This reviewer noted that the ms “needs a bit of work and clarification to reassure readers that these results are not anomalous”.

His/her first issues is that “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.”
**Reply:** We are confident that in this study we did separate bio- and photo-degradation. First, our biodegradation assays followed the standardized protocol for assessing biodegradable DOC in Arctic waters (Vonk et al., 2015). Second, all incubation were run in the absence of light, in bottles wrapped in Al foil, in the dark. For photodegradation, sterile filtered (< 0.22 µm) water was used and this is the only suitable method for biodegradation assays. However, we do agree with the reviewer and seminal papers of R. Cory et al that biodegradation can be enhanced in previously photodegradated samples, and the separation of two processes in natural settings is not possible. Yet, the present study is purely experimental, aimed at separating bio- and photodegradation in the laboratory.

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.
**Reply:** The response is below 10%, and this is the main result of the present study. We present true uncertainties and 10% is the best what an experiment can provide. It is important to distinguish these 10% of experimental uncertainties from 1-2% of analytical uncertainties. Below we present detailed explanation of the uncertainties.
To assess the variability of results, shown as vertical uncertainties in the graphs, we used the percentage ratio of standard deviation of *n* replicates at the *i*-th day of exposure to the initial DOC concentration following:

$$SD_i = \sqrt{(((BDOC_i^1 - BDOC_i^{mean})^2 + (BDOC_i^2 - (BDOC_i^{mean})^2 + .....(BDOC_i^n - BDOC_i^{mean})^2)/n)}$$

$$\%SD = (SD_i/DOC_0) \times 100$$

The results were presented as $\%BOD_i \pm SD_i$

To assess the uncertainties during photodegradation experiments, we used the percentage of standard deviation on *n* replicates at the *i*-th day of exposure to the DOC concentration in the dark (blank control) reactors as

$$PDOC_i^n = DOC_i^{blank} - DOC_i^n$$

$$PDOC_i^{mean} = (PDOC_i^1 + PDOC_i^2 + .....PDOC_i^n)/n$$

$$\%PDOC_i = (PDOC_i^{mean} / DOC_i^{blank}) \times 100$$

$$SD_i = \sqrt{(((PDOC_i^1 - PDOC_i^{mean})^2 + (PDOC_i^2 - PDOC_i^{mean})^2 + .....(PDOC_i^n - PDOC_i^{mean})^2)/n)}$$

$$\%SD = (SD_i / DOC_i^{blank}) \times 100$$

The results were presented as $\%PDOC_i \pm SD_i$

In full agreement with Vonk et al. (2015), the negative values of %BOD or %PDOC were assigned to zero.

Further, this reviewer stated that "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 CO2 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."
**Reply:** We believe that relating DOM degradation and $CO_2$ emission fluxes across the Arctic was beyond the scope of this work. Note that currently we are preparing a report on $CO_2$ concentration and emission fluxes from inland waters of Bolshezemelskaya tundra, performed over a wide range of season and geographical coverage. Further, there is an excellent review summarizing most available studies (Vonk et al., 2015) and we related to this when discussing the broad significance of our results. However we will reorganize and extend our discussion as recommended by reviewer.

**Reply to Specific Comments (we agree with most of them and will revise the manuscript accordingly, so only arguable comments are listed below)**

L61-64. Needs to be clear this conclusion is only for high latitude systems, Vonk et al 2015 didn't look at systems outside of the permafrost zone.
**Reply:** Not correct. Vonk et al demonstrated zero BDOC loss in aquatic systems without permafrost

L183. Did you measure DOC concentrations of 3um filtrate?
**Reply:** Yes, of course. The DOC concertation was similar within ±2% between 3 μm, 0.7 μm and 0.2 μm poresize. We are aware that some studies reported notable differences for poresizes, but this depends on environmental context (such as phytoplankton bloom) which is not the case of our study.

