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Discussions
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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 #1

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Zolkos et al., present a high quality characterisation of running water chemistry in a sub-catchment of the Peel River where the aim was to determine the effect of retrogressive thaw slump (RTS) on DIC sources and export. The study design, incorporating three transects at different spatial scales (1. retrogressive thaw slump (RTS) runoff water, 2. an intermediate size catchment with direct fed with RTS runoff, 3. A large catchment fed indirectly with RTS runoff through its tributaries), is an interesting and innovative sampling approach. The dataset, including a large number of key variables, is also of very high quality. The research question is also highly relevant to our understanding of the permafrost climate-feedback. The author rightfully stated that changes in carbonate alkalinity export in response to permafrost degradation has been far less

studied that those for organic carbon and carbon dioxide. The study is also taking place in a relatively understudied region, which makes it even more valuable. While the subject and design of the study is of high quality, I find that the discussion and conceptualisation of the result need significant improvement. In short, the paper does not make full use of its potential.

The novelty of the paper lies in the approach of scales on the effect of RTSs on DIC export. At the moment, these three scales are taken separately, into three almost individual studies. Is the message simply that we can perceive the RTS effect at each of those three scales or is there a greater interpretation of how these effects integrate with increasing spatial scale and decreasing land-water connectivity? The paper would have more impact if the author could conceptualise these results and formulate how RTSs affect DIC cycling across scales rather than simply testifying that it has an influence. For example, does the "RTS effect" amplifies, is conserved, accumulates or becomes diluted with increasing scale. The author already documented that RTSs alter riverine DIC cycling in a previous publication in GRL (Zolkos et al, 2018). At present, this manuscript adds little to this state of knowledge, but this could be remediated by conceptualising further the effect of scales.

I have made a few suggestions to improve the presentation of results and conceptualisation of the discussion.

The sampling design is interesting and valuable, but arguably difficult to communicate to the reader. Having the Results and Discussion section together makes it even harder for the reader to put together the key results, and follow the discussion points that are mixed through the text. I recommend to separate these two parts. The first part of the results and discussion section details the changes in water chemistry in each of the three transect. The first part of the Result and Discussion section (section 3.1, 3.2 ad 3.3) details the water chemistry patterns in each transect with discussion points mixed through the text. Having this structure increases the impression that this study actually involves three separate studies rather than one. I suggest to also structure the

results by water chemistry variable rather than sites. The study measured a large number of important and interesting water chemistry variables. Each should be presented clearly in the result section for the reader to identify. Each sections should provide, among other things, the overall range in values for the whole study, compare this range between each of the three scales and within each transect.

Rather than naming the sites by their official river name, why not call them with a more conceptual name that represents the idea behind the sampling design. I like that the symbols in figures have numbers to indicate their position along the transect, but RTS FM2 site DC and SC has little meaning for the reader. I have also provided a few suggestions below to format the figures in a more visually telling way.

Playing with your dataset I attempted to trace the d13CO2 source with the miller-trans plots, I found a clear difference in that value for the RTS runoff site (d13Csource -12‰ and the DC and SC rivers (-22.9‰. This suggest two predominant CO2 sources in this catchment and those end-members could potentially be used for calculations. The RTS site is consistent with a geogenic CO2 source, while the rivers have a predominant biogenic soil CO2 source. Would it be worth including this kind of approach to your results?

The effect of scales, with a varing degree of terrestrial connectivity, is only discussed in the context of circumpolar region with, but I believe that the study should also be put in the context of the broader literature, including lower latitude catchments, where many studies have also examined the effect of changing land-water connectivity with size.

I find it interesting to see the downstream changes in HCO3 concentration. It could be worth mentioning that studies modelling stream CO2 evasion based on d13CO2 value assume that carbonate alkalinity is conserve in river networks (Polsenaere 2012 Geochimica et Cosmochimica Acta and Venkiteswaran 2014 PLOS one).

The term thermokarst and retrogressive thaw slumps are used interchangeably, I suggest to stick to retrogressive thaw slumps since this was the focus of the study and the

findings may not be applicable to all kinds of thermokarst disturbances.

The term "abiotic-inorganic processes" is a vague term to me, what are the processes included in that? Carbonate equilibrium reaction and CO2 evasion? Could you be more specific? Either a more specific term should be used or the choice of terminology should be justified in the text.

Abstract: Line 21:26: The results/discussion section of the abstract list changes in water chemistry in each of the three transect. I believe this section, and other relevant section in the main text, should come up with a more unified message of the RTS effect across scales, rather than at individual scales.

Line 30: I have trouble with the word "prevalent" here since it implies that one is larger than the other, while such calculations have not been done in the study.

Introduction: Lines 32-33: This first sentence would introduce better the second paragraph where the source/sink relationship of DIC is detailed. This first paragraph discusses how the arctic landscape is changing and what is known of its influence on DIC export.

