**Dear Dr Koji Suzuki**

**In the revised version, we carefully considered each comment of Associate Editor and the reviewers; we agree with most of them.**

**You noted that,** "in Table 1, the DIC concentration of 0.32 mg L-1 at Piezomer was lower than the CO2 level of 3,360 µmol L-1; that does not make sense because CO2 should be a part of DIC. So, please clarify this issue."

**This is very pertinent remark. By DIC in the manuscript we intended to present carbonate alkalinity (mostly $HCO_3^-$ concentration), obtained by acid-base (Gram) titration. In slightly acidic waters of peatlands, the $HCO_3^-$ is naturally 10 to 100 times lower than the $CO_2$ concentration. To avoid the confusion, we removed the DIC notion from the tables, given that it is not discussed in the text.**

**We thank you for handling our submission and for giving us an opportunity to revise the manuscript once again.**

**Detailed responses to Reviewers are provided below**

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## Referee #1

The authors have addressed most comments from the previous revisions. The main difficulty with his manuscript is that addresses multiple questions, so the message is not necessarily clear. However, this version is considerably better,

**We further revised the text and presentation of figures. We agree that it is not easy to address multiple questions in one single manuscript, and we did our best to simplify the presentation without losing the scientific correctness and integrity.**

Line 100. Please use a different term for "poorly study".

**We replaced by "poorly known aspect"**

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# Referee #2

Major comments

I did not review the manuscript for submission or for the first revision, but I did review the second revision as the third reviewer. I think the second revision has been generally well revised according to the comments from the reviewer #1 and #2. My comments are basically minor, but I do have one concern about the second version. In Table 2, some of the CO2 concentrations seemed to exceed the corresponding DIC concentration. Is it correct? Please re-check all the numbers and units in Table 2.

**By DIC in the manuscript we mean carbonate alkalinity ($HCO_3^-$ concentration), obtained by acid-base (Gram) titration. In slightly acidic waters of peatlands, the $HCO_3^-$ concentration is, naturally, one or two orders of magnitude lower than that of $CO_2$ ($H_2CO_3^*$). To avoid the confusion, we removed the DIC notion from the tables, given that it is not used in the interpretation of results and not discussed in the text.**

Minor comments

Line 34-36: Trace metals should not be "degraded". So, it's better to rephrase the sentence.
**"transformation" is more general word here, consistent with the literature in the field.**

Lines 36-37: This sentence is not clear. It's better to rephrase such as "Here we determined the bio- and photo-degradability of DOM and its effect on the behavior of dissolved trace metals in humic surface waters…"
**Revised as recommended.**

Line 53: CO2 is probably better than C-CO2
**We agree and corrected accordingly.**

Line 60: "abiotic" is probably better than "inorganic"
**Fixed.**

Lines 62-63: As commented by the reviewer #2 (comment #3), "although the relative role of dissolved vs particulate organic carbon (DOC and POC, respectively) remains poorly quantified" is not necessary. It may be better to discuss photo vs bio rather than DOC vs POC here.
**We removed the part of the sentence on DOC and POC as suggested by the reviewer; it is a bit out of context here.**

Line 69: Please specify what the emission is.
**$CO_2$ emissions, revised.**

Line 80: "the other" is probably better than "another".
**Revised.**

Line 84: "metal-organic complexes" is probably better than "organic matter complexes".
**Replaced as recommended.**

Line 86: "metals from terrestrial to continental aquatic"?
**Agree and corrected accordingly.**

Lines 220-224: These sentences are basically duplicates of previous sentences. Please consider removing them.
**We thank the reviewer for catching this repetition and removed these four lines of the text accordingly.**

Line 254: This sentence is unclear. Please rephrase it.
**We removed this sentence because it was not necessary.**

Line 272-275: This sentence can be moved to page 12 and merged with the paragraph on ICP-MS methods.
**We shifted this sentence to L 289 as suggested by the reviewer.**

Line 347: 220 µmol L-1 is not found in Table 1. Please check it.
**Replaced by "…from 99 to 337 µmol L$^{-1}$"; it was a misprint.**

Line 358-361: These two sentences are basically duplicates. Please combine the sentences into one.
**The biodegradable DOC (BDOC, %) is calculated based on the magnitude of DOC concentration ([DOC]) change, and as such we believe that it is useful to present, first, the range of [DOC] modification. The 2$^{nd}$ sentence in this paragraph reports more general result than the first one, and thus we would like to keep it in the revised version.**

Lines 363-366: Does "integrated" mean "averaged"? I could not understand how the authors vertically integrated the values.
**We removed the term 'integral' here as it is not needed in the context**

Line 419: "increase" should be "decrease"?
**We are awfully sorry for this misprint. Certainly, it should be a "decrease". Corrected.**

