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The Cryosphere Discussions

Interactive comment on "Organic matter across subsea permafrost thaw horizons on the East Siberian Arctic Shelf" *by* Birgit Wild et al.

Serov (Referee)

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The manuscript describes content and composition of organic matter within a recently thawed permafrost horizon encountered in 3 re-drilled offshore boreholes in the Laptev Sea. The results indicate that the bulk organic matter content is rather low, which resonates with previously published works. Lignin phenol concentrations are unexpectedly low, which the authors suggest is due to its preferential degradation, river transportation and re-deposition. Using grain-size distribution, the authors attempt to reconstruct depositional environment, which would support their interpretations of the sources of the organic matter. Interpretation of analysis points toward rather heterogeneous content, origin and composition of organic matter within a small study area (maximum distance between the boreholes seems to be ∼2 km) overriding any detectable effects

of organic matter degradation.

The data quality and analytical techniques used are beyond dispute. The text is clearly written, the very general title adequately reflects somewhat diluted focus of the paper, and the subject matter of the manuscript may fit the scopes of The Cryosphere journal. However, I found a few rather large-scale issues that prevent me from recommending this manuscript for publication at its current state.

1. Lack of focus. It is not clear what this paper is about. When reading the Abstract and, partly, introduction I was sure it would tell a story of a recent thaw leading to organic matter degradation and greenhouse gas production. These first parts of text unambiguously point towards that. Yet, the results subchapters and discussion talk about depositional settings, permafrost evolution throughout the last 160 ka, origin and transportation of organic matter, etc. What all these findings have to do with the truly unique observations of rapid and recent thaw? In fact, investigating any permafrost core in the region (no matter if it is frozen or thawed, first time drilled or re-drilled) would reveal the same facts on sediment deposition, organic carbon contents, and nature of organic matter. In addition, I have to mention that the paper by Shakhova et al., 2017 covered substantial part of what is discussed in the manuscript. 2. Mismatch of the declared research question(s) and results-discussion. The manuscript stresses on importance of permafrost thaw for climate gas budgets. Therefore, degradation state of organic matter in the cores would be the most relevant part of the manuscript. However, on the second-to last page, the manuscript says that none of the analyzed parameters (org. carbon content, d13C, lignin phenol ratios, OC/TN) is sensitive enough to capture the decomposition of organics matter. The other findings do not seem to have direct relation to the climate gas problem, or the manuscript does not explicitly show such connections. Perhaps, one way to approach answering the question of CO2 and CH4 release from the thawed strata would be to calculate potential maximum and minimum scenarios of greenhouse gas generation in these particular settings and assuming the estimated thaw rates. Knowing that 1.6 kg of OC m-2 thaw-out every year does not

bring us to understanding the magnitude of associated climate gas production. 3. Watery conclusions. Throughout the manuscript, I noticed that several lines of evidence lead to somewhat empty conclusions, such as Âńtaken together these findings point at the accumulation of material of different origin at varying proportions..", or the ones saying that more research is urgently needed. I suggest cutting off all parts of the manuscript not leading to new/important results. 4. The above-mentioned issues brought me to a conclusion that I cannot say that the paper advances our understanding of how important the extremely fast permafrost thaw is for modulating CO2 and CH4 release. The manuscript does thoroughly describe several properties of the organic matter within investigated cores and provides a discussion of the sediment deposition and thaw history (which overlaps with the previously published paper by Shakhova et al., 2017). This does contribute to a growing number of site-specific descriptive studies of OM composition within permafrost, but does not provide a transformative step towards understanding the consequences of rapid permafrost thaw.

Please, find the smaller scale issues and some technical corrections in the enclosed .pdf file.

Sincerely, Pavel Serov

Please also note the supplement to this comment:
https://www.the-cryosphere-discuss.net/tc-2018-229/tc-2018-229-RC2-supplement.pdf

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