

Biogeosciences Discuss., author comment AC3  
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## Reply on CC1

Flora Mazoyer et al.

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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-AC3>, 2022

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### Answer to comments from Dr Liudmila Shirokova

We are grateful to Dr Liudmila Shirokova that reviewed our manuscript, additionally to the two reviewers appointed by the editor. We thank her for her positive judgement and answered all her relevant comments below.

***In the Abstract***, there is some self-contradictory: 18 % efficiency (L 18) is not full mineralization of the DOM pool.

**REPLY:** We erased the term "full" in the sentence, which was probably leading to confusion (also brought by reviewer 2). In the first sentence we want to express that 1) sunlight drives the transformation of the DOM pool, and 2) part of this DOM pool ends up mineralized to CO<sub>2</sub>. In the following sentence, we are entering into treatment descriptions. We simply start by expressing that 18% of the initial DOC disappeared in the light treatments. We hope this is now sufficiently clarified.

***L18 & L 24-25*** again, half loss and full mineralization are not the same things.

**REPLY:** We hope that the deletion of the term "full" clarified the apparent contradiction.

***L42*** Here, a reference is needed. Note that sediment respiration, soil input and groundwater discharge are also important drivers."

**REPLY:** We added two references to support this (line 44). We agree that inputs from soils and groundwater discharge may also be an important source of CO<sub>2</sub> to pond water, but we prefer not to distract the reader with these aspects here.

***Methodology:*** The change of redox conditions between sampling and storage – partial oxidation after aeration during sampling should be discussed.

**REPLY:** Unfortunately, we did not follow dissolved oxygen concentration in the water after collection, neither during storage time. Dissolved oxygen may have slightly increased at sampling, although water was gently collected with a thin-layer sampler connected to a peristaltic pump (Matveev et al., 2019). However, results from Folhas et al. (2020) suggest that the study lake (SAS2A) is particularly difficult to oxygenate: ice drilling in other lakes of the site caused oxygen concentration to increase in the surface layer to

20-25 mg L<sup>-1</sup>, but it did not go above 3 mg L<sup>-1</sup> in SAS2A. Also, since the container was full and kept closed in the cold chamber until the experiment started, we think storage conditions were similar to lake conditions in terms of anoxia. We added these arguments in the revised manuscript (lines 111-118).

**Table 1** Please add the pH value and specific conductivity.

**REPLY:** Unfortunately, these variables were not measured during the experiment, only in situ.

*Detailed description of experimental setup is highly appreciated!*

**REPLY:** We thank you for this positive comment. We think these details are often missing.

**L132-133** *The reason for this delay is not totally clear. Why this experiment was not run from 30 May to 18 June, given that the light conditions and temperature in summer are not the same as in early spring.*

**REPLY:** The reasons for the delay are totally logistical; time to plan and set up everything, along with coordination for other fieldwork. We added this precision in the revised manuscript (line 104). The ideal situation would have been to carry out the incubation directly in the field at ice melt to account for local irradiance and temperature, but we could not plan this way as the field station is remote, expensive to access and not equipped as down south. Although not ideal, this delayed experiment is what we have to offer, and we still believe it provides interesting insights on the susceptibility of winter DOM to photodegradation. We tried to take this into account afterwards by providing and comparing irradiance and temperature in the field. Yet, an advantage of carrying out the experiment close to the laboratory was to perform DOM analyses very quickly, allowing to capture fast-cycling DOM (which may be lost even in filtered samples kept for a few days/weeks).

**L145** *A reference for UV-transparency of FEP bottles is needed.*

**REPLY:** Please see what we added in response to this point also raised by reviewer 1 and 2 (lines 157-159).

**L211-212** *Unclear, what kind of data are discussed. Either make a part of this study or remove result of and reference to the unpublished work.*

**REPLY:** We have clarified the text relative to these added EEMs (lines 226-228). The added samples have no link with the experiment described in the article but were needed to increase the size of the dataset used to develop the PARAFAC model. They were only used for this.