Line 434 and elsewhere: The authors discussed seasonal differences in photodegradability and photodegradation rate without considering the differences in solar radiation. The authors should describe/discuss the differences in solar radiation determined by Luxmeter Testo 545.
**This is very pertinent comment. The light intensity sizably varied across the seasons, as noted by the reviewer. Results of Luxmeter measurements at the surface of water bodies studied in this work are presented in Fig. R1. In the text, we presented mean maximal daytime light intensity of June, August and October (5170±2760, 3220±2160, and 419±176 Lux, respectively) in section 3.3.1 (L 439-441) and we discussed the solar radiation differences across seasons in L525, 531-532 of the revised text.**
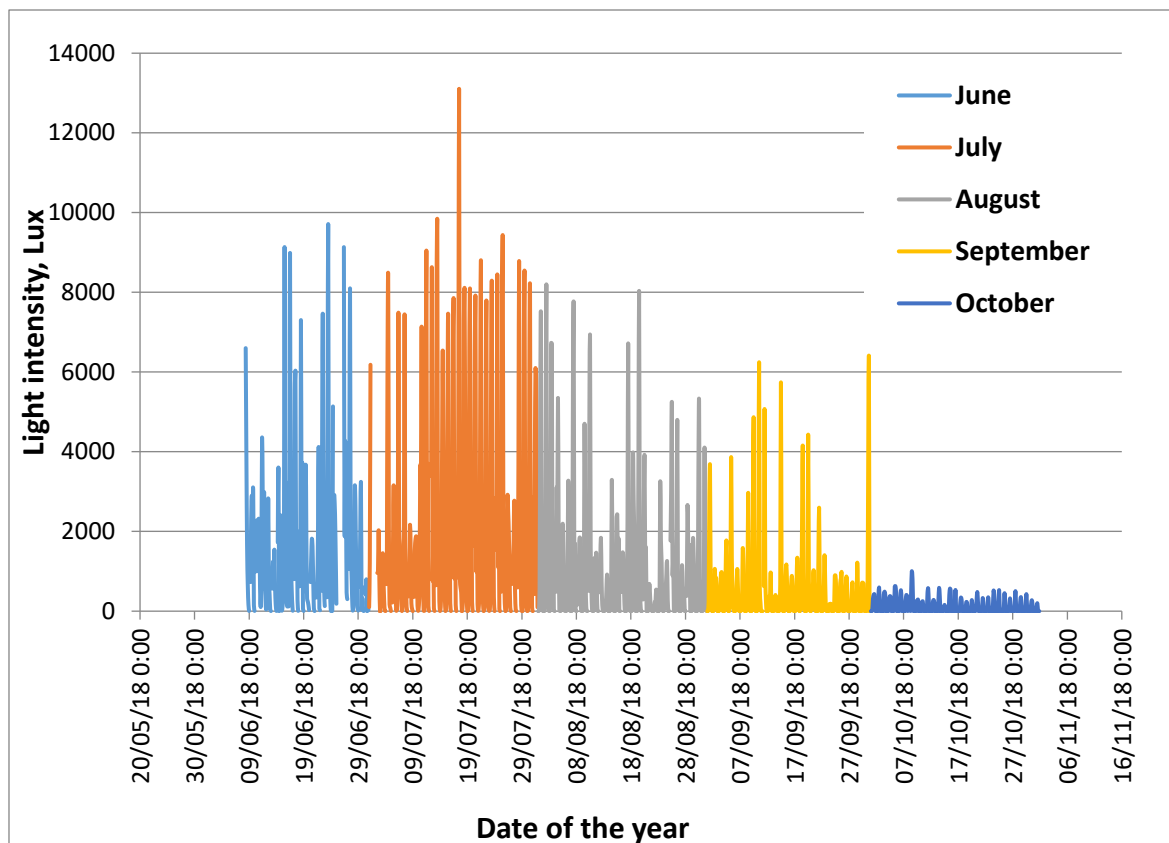
**Fig. R1.** Light intensity during photo-degradation experiments in boreal waters at the study site. Mean values over 3 hour interval.

Line 539: 650-500 nm should be 500-650 nm.
**Fixed**

Lines 549-553: This sentence is unclear. Please rephrase it.
**Rephrased as : "It is known that DOM photolysis mainly decreases the proportion of aromatic (colored) DOC and produces rather small (≤ 10 %) change in bulk DOC concentration (Laurion and Mladenov, 2013; Koehler et al., 2014; Groeneveld et al., 2016; Oleinikova et al., 2017; Chupakova et al., 2018; Gareis and Lesack, 2018). The present study corroborates these former findings across a much larger seasonal scale and spatial resolution of boreal surface waters", L 551-556.**

Lines 571-574: The authors integrated the CO2 flux from surface water to the atmosphere due to biodegradation from the surface to 10 m. I have a concern about this estimation because CO2 produced in the deep layer cannot be released directly to the atmosphere during strong stratification periods.
**The comment is well taken. Justification for this choice of the water column depth is that Lake Temnoe is not chemically stratified in the first 0 – 10 m water layer, which does not mix up with anoxic hypolimnion and is not subjected to the influence of sediment respiration. We added necessary explanation and pertinent reference (Chupakov et al., 2017) to the revised text (L574-576).**

Line 585: 1-0.8 m should be 0.8-1 m.
**Fixed**

Lines 650-653: This sentence is unclear. Does P mean dissolved organic phosphorus? What about Zn? Please rephrase this sentence carefully to make the authors' message clearer.
**Yes, here we hypothesize about P-PO$_4$ and Zn as possible limiting nutrients for biodegradation. However, we decided to remove the last paragraph of this section because it is too speculative and not supported by relevant speciation studies.**

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# Referee #3
1. Line 240: What is the exact size of the pore?
**0.22 µm as stated in L 232, L 235 of the revised text.**

2. In Fig. 2, the legends are unclear; please clarify. Additionally, the displayed depth is confusing.
**We strongly revised and reorganized Figure 2 for clarity and made the legend and depth labeling similar to those in Fig. 3.**

3. The content in Section 3.3.2 could be moved to the SI document since most of the figures are discussed there, or you could move Figures S6 and S7 into the main manuscript.
**May be there is a confusion. Figures S6 and S7 report the evolution of Mn and V concentrations during bio-degradation experiments. Section 3.3.2 describes the evolution of optical parameters of DOM. Adding figures S6, S7 (and, presumably, S8) to the main text is undesirable because the manuscript is already on the long side. The presentation of results on TE in section 3.2.4 is organized into several groups of elements exhibiting similar behavior, and only a few of them are illustrated in Supplementary figures.**