Reviewer 1

General Comments:

This paper advanced knowledge regarding mobilized POC biodegradability as a result of Arctic thaw slumps. The identification of the rate of biodegradability of slump-mobilized POC answers an important piece of the lateral carbon flux puzzle in this region, making this paper very worthy of publication. Ultimately, this paper also answers the separate question by proxy, that there is a trend for slump-mobilized DOC to decrease during incubation despite the low biodegradability of POC and TOC. The knowledge gap is clearly stated, and the introduction does a comprehensive job of outlining the main question. The paper also detailed comprehensive experimentation over the course of many years in order to answer a series of related, nested questions. There are a few modifications and clarifications that I have outlined below that I believe would help to heighten the considerable impact of this paper's findings. I have broken up my main points into four bullets below. Line edits and more detailed questions follow in the Specific Comments and Technical Comments sections.

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

- Relative to what the degradation of organic carbon would have been had it remained frozen in the Arctic tundra, oxidative loss of 4% POC per month could be significant. At this rate, this accounts for a potential loss of 16% over the course of a 4-month growing season. When scaled up across the Arctic or scaled up over many years, this represents a considerable C degradation pathway. I believe it is important for the author to contrast this 4% loss with the alternative, had POC not been mobilized by thaw. For instance, in the absence of thaw slumps mobilizing this POC, can we assume negligible loss of permafrost organic carbon over the same timescale? The paper's tone does not do this impactful result justice. I would re-casting the significance for C cycling in contrast to the degradation rate in the absence of slump-induced C mobilization. Though the biodegradability may be low, it is still quite important at the rate of 4% per month.
  Thank you for the comments. We would like to clarify a few points.
  (1) The 4% loss is really a measure of TOC loss and we have clarified this at relevant points in the manuscript. This the highest degradation rate we measured for TOC and we believe it is primarily due to DOC loss (now clarified in the abstract). In many experiments we did not see any significant loss of TOC, and in some cases a gain.
  (2) Furthermore, this 4% loss rate is what is measured under oxygenated conditions with particles maintained in suspension. Thaw slumps within our study region mobilize material far in excess of what the stream can transport (Shakil et al. 2020; Kokelj et al. 2021) which will substantially reduce the ability of organic carbon to oxidize this material.
  (3) It is unlikely that the oxidation of TOC scales linearly across time. Degradation rates tend to decrease over the course of an incubation, typically resulting in a "levelling-

off" of the carbon dioxide produced or the organic carbon consumed (see, for example, the log-associated calculation of k as a reaction rate coefficient, for many studies on decomposition). We were unable to quantify this change in rates in our experiments because we used end-point organic carbon measurements. We could not use our high-resolution oxygen measurements to quantify this as a proxy since we believed that processes other than organic carbon degradation were significantly consuming oxygen (as highlighted in the paper). Thus we used our best proxy of the most similar experiment conducted in the most similar terrain (the work by Tanski et al. 2019, which was conducted just north of our study site, on the similarly glacially conditions Herschel Island) to determine our estimate of 7% of TOC mineralized during riverine transport.

(4) We highlight, now with edits, "These findings are consistent with generally lower CO_2 fluxes and concentrations downstream relative to upstream of slumps (Zolkos et al. 2018, Zolkos et al. 2019), despite orders of magnitude increases in POC and TOC (Shakil et al. 2020)."

(5) We feel that using 0% mineralization as a point of reference may be a bit absolute considering that some microbes can adapt to mineralize organic carbon within permafrost (Leewis et al. 2020) and the large losses of CO_2 that can occur during the winter in the Arctic (Natali et al. 2019). However, we do now note that this mineralization may be elevated relative to mineralization occurring within the frozen conditions of permafrost but also note that our estimate is considerably lower than the estimate used by Turetsky et al. (2020) that 2/3rds of DOC/POC from hillslope abrupt thaw will be mineralized and highlights a need for region specific estimates of permafrost carbon release (also added to address comments from Reviewer 2)

- This paper includes many great experiments; however, the conclusion is a little truncated. Echoing point 1, the conclusion is a good place to reiterate the significance of the main finding, of 4% POC loss per month.
  We don't believe stating a loss of 4% POC per month is accurate, please see comment above. We have now modified the conclusion (with some edits in response to comments from Reviewer 2) to add more detail regarding our findings and their relevance to permafrost carbon release.

