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Interactive comment on "Thermokarst amplifies fluvial inorganic carbon cycling and export across watershed scales on the Peel Plateau, Canada" *by* Scott Zolkos et al.

Anonymous Referee #3

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Overview:

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 emis-

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Discussion paper

sion 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 Biogeosciences. 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.

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.

Detailed comments:

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

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.

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.

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.

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.

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 CO_2 (8‰ is rather theoretical. Could a mixing with geogenic and biogenic IC be possible for generating 13C-CO_2 of -11.4 to 12.1‰ You have a substantial DOC pool which is also cited by being “relatively biolabile”.

Ln 285, how CH_4 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.

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.

Ln 347-349, is it really clear that biotic CO_2 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-CO_2 values (-11.6 and -13.8‰ respectively) points towards a biogenic/geogenic mixing, or?

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.

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 CO_2”.

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

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

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