Biogeosciences Discussions

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# Comment on bg-2021-150

Anonymous Referee #1

Referee comment on "Low biodegradability of particulate organic carbon mobilized from thaw slumps on the Peel Plateau, NT, and possible chemosynthesis and sorption effects" by Sarah Shakil et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2021-150-RC1, 2021

**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.

- 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.
- 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. 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)?

- 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 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.

**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). 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 $CO_2$ and re-sequestration into sediments.

L11: Mineralization as $CO_2$ 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 $CO_2$, or re-sequestered sediment within stream systems.

Suggestion 1: "Upon thaw, permafrost particulate organic carbon (POC) may be mineralized into $CO_2$…"

Suggestion 2: "Upon thaw, permafrost carbon entering and transported within streams may be…"

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.

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)

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.

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

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.

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?

L250-255: Some DOC may be decreasing as it is converted to $CO_2$ alongside consumption of $O_2$ as shown in Figure 3F and mentioned in L215. I would propose DOC declines as a possible reason for $O_2$ consumption mentioned in L250-255.

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.

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?

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.

**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.

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.

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.

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). Indicating that SE, HB, HA, FM3, and HD are slumps on the map key would be useful. Within the inset, SE-IN is identified. Should UP, DN-1, and DN-2 also be described with the SE- prefix in the inset? 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.

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.

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.