

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Jens A. Hölemann et al.

Author comment on "The impact of the freeze–melt cycle of land-fast ice on the distribution of dissolved organic matter in the Laptev and East Siberian seas (Siberian Arctic)" by Jens A. Hölemann et al., Biogeosciences Discuss.,
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We thank the referee for the constructive comments and questions. We also thank the anonymous reviewer for the positive comment on the publication of the entire data set. To "fully" analyze this dataset, which spans a 9-year period and multiple summer and winter expeditions, and present the results in this study is beyond the scope of this paper. However, we believe that the results presented in this paper give a key to any further interpretations. The main objective is to show, using measurements and calculation of budgets, that the distribution of DOM concentrations in the Laptev and East Siberian Sea is determined primarily by the mixing of three freshwater "parent water masses" (river water during the freshet, river water after the freshet, and meltwater from the fast ice) with different DOM concentrations and compositions. As far as we understand, this central hypothesis is not challenged by the reviewer. Although this paper addresses only one aspect of the topic of DOM transport and degradation in the Arctic Ocean, we believe it makes an important contribution to the scientific debate. In order to give other scientists the opportunity for further analysis regarding other aspects, we have decided to publish the entire data set in full spectral resolution. In trying to write the paper as concisely as possible, we seem to have inadequately presented important results of our analysis (such as the presentation of the ratio of the river water fraction - O18 data - to CDOM absorption). We hope that the reviewer will nevertheless believe us when we emphasize that we have thoroughly analyzed the data set.

A detailed determination of the degradation rates of tDOM on the Siberian shelves is not the central topic of this study. Our data set also indicates that some tDOM is mineralized in the area of the Siberian shelves. However, it also indicates that degradation of tDOM cannot adequately explain the observed concentration distribution on the shelves. However, we agree with the reviewer that we should better elaborate on the key messages by including figures of river water fraction (O18 data) and CDOM absorption and discussing these data in more detail. We will do this in the revised version of the manuscript and hope to address the reviewer's main criticisms. We will also discuss the slope values (S275-295) and their significance in more detail. However, the inclusion of these results, which is certainly necessary, does not disprove the central conclusions of this paper but is further evidence of the plausibility of our hypotheses. We have

summarized the response to all of the referee's comments and questions in a separate document (see Appendix).

The formation of the Arctic halocline is an important and interesting research topic that our group has also been working on for more than 20 years. However, the description of this physical-oceanographic process is not - and cannot be - the subject of this paper. In this context, we refer to the published studies of some of the coauthors: Bauch et al. (2009, JGR), Bauch et al. (2011, Prog in Oceanogr.), Bauch et al. (2014), Janout et al. (2017), and Janout et al. (2020). These papers, which are cited in our paper, provided an important basis for the hypotheses presented in this study.

Please also note the supplement to this comment:

<https://bg.copernicus.org/preprints/bg-2020-462/bg-2020-462-AC2-supplement.pdf>