- Further, I would suggest taking a stronger stand on each of your experiments and the text of the discussion, where you parse out the relative importance of each of the potential POC sequestration pathways (abiotic and biotic). The conclusion would be improved if it included more regarding the vulnerability of this mobilized and subsequently sequestered C. Will the sequestered C be vulnerable to a faster rate of decomposition? What is the most important sequestration mechanism in the second to last sentence (L308)?
  Thanks for these comments. We dedicate a paragraph in the discussion to discuss pathways of chemolithotrophy and a paragraph to discuss sorption, the effects of sorption on DOC biodegradability, and potential conditions in the stream system where adsorbed DOC could be released. We do not feel we have enough supporting evidence quantifying

these other two processes to make firmer statements than contained in our original text and also don't feel we have any basis to estimate the rate of decomposition of sequestered carbon. Our purpose was to highlight that these processes may be significant in typical experimental designs used to quantify degradation rates and need to be accounted for and to encourage future research to explore these avenues.

- The supplemental flow charts (S5-S7) are incredibly useful in aiding the reader's understanding of the experimental design. Within the text itself, the location/code of individual slumps and the treatment codes distract the reader from the main findings beyond Figure 1, which orients the reader to each slump location and code. In general, I am curious why the authors did not combine the slumps that were similar in their analyses, treating them as replicates of one another (with the exception of the slump with some encroachment reported). As a general suggestion overall, if it is possible to remove all reference to specific site acronyms (rather, refer to each site as site 1, 2, 3, etc.) and to refer to the treatments with complete description, e.g., "unfiltered upstream" rather than by acronym "UU", I believe the text clarity and readability would be greatly improved. This is a minor update that I believe would have a major impact.

(1) Thank you for the comment on the flow diagrams. We are glad to hear that the reviewer found these to be useful. We are happy to move the flow diagrams associated with the 2015 and 2016 experiment to the main text (or appendix associated with the main text, also in response to comments from Reviewer 2). Since our 2019 experiment was simple and really a replication of the SD treatment in 2015, with an added sterilization, we think it's best to keep this diagram in the appendix for brevity. With respect to slump codes, we have chosen to retain these as-is because these sites have been the subject of multiple investigations. As a result, we would like to provide readers with the tools (i.e., clear reference to existing slump codes) to compare our results with those from previously published studies. Although we acknowledge that the treatment codes do add some complexity to the manuscript, we provide Table 1 and Figures S5-S7 (which can be moved to the main text / appendix) as a quick reference guide to help alleviate this concern.

(2) There was large variation in rates of oxygen consumption between slump-affected treatments between sites (Figure 2 a-c). We believe this is largely associated with large variations in sediment concentration which are difficult to control (Table S2). We wanted to keep the slump sites separate so that the differences between treatments wasn't lost within the variation between sites and it also is a truer representation of our experimental design.

(3) In the text we do primarily use the text description to refer to treatments but kept the acronyms in brackets for ease of reference to figures since there is not enough space in the figures to put the complete text description. With movement of the flow charts to the main text we hope that these acronyms will be easier to follow.

- <1 week incubation times seem very short. For soil incubations, this short time would be considered a disturbance measurement since there are artifacts from handling and setting up an experiment in conditions away from the field. Furthermore, POC from older carbon permafrost soil (as evidenced by radiocarbon age) would likely have a slow turnover time, which by nature takes longer to measure rates. Please add some visible text to the discussion and conclusion that the short term experiments might be limited in both detecting the actual rate of change (Type II statistical error), and the role that the novel lab conditions may play during this time period.