**Results:** *Please present the changes in pH if any.*

**REPLY:** Unfortunately, we did not follow changes in pH, so we have no values to present.

**Discussion.** *The authors do not discuss any possibility of phototrophic bacteria production. The BP by leucine is good for assessment only heterotrophic bacteria.*

**REPLY:** We do not discuss this potential contribution by phototrophic bacteria because we checked the presence of picophytoplankton with the cytometer and we did not find any, even at the end of the incubation under sunlight. This is specified in the revised manuscript (lines 250-252).

**L405-429** Was there any bacterial exometabolite production that could diminish overall DOC loss?"

**REPLY:** This was not specifically measured, but it is certainly plausible that DOM was produced at the same time as it was consumed, as suggested by the rising concentrations of fluorophores (mainly observed in the dark). We added a sentence to acknowledge this (lines 563-565).

**L433-435** Rose Cory did not work with peatland lakes and rivers and thus irrelevant in this paragraph.

**REPLY:** We removed this sentence as suggested.

*It is a bit surprising that a seminal paper on biodegradation is not cited in this manuscript. I can only guess that it is not an intentional action from the senior authors but simple negligence from their younger colleague. My advice for the young researcher would be to check the keywords for the articles in the journal before submitting a manuscript. (Vonk, J. E., Tank, S. E., Mann, P. J., Spencer, R. G. M., Treat, C. C., Striegl, R. G., Abbott, B. W., and Wickland, K. P.: Biodegradability of dissolved organic carbon in permafrost soils and aquatic systems: a meta-analysis, Biogeosciences, 12, 6915–6930, <https://doi.org/10.5194/bg-12-6915-2015>, 2015). In particular, Vonk et al here discusses the seasonal effects on DOC biodegradation.*

**REPLY:** Thanks for the suggestion. We have added this interesting paper in the discussion part about biodegradation (lines 511-515). However, note that this paper is mainly dealing with lotic ecosystems, which have obviously very different biogeochemical dynamics from lakes. Also, they do not have many data points in winter, but much more in spring when DOM pool in rivers is receiving strong inputs from snowmelt. By comparison, our experimental water was sampled at the early end of the winter, when there is no autotrophic production in the system and no inputs from the landscape.

**L 430-431 and L 443-444** are somehow inconsistent; may be tone down the statement in L 430-431 or be more specific about the season.

**REPLY:** We agree and have adjusted the text to tone down this point (lines 451-4522 and 461-462).

**L451-454** May be provide the maximal range of this process

**REPLY:** We added the ratio provided by Moran and Zepp (1997) in lines 471-472.

**L 489-490** The reference is interesting; however, without comparison of this duration with water residence time in specific reservoirs, it does not add anything useful. It absolutely does not matter if half of DOC is biodegraded over 2.5 years if water resides in a given pond or streams for less than a few days or weeks.

**REPLY:** We agree, but this reference was rather added to illustrate the importance of time-scales when considering DOM degradation experiments. We added a sentence to raise the importance of considering residence time (lines 517-518).

**L570** Unclear what is complex pattern of sunlight exposure.

**REPLY:** We have adjusted the text to clarify this point (line 616). We replaced the expression "complex pattern of sunlight exposure" with "complex DOM pool containing molecules photodegraded at various degrees".

**L590-591** *The 7-cm depth of this study is strongly inconsistent with 0.8-m depth (L586) of other studies. Explain the context more specifically, i.e., humic vs non-humic lakes.*

**REPLY:** The 7-cm depth given here corresponds to the experimental bottle diameter containing the lake water exposed under a thin layer of clear tap water, constraining the exposition during the incubation. We assume that in this comment, you rather refer to the discussion where we describe the attenuation depth measured in our study lake (lines 635-638); these were indeed shallower by comparison to the depth indicated in the paper from Koehler et al. (2014). We adjusted the text to draw the reader's attention to the extreme humic characteristic of our study lake (lines 638-641).

**Cited references:**

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