Biogeosciences Discussions

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Reply on RC1

Flora Mazoyer et al.

Author comment on "The dominant role of sunlight in degrading winter dissolved organic matter from a thermokarst lake in a subarctic peatland" by Flora Mazoyer et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-26-AC1, 2022

Answer to comments from Reviewer 1

We thank the reviewer for his (or her) time evaluating the paper and for his (or her) precise and well-argued comments. Please, see our answers below.

Shortcoming 1: *The authors aimed to test the reactivity of DOM after ice-off in a lake, but had an altered DOM composition and possibly different bacterial community composition during their experiments compared to the water originally present in the lake. It is likely that the changes in DOM composition (shown by the substantially lower SUVA254 value and higher Fmax value) measured between the initial water sampled (Table S1) and water at the start of the experiments (Table 1) were due to bacterial mineralization of DOM during the 4-month storage period of the unfiltered lake water. The authors found that fluorescent DOM increased with bacterial production during their experiment. Then, previous work from these authors showed that bacteria in similar lakes degrade aromatic DOM (Laurion et al, 2021).*

Many papers have shown how bacterial community composition and activities change to adapt to new DOM compositions (Crump et al, 2003, Dinasquet et al, 2013, Logue et al, 2016). If bacterial community composition did change to adapt to the altered DOM composition, then the bacterial production and mineralization measured during the experiment does not reflect what is taking place in the lake water. I think that the authors should include these limitations in their discussion because they impact the authors' conclusions on how much sunlight and bacteria can mineralize DOM to CO2 following iceoff in thermokarst lakes.

REPLY: We first want to underline that lakes at SAS site are very different from the ones at Bylot site (Laurion et al., 2021). Moreover, this study on Bylot was done on DOM collected after a dry period in July, but see reply to shortcoming 2.

We agree with reviewer 1 that this delay, due to logistical reasons, is not ideal, and that this water is not completely representative of the DOM and bacterial community at ice-off. We already underlined in the manuscript that winter-like conditions (~4°C, darkness, no oxygen) were artificially prolongated by 2 months in the refrigerator as compared to the ice-break occurring mid-May (Matveev et al., 2019), but we clarify this point further (lines 65-66, 105-118, 519-534). For example, we know that under natural conditions, phototrophic production can start again as soon as light is becoming more available under

the ice, changing the DOM pool and the bacterial community before spring melt (Bertilsson et al., 2013). The purpose of the study was to explore the degradation potential of winter DOM pool that is rich in recalcitrant carbon after a long dark period without external inputs, rather than the degradation potential of spring DOM pool that can already be enriched in autotrophic DOM and other inputs from the ice-melt period.

The decrease in SUVA and increase in FDOM seem to have indeed continued during this prolongated period, but we think this should not be considered as an experimental flaw. Moreover, we think that changes in bacterial assemblages were already adapted to a DOM pool with limited lability after more than 5 months under an ice cover (from the ice formation mid-October to sampling end of March). We agree that the community has likely changed from in situ conditions in March until T0 of the experiment, but more because of its transfer into a container than because of a prolongated winter. We assumed that most changes would have occurred relatively quickly after water collection, and that prolongating by 2 months would not change much on the story. Any experimental set up has effects on microbial assemblages, and only a few people doing experiments can overcome such effects if the logistics is simpler. We see no reason why the DOM-bacteria tandem would evolve that differently in the container than it would evolve under the ice in the deep of the winter. The main structuring physico-chemical conditions were very close: 4°C, darkness, no oxygen. These are likely the most important characteristics defining the habitat and influencing the metabolism (Bertilsson et al., 2013; Jansen et al., 2021).

The literature proposed by reviewer 1 mainly points out how organic matter source and quality drive shifts in bacterial communities. For example, Crump et al. (2003) noted that there are persistent populations throughout the year and that the transient populations are linked to spring terrestrial inputs and summer phytoplankton development. Actually, the results of this work even suggest that, in the absence of a strong perturbation in the DOM pool (strong inputs suddenly coming or suddenly stopping), changes in the bacterial community should be limited. Between water collection and the start of the experiment, the collected water did not receive any DOM inputs, so we think that changes in the bacterial community were limited, as discussed in the proposed articles.

