

# ***Interactive comment on “Humic surface waters of frozen peat bogs (permafrost zone) are highly resistant to bio- and photodegradation” by Liudmila S. Shirokova et al.***

## **Anonymous Referee #1**

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### General comments:

This paper presents a potentially important challenge to the current emerging paradigm that photo- and bio-degradation of DOM in the water column of streams and lakes is an important control on inland water CO<sub>2</sub> emissions in permafrost regions. Because of this, the manuscript needs a bit of work and clarification to reassure readers that these results are not anomalous. I have made some recommendations here, and include three more general points below and several specific comments to help with this.

One of the core issues partly addressed by this ms is that experimental treatments for DOM incubations are designed to isolate specific processes to answer the ques-

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tions at hand, but of course in the natural environment these processes don't operate independently. A lot of controlled laboratory studies have shown that in stream processing of DOM is important, but it is not well demonstrated how this actually occurs in the field given the often unrealistic conditions of incubation (e.g. doi:10.5194/bg-15-7141-2018). Biodegradation doesn't occur entirely in the absence of photo-oxidation, priming from fresh plant/soil leachates, or the full breadth of bacterial and viral community dynamics. The separation of these processes may to some degree explain the results presented here, and this should be addressed more fully in the discussion.

Another issue with this study is the uncertainty. In the methods, the authors present uncertainty values which are roughly equal to the degree of degradation observed in the current study. It is not clear how these uncertainties were calculated and propagated through the results, and therefore whether these rather high uncertainties explain the limited response of the incubations.

Finally, this study stresses how different the study site is to previous studies which have shown degradation of DOM in the water column to be an important contribution to CO<sub>2</sub> fluxes. A more robust comparison than the general terms currently used would be very useful to more clearly explain the site differences and enhance the discussion on why these might cause such divergent results. I give more details in the specific comments below.

Regardless, these are very interesting results that should be of great interest to the community. It will be interesting to see if other studies find similar results beyond the somewhat limited spatial coverage of Arctic inland water DOM degradation sites to date.

Specific Comments:

L44. 10% is generally measurable, is it not? See later comments on uncertainty.

L61-64. Needs to be clear this conclusion is only for high latitude systems, Vonk et al

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2015 didn't look at systems outside of the permafrost zone.

L78. These studies aren't from mountain regions. They are tundra ecosystems with peat soils overlying the mineral substrate on the Alaskan north slope. See comments below on characterising the study sites of previous work.

L86. A key point here is not that photo-oxidation will convert much DOC directly to CO<sub>2</sub>, but that it can transform DOM molecular structures into more (or sometimes less) biolabile forms (see Cory and Kling 2018). The interaction between photo- and bio-degradation is an important aspect, not just the individual processes themselves. The combination of the two processes is not explored by this experimental design, and so cannot be ruled out.

L87. I wouldn't say they are controversies, more that there is an emerging paradigm which may not be as consistent across the Arctic as previously thought.

L96. Citation needed.

L103-104. I think you are generalising too broadly here. The Mann et al. 2015 study was conducted in the yedoma region on the eastern Siberian Arctic where drainage flows through upper peat layers as well as frozen yedoma soils.

L107. Photo-oxidation and biodegradation are important components of peatlands also (e.g. doi:10.5194/bg-14-1793-2017; doi:10.1016/j.jhydrol.2013.03.016; doi:10.1029/2018JG004650)

L122-125. This does not belong in the introduction.

L169. "All"

L180. Add citation to support this (e.g. doi:10.5194/bg-15-7141-2018)

L183. Did you measure DOC concentrations of 3um filtrate?

L185. There is not a lot of carbon in the 0.22 um to 0.7 um size range (see discussion

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in doi:10.5194/bg-15-7141-2018 and references therein). L206-209. Which samples got which treatment? I would have thought a consistent treatment of all samples would make more sense, so please justify and clearly explain each incubation process in more detail.

L210. So they were all incubated in an outdoor pool? This makes the previous sentence confusing as to why it was written that way.

L211. So the headspace was a closed system, i.e. no O<sub>2</sub> was able to enter nor CO<sub>2</sub> exit the incubation vessels? Is this standard protocol? Can you justify whether this method would prevent O<sub>2</sub> limitation from slowing photo-oxidation?

L237. This is not a common method for measuring dissolved CO<sub>2</sub> in aquatic systems. How long was the probe submerged for? How did you ensure it was in equilibrium with the water being measured? Did you measure replicates, or attempt to constrain local variability in your measurements?

L241-242. Add a sentence here to explain why you selected these wavelengths and what they can tell the reader about DOM structure. Later you mention E<sub>4</sub>:E<sub>6</sub> ratios as well, introduce them here.

L273. How were these uncertainties calculated? This becomes very important because in your results you do not present any changes that greatly exceed these uncertainties, and in the abstract 10% is highlighted as below detectable - these limits need to be carefully and quantitatively justified in order to support the main conclusions of this study.

L279. Which statistical package did you use for these analyses?

L309. This is not well worded. The inability to detect this change was due to the instrument limitations, not the high DOC content.

L338. This data should be presented for transparency, but is fine to go in the supplement - same goes for the EC data, DIC and O<sub>2</sub> data.

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L362 and 366. Again, why are these not presented somewhere, even if in the supplement? This does not help transparency of the study methods.

L378-379. Yes, but according to your uncertainties, these values would also mostly lie outside of the detectable limit.

L381. It feels like this point is being over sold, and that the differences between the study site and the previous work in high latitude permafrost systems are not that great, and what differences there are currently are not well constrained. I would recommend that the authors develop a framework to present these inter-site differences, for example a table with soil C-content, soil depths, climate, elevation etc. This would not be a big effort, and would much more strongly back up the claim that these sites are so different.

L389. 0-1% does not equal the 0-10% seen in this study. Maybe this is just an artifact of the way you present these values, i.e. were the majority of the BDOC values closer to 0 than to 10%? Maybe rethink how you present this number when contextualising.

L406. This isn't well explained - there isn't any clear impact of priming here because to investigate priming, an interaction between the fresh vegetation/soil leachate and the recalcitrant organic matter pool has to occur in the experimental design. It's also not clear what direct relevance the priming question is to the current study design.

L428. This is also partially supported by the findings of doi:10.5194/bg-15-7141-2018, and it may also be worth noting that bacterial communities are not just shaped by size fractionation by filtration, but also the presence or absence of bacterial grazers.

L438. While I can see the interest in including a 37degC treatment to help answer the question of degradability, it cannot be argued that surface waters in the Arctic would ever be expected to reach those temperatures. Consistent 23degC in surface waters is already unlikely in that part of the world. This doesn't change the point presented here, but I think it's important to not misrepresent the experimental design.

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L472. See my comments above for L381, but that said the discussion here is strong and well supported.

L478. "hours"

L487. This proposed mechanism requires the water column to be well mixed, with no photo-degradable DOM present in water deeper than 0.5m. Is this reasonable at the study site? Is this the key difference between this site and the other cited in the literature? Again, this could be addressed with a more in depth exploration of the site differences (see comment re. L381).

L517. How does "sizeable" compare to other studies?

L538. Not a lot of photo-oxidation occurring in the soil, or do the authors mean once the DOM enters open water?

L558. Based on the discussion above, residence time in the soil sounds like the most important control, given that the authors argue that degradation in the soil means almost no degradable DOM is entering the aquatic systems in the study area.

Fig S1. The upper panel does not give a good regional context of where the site is. I suggest presenting this location in the context of the whole Arctic.

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