

## Reviewer Comment 2

We would like to thank the reviewer for their careful comments, and also for this positive assessment of the manuscript.

### General Comments

The study utilizes experimental aerobic incubations of sediments taken from slump affected streams in the Peel Plateau to investigate if the potential of mineralization of slump derived POC varies from that of POC in non-impacted streams, and to quantify the biodegradability of slump POC fractions relative to their transport potential.

Several experiments involving samples collected over different sites and seasons. In 2015 samples were collected from sediments in streams near and within different slump sites to test if slumps affected the biodegradability of POC. Water samples were also collected above, within and downstream of slumps. Water samples UU unfiltered upstream (in situ POC) relative to filtered upstream water to which slump POC was added (SU). In 2016 samples of sediment were collected near the SE slump, upstream, in the slump, and downstream of the slump – to test variability in biodegradability with transport. In 2019 sediments were collected within and downstream of slump FM3 for follow-up experiments.

The authors conclude that there is minimal (4%) mineralization (oxidation) of POC over 1 month incubations. The authors propose that these low rates may be due in part to protection by adsorption to mineral particles. Additionally, the authors propose that the surrounding mineral rich tills promote inorganic C sequestration via chemolithoautotrophic processes.

This study involves the application of a carefully executed field sampling design, combined with carefully designed laboratory experiments and sophisticated analytical tools to address a very important knowledge gap in our understanding of the biogeochemical controls on the fate of particulate carbon released from permafrost thaw and disturbances. This is a study very worthy of publication. The methods are well detailed and documented, and overall the results are very well presented, although I have some concerns and suggestions about results. This is a very data/results rich paper.

My only substantive concern is the brevity of the conclusions. I feel the authors have missed the opportunity to really put these findings in to context. The authors show that these systems release and move a lot of carbon, but that the GHG emission potential is minimal – this is very significant, and should be discussed in the context of other work that suggests these abrupt thaw events might account for a large part of emissions from thawing permafrost - For example, the authors should discuss the meaning of their results in the context of the findings of Turetsky et al.'s Nature Geoscience, 2020 article (<https://doi.org/10.1038/s41561-019-0526-0>).

Thank you very much for these comments. We have now added a paragraph in our discussion section and modified our conclusion to place our findings within the larger context of permafrost

carbon release, including comparison to parameters used by Turetsky et al. (2020) highlighting that our highest estimate of 7% of TOC loss in this study is substantially lower than the estimate by Turetsky et al. (2020) that 2/3rds of DOC/POC from hillslope abrupt thaw will be mineralized. We compare our findings to the studies their estimate is based on and note that there are few studies that quantify rates of permafrost carbon mineralization within fluvial networks, and even fewer that include POC in these assessments. Therefore, current/early estimates of permafrost carbon feedbacks that include rates of fluvial C mineralization likely have a high degree of uncertainty and we recommend further studies, including POC and assessments of other carbon pathways, to create region-specific assessments of carbon release via hillslope abrupt thaw.

### **Specific Comments**

This was a remarkably comprehensive and carefully designed and executed series of experiments. Although, I really appreciate the very concise explanations of the experiments in section 2.2, the subsequent results (sample acronyms and experiments) were hard to keep straight, until I read through the supplemental and saw Figures S5, S6, S7. I strongly recommend including Figures S5, S6, and S7 (or maybe some reduced form of one or two these) in the methods section of the main paper. These are great illustrations. I realize that space constraints might make this an editorial decision, however I found these figures were critical to clearly communicating the methods and experimental design. These will also help the reader keep the acronyms for the samples straight.

Thank you! We can move the 2015 and 2016 supplementary flow diagrams into the methods section of the main text. An additional option is to have them in the appendix area so that they can still be referenced within the main text body but not taking up space before the results. However, we don't think that moving the 2019 experiment flow diagram into the methods is necessary because the design is a replicate of 2015 SD treatment (as mentioned in the text), only with the addition of the sterilization treatment, so it is quite simple.

Table 1 is a really great help for summarizing the findings, and the limitations. However, it is a bit difficult to follow in places, I suggest presenting this table in landscape format, so that it is not as crowded. Again I realize this may be an editorial decision, however some rearrangement or reformatting is required to really maximize the readability of this important table.

We have altered Table 1 to landscape format, also in response to Reviewer 1's comment.