L210. So they were all incubated in an outdoor pool? This makes the previous sentence confusing as to why it was written that way.
**Reply:** yes, this is most conventional methodology. All manipulations were done in laminar hood box.

L211. So the headspace was a closed system, i.e. no O2 was able enter nor CO2 exit the incubation vessels? Is this standard protocol? Can you justify whether this method would prevent O2 limitation from slowing photo-oxidation?
**Reply:** There was no $O_2$ limitation because, as it is stated in L338-339, the exposed water remained oxygenated.

L237. This is not a common method for measuring dissolved CO2 in aquatic systems. How long was the probe submersed 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?
**Reply:** This is new and highly reliable method. We can only reference a recent paper of our group (Serikova et al., 2018, Nature Geoscience) that discusses methodological aspects.

L378-379. Yes, but according to your uncertainties, these values would also mostly lie outside of the detectable limit.
**Reply:** We cannot judge mathematical significance of the differences between the present study and other works. Vonk et al reported 3 to 18% (mean 13%) in continuous permafrost

and 5 to 15% (mean 14%) in discontinuous permafrost on mineral soils. Our study adds another important and representative site to this list, where the BDOC ranges from 0 to 10%.

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.
**Reply:** Such a review goes a bit above the scope of this work. The compilation is available in Vonk et al (Table S1 of their article), so we do not see any interest of reporting it again.

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 contextualizing.
**Reply:** The value in Vonk et al (2015) is between 0 and 1%. The experimental reproducibility is around 10%. Indeed, the majority of our BDOC values were closer to 0 than to 10% - but this is still within 10% of experimental uncertainty. Such an uncertainty is merely inevitable, because we deal with the difference of two large values (high DOC concentration in humic waters). In oligotrophic low-DOC waters, the analytical and experimental resolution would allow for much higher precision of delta DOC measurements.

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.
**Reply:** We only partially agree with this. Russian Arctic inland waters are really different from other regions of the world, and we encountered water temperatures around 25-26°C in Bolshezemelskaya Tundra in July 2015 (in preparation), and thermokarst lakes of western Siberia discontinuous permafrost zone had water temperatures around 27-29°C in July 2012 (Pokrovsky et al., 2013, Biogeosciences)

L472. See my comments above for L381, but that said the discussion here is strong and well supported
**Reply:** We thank the reviewer for pointing this out, but the comparison between sites is not trivial. The key issue is how the soil and plant litter DOM are delivered to lakes and rivers via suprapermafrost waters. If these waters drain through mineral or organic surface soil horizons, this will determine the difference between sites. Such information on the exact position of active layer within a mineral or organic soils is rarely available except several case studies (i.e., Raudina et al., 2017 Biogeosciences; Raudina et al., 2018 Sci Total Env). Today, without hydrological model of water objects (and they are rarely available for BDOC studies), straightforward comparison between sites considering just general soil and climate context is not warranted.

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).
**Reply:** The depth of majority of thermokarst lakes is < 0.5 m. Yes, the water column of lakes and rivers in BZT are well mixed and well oxygenated.

L517. How does "sizeable" compare to other studies?
**Reply:** Sizeable here means 20 to 50%. To our knowledge, this is the only study where P, Fe and trace metals were monitored during photolysis of humic waters from permafrost zone

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.
**Reply:** Yes, we hypothesize that water residence time in soil exerts primary control on bio- and photodegradability. However, it is possible that the degradation happens very fast, once the soil fluids enter the open waters (as actually indicated by this reviewer in his/her comment to L 538). In that case, it is the balance between half-life time of soil BDOC (minutes to hours) and water residence time in surface reservoirs (days to weeks), which determines the biodegradability potential of DOM in sampled waters.

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.
**Reply:** We would like to note that our map encompasses huge part of the Arctic, from 35 to 75°E. The studied territory is between the Scandinavia and Yamal Peninsula and its position is clearly indicated by rectangle. We will modify this map in revised version

We thank reviewer No 1 for very insightful and constructive comments