Biogeosciences Discussions

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Comment on bg-2021-150

Anonymous Referee #2

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-RC2, 2021

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 or 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) or 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).

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.

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.

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.

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 <20um fraction?

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 exeption might be the material in Appendix C, which could go in the supplemental if necessary.

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.

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

Line 53: Delete "Variations in", start sentence with "**S**ource composition can also vary…"

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**)

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).

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 …."

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?

Line 106: Delete "First," and start this sentence with "To assess…"

Line108: 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

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

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.

Line 113: Since your goal as stated at the start of section 2.2.4 is to asses O2 losses and OC gains, and since not all the equations (1-4) contribute to generating OC. You should insert "could consume O_2" in this sentence. E.g. "…could **consume O_2** and/or generate OC, …".

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.

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

line 160: Figure 2 caption, you say "measured (point) and modelled (line) O2" but there are no points visible in panels a-c.

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.

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.

Line 215 and 218 – I suggest replacing "balancing to" with "resulting in"Line 218: Insert "in sterilized bottles" after TIC.

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.

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 …. ".

Discussion:

Line 261-262: … indicate that CO_2 production ceased by the end of …" this sentence requires a citation to support this statement.

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?

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.

Supplemental Information

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

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

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

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?

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.