### **Results**

In section 3.1 the authors report that the %change in POC is lower where the slump particles were added, and that this is likely due to the fact that the particle concentrations were so high in those samples that the % change is small. The % changes is potentially masking the importance of the magnitude the change in total mass of POC. It would be useful (more useful) to provide tables (or figures), in not in the main paper, then at least as a supplement that illustrates the changes in DOC, POC and TOC in terms of total g of C. Perhaps the point can be made at least in part by referring to the data as shown in Figure 3a and/or 3d for the DTOC.

Thanks for this comment. The percent changes are reported to assess the relative biodegradability. It is a common standardized metric used to assess biodegradability of organic carbon since larger initial organic carbon amounts would result in larger losses (Vonk et al. 2015). We do show absolute TOC changes relative to absolute oxygen changes in Figure 3 and we discuss it across all experiments and in reference to blank measurements in section 3.4.

Similarly, for the fractionated vs. unfractionated experiment. It would be helpful to show somewhere (e.g. Table B2) how the mass of C is distributed across the size fractions, to know where the greatest total C losses and gains are occurring, and thus to better interpret the % changes in terms of effect of size fractions on mass of C lost/gained. Perhaps this can be done in part by citing Figure 3b – which shows that the greatest change in C is due to the <20µm fraction?

We have modified Table B2 to show that most of the carbon within the sample that was fractionated is associated with particles less than 20 µm (the smallest size fraction) and have added this statement into section 3.3.

“The majority of POC (73%, Table B2) was associated with particles less than 20 µm.”

Section 3.4 does discuss the absolute carbon changes and we prefer it in this section because it compares the changes to oxygen changes.

The references to Appendices seemed odd to me. I am not familiar with Journals that support appendices, so I didn't even know where to look for them at first – I was happy to see they were at the end of the main document.. I think they definitely ought to appear in the main paper somehow – rather than in a supplemental -given that these data are very important in terms of the support they lend to the findings. The only exception might be the material in Appendix C, which could go in the supplemental if necessary.

These appendices were created following the guidelines on the Biogeosciences website, which allows for both Appendices at the end of the manuscript, and a separate online-only Supplement (see <https://www.biogeosciences.net/submission.html#manuscriptcomposition>). We felt the content in the Appendices were more technical details that detracted from the main point of the paper that we were not finding high biolability of organic carbon mobilized from slumps to streams despite consistently rapid oxygen consumption rates across a suite of experiments, which was a surprise to us and not what the experiments were originally designed for (hence the follow-up in 2019). Much of the content in Appendix A (ANOVA tables) and Appendix B (characterization of size fractions which we believe to be ancillary) are reported in the main text. We would prefer to keep it there since our main text will lengthen with the addition of the flow diagrams.

### **Technical Corrections**

Line 50: I suggest including some years to provide reader with more confined age of the Pleistocene age tills in this area, if known.

The tills were deposited as the Laurentide Ice Sheet advanced onto the eastern slopes of the Richardson mountains (Kokelj et al. 2017). We added in the period of maximum westward extent of the Laurentide Ice-sheet (c. 18 ka cal year BP (Lacelle et al. 2013)) and the point after which the Plateau likely was ice-free (13 ka cal year BP, Zazula 2009).

Line 52: Insert “the” after comma following “Thus, the relative..”

Done

Line 53: Delete “Variations in”, start sentence with “Source composition can also vary...”

Done

Line 56 : Since there are 4 sites on the map in figure 1, I suggest inserting the site names of the 3 sites in brackets in this sentence to clarify the sites sampled for this experiment “In 2015, ...within three slump sites (**HA, HB, HD**)

Done, also in response to Reviewer 1’s comment

Figure 1: label all panels in the figure. E.g. the map should be labelled as panel a) then the headwall units panel b); and the sampling site locations panel c).

Done

Line 78: it is unclear how much water was used, the serum bottles were 120ml, but does this mean you used 120ml of water + 2 ml of slump runoff? Insert sample volume to be clear how much headspace was left in the bottles. e.g. “we incubated <xx ml> unfiltered upstream ....”

The bottles were filled to have no headspace. This was highlighted in the supplementary but this detail is now added at the end of section 2.2.1 and 2.2.2.

