A., Finlayson, B. L., Gippel, C. J., & Nathan, R. J. (2004). Stream hydrology: an introduction for ecologists. John Wiley and Sons.

Ln 237-239, how come these three variables were used in the MLR? Comes currently a bit out of the blue and needs to be better motivated.

Hydrology, terrain roughness, and vegetation productivity were included as covariates because they are known to be among the primary landscape controls on DIC cycling. We have clarified this and included citations.

Ln 239-245, again it is hard to judge the certainty in this modelling effort given the already above raised concern about the Q estimation.

Please see our reply to your comment for Ln 153-160. We acknowledge there is some uncertainty. Yet, this approach enables us to generate a first estimate of the relevance of RTSs in *carbonate alkalinity production and export relative to other landscape conditions known to influence DIC in fluvial networks.*

Ln 259-, I guess very much a question of personal taste but I feel the ms do not benefit from the mixing of results and discussion. It would be easier to keep focus by separating them in my opinion.

We agree with you and Reviewer #1 about this. We restructured the manuscript so that the Results and Discussion are presented separately.

Ln 278, I am not familiar with the given reference, but what is meant by "regional carbonate"? Also in this couple of sentences, I agree with the overall argumentation, but can you completely rule out a biotic source contribution? The fractionation between carbonate and CO2 (8‰ is rather theoretical. Could a mixing with geogenic and bio- genic IC be possible for generating 13C-CO2 of -11.4 to 12.1‰ You have a substantial DOC pool which is also cited by being "relatively biolabile".

This was meant to read "... regional carbonate bedrock". Revised. Yes, good point about CO_2 being a mix of biogenic and geogenic sources. We have added brief text clarifying that these isotopic values may also reflect some contribution of ^{13}C-depleted CO_2 from biogenic sources.

Ln 285, how CH4 was sampled is mentioned in the methods but from what I see this is the only place where any data is presented, and then very shortly. Maybe the data is saved for another story but I believe it would further strengthen the story if it could be included for example in table 1 and with subsequent incorporation in the text.

We tell the CH_4 story in an earlier publication (Zolkos et al. 2019, e.g. Sections 3.1, 3.2, 3.3, 4.1, 4.3, 4.4). CH_4 measurements in this study were done to assess for potential effects from methanogenesis on stable CO_2 isotopes.

Zolkos, S., Tank, S. E., Striegl, R. G., & Kokelj, S. V. (2019). Thermokarst Effects on Carbon Dioxide and Methane Fluxes in Streams on the Peel Plateau (NWT, Canada). Journal of Geophysical Research: Biogeosciences, 124(7), 1781-1798.

Ln 310-313, yes it could be due to adsorption to RTS sediments, but I guess it could also be due to lower mineralization than degassing rates. Might be worth to mention.

Yes, good point. We have clarified this in the text.

Ln 347-349, is it really clear that biotic CO2 were the primary source of DIC in the headwaters of Stony Creek? Could not geogenic sources still be highly influential? The 13C-DIC and 13C-CO2 values (-11.6 and -13.8‰ respectively) points towards a biogenic/geogenic mixing, or?

Fair question. As noted, mixing between the stream and atmosphere was a primary CO_2 source. This is supported by the values from other measurements, such as pCO_2 at an approximately atmospheric level and also low HCO_3^- and pH. The former suggests relatively minor biotic CO_2 production (organic matter mineralization) and/or greater effects from degassing on CO_2 than from biotic processes. The latter suggests stronger effects on DIC speciation from variability in pH ($CO_2 > HCO_3^-$) than from mineral weathering. Together, these results suggest that geogenic mixing is a less parsimonious explanation.

Ln 403-405, do the study really evaluate “across gradients of thermokarst disturbance”? I believe something like influence of RTS on IC cycling and how this signal is propagated across different fluvial scales is better describing the story.

Good point. Revised.

Ln 419-434, I somehow miss the full interpretation of the findings of the current study for the large scale picture. How do you suggest your results should be considered in large scale estimates, i.e. how does it affect the previous judgement of the area as a “modest source of CO2”.

Fair point. We consider the broader relevance of our findings in our revised Discussion and we provide an updated conceptual model to help generalize our findings.

A general question: how common are RTS across permafrost regions worldwide? How applicable are the findings here for other areas?

Good question. We consider this in the Introduction and we believe that our edits help to clarify this.

Figure 1. For a non-north American reader, a more large-scale inset of where the area is found would be appreciated.

Good suggestion, thanks. Figure revised.