(1) The experiments range from 7 to 27 days. However, while these time frames may be short for soil experiments they are not uncommon in the aquatic literature (Vonk et al. 2015, Richardson et al. 2013). In Tanski et al. (2019), the experiment we compare to, ~58% of the total CO_2 production occurred within 27 days. In Spencer et al. (2015), a study conducted in a region where high biolability rates have been noted for DOC, they found ~50% DOC loss in <7 days). Thus, we have used these studies to frame the timespan of our incubation, and also chose to err on the side of shorter incubations to avoid issues related to "bottle effects" (e.g., Vonk et al. 2015). Further, we do have text (line 256 – 265, "Our finding~~s~~ of low POC biodegradability is likely conservative…") discussing the short time frame of our experiments and estimating what the percent loss would be if we extrapolated beyond the time frame used (see also responses above).

(2) Older radiocarbon age doesn't always equate to slower turnover time for permafrost soils. Cryoturbation, low temperatures, and poor drainage over long time periods can result in carbon becoming sequestered in frozen soils with minimal exposure to degradation, although this varies across landscapes based on permafrost history, geology, etc. (Tank et al. 2020). This is highlighted in papers that have noted relatively higher biodegradability for relatively older carbon via experiments or lipid proxies (e.g., Spencer et al. 2015, Bröder et al. 2020).

Specific Comments:

This paper is primarily focused on POC not POC and DOC, however, the ultimate findings suggest that POC fractions studied have lower biodegradability than DOC and I believe that contrasting the two broader size classes of stream OC and how they may interact could be of use given the ultimate findings (e.g., increased POC mineral input into streams has the potential to increase DOC sorption).

In the last section of the discussion (lines 289 – 305) we contrast the differences between POC and DOC, highlight interactions, but also highlight complications in quantifying different mechanisms with these experiments. We have adjusted the abstract to highlight that most of the TOC loss appears to be due to loss from the DOC pool and have also added in further text in the discussion to contrast previous findings on lability and sources of the two pools.

I also believe that POC and DOC transport is an important aspect of lateral carbon fluxes worthy of mentioning early on in the abstract, albeit briefly. Transport of carbon is the initial mechanism that allows for mineralization into CO2 and re-sequestration into sediments.

We modified a sentence in the abstract and introduction to make it clear that POC is the primary mode through which organic carbon is mobilized to streams in landscapes affected by hillslope thermokarst disturbance.

Abstract:
"Warming and wetting in the western Canadian Arctic is accelerating thaw-driven mass wasting by permafrost thaw slumps, increasing total organic carbon delivered to headwater streams by orders of magnitude due to orders of magnitude increases in particulate organic carbon (POC)."

Introduction:
"Thaw slumping along stream sites in this region can increase TOC yields by orders of magnitude almost entirely due to orders of magnitude increases in POC yields (Shakil et al. 2020)."

L11: Mineralization as CO2 and sedimentation are two POC fates, but this sentence does not address re-sequestration of stream C by aquatic plants or transportation downstream (though transportation is not an ultimate, chemical fate). I believe 1) it would be useful if the abstract jumped right into POC as this is the primary focus of the paper's research OR 2) for the abstract to include mention of transportation as the mechanism allowing for soil organic carbon to become transported POC, mineralized CO2, or re-sequestered sediment within stream systems.

Suggestion 1: "Upon thaw, permafrost particulate organic carbon (POC) may be mineralized into CO2…"
Suggestion 2: "Upon thaw, permafrost carbon entering and transported within streams may be…"

Following suggestion 2 we have added "and transported within".

L30-35: Transport is covered in this section, I believe it should be mentioned in the abstract, briefly as is presented in Specific Comment #1 above. The dichotomy of fates as it relates to the transport trajectory (transport vs deposition according to size and density fractions) is ultimately relevant to the study findings.

We incorporated suggestion 2 for the comment above to address this.

L32: It is probably worth mentioning that anoxia reduces overall mineralization rates but also shifts carbon loss towards methane (Schaedel et al. 2017 Nature Climate Change)

We have incorporated this into the sentence:
"…(Peter et al. 2016), though carbon release can shift to be in the form of methane (Schaedel et al. 2017)."

