

Dear Helge Niemann,

We thank you for your evaluation report. We modified the manuscript to remove any remaining ambiguity raised by Reviewer 1. Please find our answers below.

Best regards,

Flora Mazoyer, Isabelle Laurion and Milla Rautio

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### Answers to comments from Reviewer 1

Dear Reviewer 1,

We thank you for taking the time to review our work a second time. Line numbers in the new version of the manuscript (version3) are specifically indicated in blue, while they are left in black when it refers to the previous version (version2).

**Shortcoming 1:** Thank you for discussing how the 4-months storage period could have impacted DOM composition and bacterial community composition during the experiments in the methods and discussion. While this is now mentioned in the main text, there is a lack of data to support the conclusions written in the manuscript.

- Lines 106-109: Without measurements of the lake water chemistry and bacterial communities at ice-off (30 May), there is no way for the authors to know whether these variables were similar in their water sample after 2 months. There are no data supporting the authors assumption that there was only a 2-month delay (instead of the actual 4-month delay) between water collection and the incubation experiments. Thus, any mention of the 2-month delay should be removed from the manuscript.
- Lines 524-525: How can the authors confirm that there was no detectable change in DOC concentration between water collection (19.2 mgC L<sup>-1</sup>) and T0 (22.1 ± 0.7 mgC L<sup>-1</sup>) if statistical tests are not performed? A 15% increase in DOC concentration is substantial and suggests microbial activity is taking place (either from the active release of exometabolites or the decay of dead microbial cells as community composition shifts). Without the statistical tests, the authors have no way to prove that the change in DOC was not statistically significant and thus need to discuss possible causes for this change.

**REPLY:** You are right that we lack data demonstrating that changes naturally happening under the ice between March (water collection) and May (ice-off) are similar to the changes observed during the storage period. We still think a 4-months delay should not make much difference with a 2-months delay and could not invalidate or bias our conclusions, but we can only assume changes happening in the refrigerated container were similar to natural changes happening under the ice. We modified the discussion to be more accurate about what is supported by our data.

Specifically, about lines 106-109: We erased all mentions of the 2-months delay to remain factual, as suggested. Therefore, we erased the sentence previously at lines 106-109, and we modified the text at lines 519 (522) and 533 (538).

Specifically, about lines 524-525: We did not say there was *no detectable change* in DOC concentration between water collection and T0, but that there was *no DOC decline*. We cannot properly test this statistically because replicate samples were not taken on the sampling day. This sentence was intended to underline the absence of any observable DOC decline over the storage period (i.e. potentially limited conversion of DOC into CO<sub>2</sub> or biomass), supporting the hypothesis that winter DOM was relatively biorecalcitrant. We are unsure what caused the DOC to increase, but agree that increasing FDOM suggests that some microbial activity was taking place (see lines 530-532). Therefore, bacterial activity may have been responsible for part of this DOC release, but it would need to come from the particulate fraction (POC). Alternatively, DOC could have leached naturally from POC, without the action of bacteria. The collected winter water was indeed containing particles and flocs visible to the naked eye, but this was not quantified. The carbon released into the dissolved fraction needs to come from somewhere, and it is very unlikely that ~2 mg L<sup>-1</sup> of DOC would have originated solely from already-present bacterial cells alone (as exometabolites or dead cell senescence). We thus propose that it would originate from the POC fraction, and reworked the paragraph accordingly (lines 527-530).

**Shortcoming 2:** Although the study site in Laurion et al. (2021) was much different from the lakes in this manuscript, it would still be helpful to compare their results to Laurion et al. (2021) given that there are not many measures of DOM processing in wetland thermokarst lakes.

**REPLY:** After rethinking the question, we agree with you because these two sites have enough in common to bring a meaningful comparison, which we added in the photodegradation part of the discussion (lines 457-458) and in the biodegradation parts of the discussion (lines 546-548 and 581-582).

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## Answers to comments from Reviewer 2

Dear Reviewer 2,

We thank you for your time reviewing our work. Your suggestions of language improvements were very much appreciated.