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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.

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Dear Dr Laurion We are very grateful for your constructive comments which will allow improve the manuscript. First of all, we totally agree that, in order to challenge the paradigm, we have to test both processes (photo- and bio-degradation) at the same time. The present paper was designed to rigorously evaluate them independently, which represents only a first step in this direction. We are aware of the importance of photo-produced organic ligands. In fact, low molecular weight organic carbon production upon sunlight exposure of surface waters is well known since pioneering works of Zafiriou et al. (EST, 18(12), 358A-371A, 1984) and confirmed over past decade in

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the Arctic waters (Cory et al., 2007, 2014). In our experiments on humic waters from permafrost-free zone of NW Russia, these photochemically produced organic ligands were <1 kDa as followed from the increase of LMW< 1 kDa concentration of organic carbon in quartz reactors in the course of sunlight irradiation (Oleinikova et al., 2017, GCA, 211, 97-114)). For example, over 10 days of solar irradiation, the bog water from N. Karelia produced a 2-fold increase in concentration of low molecular aliphatic acids (acetic, formic, oxalic and citric) and benzol-carbonic acids. This increase (ca. 0.2 mg/L of acids), however, represented less than 10% of overall DOC increase in the LMW< 1 kDa fraction. As such, we believe that the overall photoproduction of biolabile organic ligands does not exceed 10% of the initial DOC and as such cannot account for more than 10% of bio-degradation. This is consistent with maximum 10% of bio and photo-degradable DOC reported in the present work.

Your comment on dark DOM chemical oxidation that can be important in iron-rich organic-rich waters facing redox oscillation is well taken. Concerning Bolshezemelskaya Tundra environments, we do not expect sizeable redox oscillations in aquatic systems, be it large Pechora River or shallow (< 0.5 m) oxygenated thaw pond. We acknowledged anaerobic C mineralization in thermokarst lake sediments (L547-549) as a possible mechanisms of CO2 production.

You stated the importance of delay between sampling and experiment, and we seriously took this issue during our design. This time was minimized to several hours employing fastest transfer of refrigerated samples to the laboratory. Note that the DOC concentration at the moment of sampling and at the beginning of experiments was identical thus suggesting the lack of transformations.

The issue of DIC evolution is important. However we were not able to detect any change in DIC concentration in the course of experiment, i.e., within 5% of analytical and experimental uncertainty. We will present the DIC data in supplementary information of revised paper.

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Finally, the comment on total bacteria count (DAPI method) is pertinent, and we also addressed it in our response to Reviewer No 2. The TBC increased over the course of experiment as illustrated in Fig. S3D of the supplement.

We thank you for very pertinent comments and we will revise the manuscript accordingly.

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