L40: Might be helpful to discuss different sources of POC in slump affected- and non-affected streams so reader can understand why lability might go up/down.

We have modified the sentence to read,
"Slump-POC chemical composition suggests lower bioavailability as POC sources shift from the active layer and some periphyton material to Pleistocene-aged organic carbon and petrogenic organic carbon mobilized from permafrost (Shakil et al. 2020, Bröder et al. 2021). However, POC bioavailability has not been experimentally assessed."

L127: circumneutral-pH, in my experience, pH of many Arctic water tracts is closer to pH5 than pH7.

We note that pH in the streams in this study can be quite variable but tend to be circumneutral, often varying around pH7 and most ranging from pH 6 – 8 (see supplementary data of Shakil et al. 2020). This text has now been added.

L190: for clarity, identifying SE particles as slump SE would be useful and parallel HA slump particles later in the sentence. However, see point #3 in the general comments.

We have added the term "slump".

L184: most organic matter is partially oxidized because it has oxygen molecules. For example, glucose has a lot of oxygen molecules. Would this line be expected to be a 1:2 line instead of a 1:1 line? Most organic matter has oxygen as a part of it, does this change the heterotopic respiration line of 1:1?

Thanks for this comment. The net balance of the aerobic respiration of glucose is represented as:

$C_6H_{12}O_6 + 6O_2 \rightarrow 6CO_2 + 6H_2O$,

thus, the complete aerobic oxidation of glucose is results in a respiratory quotient of 1, (on balance, the CO_2 produced is equivalent to the moles of O_2 consumed). This ratio has been documented for the aerobic respiration of glucose in environmental work in soils and litter (e.g., Dilly et al. 2001). Although compounds oxidized in aquatic systems certainly vary beyond glucose (as noted in Berggren et al. 2012) and thus the respiratory quotient for bulk organic carbon can vary, we chose to represent a 1:1 line given the long history of an assumed respiratory quotient of 1.0 for heterotrophic respiration in aquatic systems, and measured heterotrophic RQ values close to this value in boreal systems (e.g., see Berggren et al. 2012). Further, we note that this doesn't impact our commentary with reference to the figure since even a 1:2 line would still be trending in a different direction that our data.

L250-255: Some DOC may be decreasing as it is converted to CO2 alongside consumption of O2 as shown in Figure 3F and mentioned in L215. I would propose DOC declines as a possible reason for O2 consumption mentioned in L250-255.

Treatments containing particles always had elevated oxygen consumption rates relative to filtered controls for both upstream and downstream water so DOC mineralization can't account for the elevated oxygen consumption. We have added in a sentence to clarify this. Further, we note that our abiotic experiment shows high potential for abiotic consumption of oxygen.

"Despite a lack of TOC or POC loss, oxygen consumption rates in treatments containing particles were always elevated relative to their DOC controls, highlighting that oxygen consumption could not be solely accounted for by DOC mineralization."

Figure 4: Please note, MQ water has been found to carry a baseline amount of DOC, typically below the standard detection limits of a TICTOC but enough to impact radiocarbon measurements (0.5 ppm) if MQ water is used to generated standards.

The Milli-Q water we use has a carbon filter and is quality controlled to be less than 10 ppb TOC so we assumed it negligible compared to the amount of carbon that may be introduced during the experimental set-up. We have added in this detail in section 2.2.4,

"MQ water was sourced from a machine with a carbon filter and was quality controlled to have less than 10 ppb TOC."

Supplemental Information:

Page 3: Do you suspect that the varying incubation timing (7, 11, 8, and 27 days) has any impact on the resulting POC degradation?

It's possible, but it is difficult to disentangle the effect of variation in time with variation in experimental set-up and slump sites We have added a note on this in the discussion.

However, we think the gains detected in organic carbon, even within the shorter incubation times of 7, 8, and 11 days (Table S4) may still highlight an important carbon sequestration process that should be further investigated. Though experimental duration of 7 days is short for soil experiments they are common within the aquatic sciences (see responses above and Vonk et al. 2015)

Unresolved general question: How did you store your samples prior to analysis? How many days were they stored once collected, were they refrigerated, frozen, or acidified? Were they stored in the dark? These questions impact the ultimate degradation of the C within the samples.