Shortcoming 2: *The authors did not compare their findings to Laurion et al. (2021), which studied DOM mineralization in other lakes in the region with a similar experimental design. Because Laurion et al (2021) conducted the sunlight exposure and bacterial incubation experiments sooner after water sample collection, the authors could benefit from discussing how their results compare and how they might be due to differences in the experimental design alone (not due to different study sites).*

REPLY: We do not think that these two studies are easily comparable. These ponds have in common to be wetland thermokarst systems, but there are many other differences. Bylot Arctic ponds (73°N) from Laurion et al. (2021) are diverse, with clear to brown-colour and DOC content generally around 10 to 13 mgC L^{-1}. Local soil is perennially frozen (continuous permafrost), which limits hydrological exchanges. On the other hand, SAS ponds (like SAS2A in our study) are subarctic ponds (55°N) lying in ombrotrophic bogs, with ice remaining only in the heart of palsas (sporadic permafrost). Waters are black-coloured and much richer in DOC (~22 mgC L^{-1} here). Surrounding unfrozen soils are flooded, which facilitates exchanges. Soils from Bylot and SAS do not have the same age nor the same history, and the surrounding vegetation is different. At last, the experiment from Laurion et al. (2021) was made on summer water, containing DOM inputs from phytoplankton, macrophytes and the surrounding land (although after a dry period with limited inputs). On the contrary, our experiment was done on winter water, deprived from summer inputs, which makes a very big difference. The novelty of this study has been further underlined (lines 64 and 67-68).

Lines 18-21*: It is incorrect to report that 18% of DOC was directly lost over 18 days of*

sunlight exposure when the higher abundances of bacteria later in the experiment could have contributed to that mineralization. The authors do a good job discussing how much DOM mineralization could have come from sunlight and bacteria, so this should be clarified in the abstract too.

REPLY: Thanks to the reviewer for bringing this up. We have adjusted the text accordingly stating that "Up to to 18%" was lost and we have made it clearer that there had been a bacterial regrowth (lines 18 and 20-22).

Lines 106-108*: The sentence "Indeed, DOM variables … and aromaticity)" needs to be revised to accurately report the results. Tables S1 and 1 show substantial changes in DOC, SUVA254, and the fluorescent DOM components during the 4-month storage period. Statistical tests should be used to report the statistical significance of those differences.*

REPLY: Unfortunately, these differences cannot be statistically tested because we do not have the adequate replication for that. The data give an idea of the qualitative change that happened, but we agree that a quantitative appreciation of these changes was missing. We now report more accurately these changes and added more justification as mentioned above (lines 108-118).

Table 1*: Define all of the abbreviations in the caption.*

REPLY: We redefined explicitly the treatments in the caption as suggested.

Lines 144-145*: Can you cite a paper where this is reported?*

REPLY: We added a reference were the same type of bottles have been used to carry out photodegradation experiments and showing Teflon bottle transmittance spectra (lines 157-159).

Line 483*: It is not accurate to report that the DOM in the lake prior to ice-off is refractory to biodegradation when there were 4 months before the experiment when bacteria could have been mineralizing the most labile DOM to CO2.*

REPLY: When the water was sampled at the end of March, it was already after more than 5 months of winter conditions, but it is true that we cannot exclude the fact that there could have been labile molecules left by then. However, we do consider that this sentence is accurate considering that it derives from the biodegradation experiment results (and we say "the data suggest"). Nevertheless, we now address the point raised by the reviewer lower in the discussion (next paragraph; lines 519-534).

In general*, I think the manuscript would read better if each treatment were spelled out rather than abbreviated.*

REPLY: If possible, we would like to maintain these intuitive abbreviations (L for light, B for bacteria), since they help to make the text lighter.

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