Lines 85-87: The sieving process could use some additional explanation. It is not at all clear how such a small sample volume (0.5ml) could be sieved. Also were the fractions weighed? How did you know the mass of each fraction added (or the concentrations) of the final 60ml solution?

The fractionation process is detailed in supplementary S2 which has been slightly modified for clarity. The sediment concentrations (as highlighted in Figure 3e and Table S2) are abnormally high in this system. For the site where the sample was obtained (within slump SE) total suspended sediment concentrations can easily be upwards of 100 and 200 **grams** per litre. Thus, for these experiments (that were aiming to mimic downstream concentrations as shown in Table S2) we did not need to sieve more than 0.5 mL, which was still quite time consuming and difficult given the sediment load. We did not weigh fractions, we tried to approximate based on volume. The unfractionated treatment used slump runoff that was diluted to approximate downstream concentrations (Table S2) and so each fractionation was diluted to make the initial 0.5 mL used for fractionation up to a similar level of dilution. We tried to minimize loss of particles during the fractionation process but comparison of the sum of TSS across fractions to

the unfractionated shows there was error in the process (TSS sum across fractionations is 61% of TSS of unfractionated). However, we still think the relative proportions of material in different fractions is representative of the environment and this loss doesn't affect comparisons of % change which would compare the relative biodegradability of material in the different fractions (our initial aim of the experiment). Furthermore, the unfractionated control is presented alongside all results.

Line 106: Delete “First,” and start this sentence with “To assess...”

Done

Line 108: Insert the volume of sample used, so that water vs. headspace volume in the 60ml bottles is clear. “We incubated “**XX mL**” of sample in 60mL glass BOD bottles

We addressed this comment above by noting all bottles were filled to not have headspace. Comparison of concentrations to *in situ* concentrations are shown in Table S2.

Line 113: The bracket should include reference to equations 1a, 1b. “...(eqns. 1-4)...” x

References have now been added to each equation bracket, with references used below (for full refs see reference list at the end).

Equations 1a and b (Stumm and Morgan 2012)

Equation 2 (Klatt and Polerecky 2015)

Equation 3 (Percak-Dennett et al. 2017)

Equation 4 (Zolkos and Tank 2020, Calmels et al. 2007)

Line 114: Indicate the methods used to quantify N species and sulfate, and/or refer to citation or supplemental where this is explained at end of sentence.

This detail has now been added with reference to supplementary 3.3 which has been modified to provide more detail on the analysis.

Line 113: Since your goal as stated at the start of section 2.2.4 is to assess O<sub>2</sub> losses and OC gains, and since not all the equations (1-4) contribute to generating OC. You should insert “could consume O<sub>2</sub>” in this sentence. E.g. “...could **consume O<sub>2</sub>** and/or generate OC, ...”.

We have modified the sentence.

## Results

Line 141. I am not familiar with having appendices in journal articles. I suggest adding material from Appendix A to the main paper or the supplemental.

Please see our comment regarding this above

Line 145: It would be helpful to show the DSUVA254, in the paper or in the supplemental.

The change in SUVA is provided in Appendix C (Table C1).

line 160: Figure 2 caption, you say “measured (point) and modelled (line) O<sub>2</sub>” but there are no points visible in panels a-c.

We have amended the figure caption to show only modelled values are shown in Figure 2 (ease of readability in the Figure). Comparison of measure to modelled are shown in the supplementary (Figures S1 – S3).

Lines 185-191: Minor point, but you use lower case letters to identify the panels in Figure 3, yet in the text you cite Figure 3A, 3C etc. using upper case. I suggest that you should use lower case letters in the in text citations (Figure 3a, 3c...) to be consistent with the figures.

Done. If we missed any, we will re-address this during editorial reviews.

Line 202: Figure 3 caption, note the 1:1 line in panel (f) is solid vs. other panels where it is dashed. Is there a reason why this one is different? If so explain this in the caption, if not edit so that it is dashed as in the other panels.

Done

Line 215 and 218 – I suggest replacing “balancing to” with “resulting in”

We prefer “balancing to” because it is a balance

Line 218: Insert “in sterilized bottles” after TIC.

Done.

Line 223 and 227 – use lower case letters in reference to figure panels, so that these are consistent with how they appear in the figure.

Addressed above.