Much of this was addressed in the supplementary but we have modified and moved text to the main text for clarity as noted below:

(1) For samples used for the experiment set-up:
Now in section 2.1 (region and field sampling) in the main text – "All samples were processed (i.e., filtered) within 24 hours of collection, apart from within-slump and downstream samples used for adding particles to "unfiltered" treatments in 2016 that were stored in the dark at 4°C until the start of the experiment. Experiments were started within 24 (2015, 2019) – 48 (2016) hours after processing (thus 48 – 72 hours after field collection, details per experiment below). The extra hold time for the 2016 experiment was due to the extra time needed for size fractionation of samples (see below and supplementary S2)."
(2) For samples stored for analysis after removing from the experiment details are in supplementary S3 for each analyte. If missing, details on storage until analysis were added. Storage and analyses of sensitive analytes (e.g., DIN) were conducted following the tested methods of the Canadian Association of Laboratory Accreditation (CALA)-certified Biogeochemical Analytical Service laboratory.

Technical Corrections:

Table 1: In my copy of the manuscript, Table 1 text is too large for the cells, with overhanging letters in the first four columns.

We have altered Table 1 to be landscape format.

L56: 1) removing the slump site identifiers entirely from the text regarding the 2016 and 2019 experiments or 2) Identifying which three slump sites were used (HA, HB, HD) in 2015 would be useful for the reader and would mirror the identification of slumps SE and FM3 in the 2016 and 2019 experiments in line 61 and 62, respectively (see comment on L59, below). However, see point #3 in the general comments.

On line 56 we have added "…. three slump sites (HA, HB, HD)..." to specify the sites used in 2015.

L59: Site HD-UP is introduced in the text before the reader is oriented to what site "HD" represents; supplemental Figure S5 does not portray HD-UP, I believe this should be updated to Figure S4 and HD could be introduced in Line 56 as mentioned above. However, see point #3 in the general comments.

With the addition of the slump names for 2015 (see response above) we think this will be more clear since we introduce the UP, IN, DN acronyms in lines 58-59. We also adjusted the Figure 1 caption to indicate that HA, HB, HD, SE, and FM3 are slump sites. This note was here for later reference of why HD upstream treatments were more similar to slump sites than HA and HB (line 153).

Figure 1: It would be beneficial to the reader to identify slump SE on the larger map as well as in the map inset (slumps HB, HA, FM3, and HD are all identified on the larger map, but SE is missing).

The transect for SE was highlighted on the main map. We have now added in a red dot (similar to the other slump sites) on the main map for the location of SE.

Indicating that SE, HB, HA, FM3, and HD are slumps on the map key would be useful.

A notation in Figure 1 has been added.

Within the inset, SE-IN is identified. Should UP, DN-1, and DN-2 also be described with the SE- prefix in the inset?

Inset b of Figure 1 has been modified to explain the UP, IN, DN abbreviations (upstream, within, downstream).

I would suggest labeling the entire inset as the slump SE transect and omitting the label SE- from the IN location. However, see point #3 in the general comments.

We have labelled the entire inset as the 2016 transect experiment.

L81-83: The settling component of the 2015 experiment is distinct from the 2015 incubation experiment. I believe this would be best organized in a subsection, rather than grouping the incubation and settling together by year in one paragraph, as variation over year is not a factor of interest in the overall paper.

Thank you for this comment. We did consider this, however, we wanted to maintain results within each experimental set-up since each experimental had a different design for different goals (as summarized in Table 1). The results for settling are already presented in a separate subsection ("3.2 Experiment 2015: Effects of background dissolved constituents and settling"). The years are presented for organization purposes, matching with field collection, and so it's clear that the experiments were conducted in different years.

L85: Slump SE is referred to in this section however the 2015 sites were not mentioned by name in the previous section (2.2.1). I'd recommend consistency between the sections. However, see point #3 in the general comments.

We have addressed this, see comments above.

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