Lines: 41-44: Since we include CO2 in this pool, the increase in soil respiration and/or increase in aquatic DOC degradation should also be part of this list. The citations be separated to indicate which if the listed mechanisms have been highlighted by each study.

Lines 47-64: This is mostly textbook material and could be synthesised. I think what you want to express is why it's important not only to account for the mass of DIC exported but also to identify its sources DIC. Without this we can't determine whether this is a new sink or source of C in the short and long term perspective. I would suggest to move some of this information in section 2.5 of the methods and rework this paragraph to emphasise the simply the importance of source separation for DIC.

Line 65: Could you state the representativeness of this study, which areas across the

circumpolar north could be similar to your studied location?

Line 94: This is a very important point which I think should be stated earlier in the introduction and given more emphasis.

Methods Line 133: How deep are those rivers? Can stream order be provided somewhere too.

Section 2.3 This is a nice way to work around flow measurement scarcity in this region. But should this section come after section 2.6?

Line 212: What threshold was used in the flow accumulation to delineate the stream lines and catchment boundary? Was it validated with the areal photos mentioned on line 155 or something else?

Section 2.4, shouldn't this section be called water chemistry analysis?

Line 245: Why not model the full carbonate alkalinity pool (HCO3+ CO3)? Arguably the CO3 pool is small at this pH range and shouldn't make a much difference to the model, but I find this conceptually more appropriate.

Results and Discussion

Section 3.1, 3.2 and 3.4 starts with a sentence stating how the author interprets DIC sources and cycling in each transect. This seems odd to me. I would rather the author takes me to that conclusion by presenting the results first.

Line 275: The Miller-tans analysis supports that as well. Again, this statement comes before presenting the key results.

Line 290: Is the term "geogenic" more appropriate?

Line 285: Probably right, but do you have any measurements or estimates of CH4 concentration at the source - in the groundwater? The CH4 might have evaded already, but its imprint on d13CO2 values might still be there.

C5

Line 301: That is also supported with the Miller-tans. But this biogenic soil CO2 source seems to prevail in other sites as well albeit with some mixing with the geogenic source. Could mixing between the H2SO4 weathering and biogenic soil CO2 be back calculated?

Line 314: This is also demonstrated in headwater streams at other latitudes and should be mentioned as well.

Line 314; Again I find this term "abiotic inorganic" to be vague. And what do you mean by dominates? Abiotic dominates biotic, or inorganic dominates organic?

Line 351: What does "amplified inorganic carbon cycling" means?"

Line 371: The term biotic is used here, but could the DOC be degraded photochemically as well?

Line 380: Is the model intended only to bring evidence to the fact that RTS increase alkalinity export or for a possibly larger modelling/budget exercises? Could you make use of that model already in this paper for a final "wrap up" exercise ?

Line 393: Dominate over what?

Line 405: This "conceptual model of land-freshwater linkages" needs more elaboration. As it stands, this model seems more like a list of DIC sources and sinks across this catchment than a generalisable model. A starting point would be to determine how does it integrates with other models at lower latitudes? How do changes in DIC sources and sinks caused by RTS integrates with other water chemistry properties and C species (organic vs inorganic) that were documented in other studies?

Line 410: What exactly was "striking"?

Line 414-418: How does this conclusion differ from the authors previous publication cited here?

Line 432: Should your model be used to that effect? If so, it should be stated.

C6

Line 443: " change for C cycle in the rapidly changing arctic landscape"

Authors Contribution What did D.O. do?

Figures and Tables

Table2: Should these values be presented in supplementary and only the model be presented in this table? I find the second part of the table easy to miss.

Figure 1: The map feels quite dense, can the photos and context map be placed outside? Could the bedrock lithologies be illustrated on the map?

Figure 2: The figures should be placed vertically rather than horizontally since they have the same x-axis. Also in caption, please clearly state that the top x-axis is for the dempster creek transect while the second is for the stony creek. Could the points for each transect be connected with a line for visualisation. Could HCO3 and CO2 concentration be on the same unit? Could there be a third 3 axis for the distance along the RTS runoff transect ?

Figure 4: Can you give a reference to these end-members

Figure 5: Open vs closed symbols would be clearer perhaps?

Figure 6 and 7: Should these two figures be merged with Figure 2? This would help draw a more complete picture of simultaneous changes in water chemistry along the transects. Why isn't there a similar figure for d13C-DIC values?

Figure 8: This is a nice schematic, but it limits the scope of the study. The schematic mostly lists the sources and transformation of DIC in this catchment. Does it only applies to this catchment, i.e. was the goal to map those processes, or can it be generalised to other catchments? I think this figure could be useful if it was to conceptualise the effect of RTS across scales, not just make a summary of all the processes identified in the data for this specific catchment. I have in mind something along the lines of Hotchkiss et al. 2015 NatGeo Figure 3.

Table A1. Why not keep the distance units the same and just add decimals for FM2 site.

Table A2: I find this could be useful in the main manuscript since it also provides a list of the DIC sources you are trying to separate.