Line 228-230: I think this sentence requires rewording to clarify the message the authors are making. The authors suggest that the increase in simple compounds in sterilized samples “cautions against assuming the sterilized treatment is a true abiotic control of organic matter changes”. This seems to suggest that you are calling into question the fact that your sterile samples were truly sterile, which I don’t think is the intent. I think you mean to indicate that the changes in DOM could be entirely due to the sterilization process itself, hence the change in DOM composition of the sterile samples cannot be considered a “control” or “baseline” of the of DOM if there had been no biological activity in the samples. I suggest maybe a simple correction, delete

“PC1 separated DOM...proportion of simple compounds,” **Given that the sterilization process itself could increase the proportion of simple compounds**, the results caution against .... “.

We edited the sentence to read:

“PC1 separated DOM in sterilized and unsterilized bottles, suggesting the sterilization processes increased the proportion of simple compounds. Since the sterilization process itself appears to increase the proportion of simple compounds, the results caution against its use as an abiotic baseline.”

## Discussion

Line 261-262: ... indicate that CO<sub>2</sub> production ceased by the end of ...” this sentence requires a citation to support this statement.

The reference is Tanski et al. (2019) and has been added (see reference list below).

Line 282: you suggest that chemolithoautotrophy as a possible mechanism for counterbalancing OC mineralization. Can you discuss or provide evidence to support that these reactions are likely/possible in these environments - i.e. are the thermodynamics/redox conditions consistent with environments where these chemolithoautotrophic processes (organisms) are known to occur?

Nitrification is a common process in freshwaters (Stumm and Morgan 2012) but we do discuss the slow growth of nitrifying microbes and how the stoichiometry doesn't balance to the gains we see in experiments (see paragraph 3 of the discussion). In mine tailings pyrite oxidation has been found to be a notable carbon sequestration process (Li et al. 2019). The process is commonly associated with acidic-pH conditions, but it can also occur at circum-neutral pH (e.g. Percak-Dennett et al. 2015). However, sulfide oxidation and associated carbon sequestration is a very diverse and complex process (Klatt and Polerecky, 2015) so the stoichiometry can range quite a bit, but given the broad prevalence of sulfides across the Peel Plateau we think it's a mechanism worth exploring. A paragraph in the discussion (with some additional edits added) is dedicated to this.

## Conclusions

These findings of the low biolability of the permafrost POC are so important, yet the significance is not raised at all in the conclusions. These findings need to be put into context in the conclusions to better highlight their significance – as stated above especially with respect to Turetsky et al 2020. It seems to be broadly accepted that these large abrupt permafrost thaw events are likely to have strong positive feedbacks on atmospheric C and climate. Your study calls this into question – I think you need to highlight this.

We have addressed this comment and modified the conclusion as noted above.

## Supplemental Information

**Page 3, Para 2: line 4:** “Material **the** passes through “ replace “the” with “that”

Done

**Page 3, Para 2: line 6:** "... Material passed through the filter **discard** and..." should be "discarded"

Corrected to, "Material that passed through the filter was discarded..."

**Page 3, Para 2: line 9:** INSERT "from the 0.5 mL sample" between "particles" and "through the ..."

Done

**Section 3.3 paragraph 1 line 2:** a 0.7 micron filter is not standard for these analyses. Can you comment on why you used this pore size, and also what if any effect the larger pore size might have relative to standard measures?

This pore size is larger than the 0.45 micron pore size typically used for analysis of nutrients and trace metals. We filtered for PIC using a 0.7 GF/F filter (as required for carbon analyses) and then used the filtrate for dissolved analyses because we had a very limited amount of sample water available. A main point of concern with using a GF/F filter for chemical analysis, particularly for dissolved nutrients, might be greater bacterial activity since 0.7 microns may allow more microbes through than 0.45. This potential effect is likely limited because samples were immediately frozen and submitted for analyses within 2-4 days of collection. Further, we do not think the matrix would have changed substantially between filter sizes because colloidal organic matter (i.e. not truly dissolved material) still exists below 0.45 microns.

**Figure S5** – since you didn't use both time points, I suggest removing the one you didn't use. Also indicate the time of the timepoint (30 days?) on this and other figures or in the captions.

Data from timepoint 2 is available in the supplementary so we wanted to leave it in the figure for any interested readers. We now indicate in the caption that we focused our analyses on timepoint 1 for brevity, with results from timepoint 2 available in the supplement.

## References

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