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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.

Anonymous Referee #1

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General comments 1) I am puzzled by this manuscript. The authors provide a large data set and the manuscript is well written. I do not have a lot of criticism about each chapter itself, however, I found myself asking what the real aim of the study was since each of the chapters seems to navigate towards different topics that do not have a common scope. I think this is mostly due to the fact that there are no distinct changes between proxy data above and below the IBPT, which was likely not expected. Accordingly, other aspects are discussed, but I am missing a little more focus on how this helps understanding subsea permafrost thaw (feedbacks). Here is my summary of the main points of each chapter, which hopefully shows why I think the overall manuscript needs a more structured aim. The introduction provides background on the ESAS and potential CH4 release from subsea permafrost due to climate change and per-

mafrost thaw. This permafrost carbon feedback and CH4 dynamics particularly is/are not mentioned/discussed again. If this will be picked up again, the discussion should acknowledge that looking only at lignin phenols and no other compound classes provides a limited assessment of the molecular composition a very limited view on microbial degradability. The discussion starts (chapter 3.1) with a comparison of the grain size data with other Siberian permafrost deposits in order to constrain the origin of these deposits. This certainly is important background knowledge, but the data are acknowledged to having been published by Shakova et al. (2017) already and cover a much wider depth range than what is relevant for the remainder of the discussion and understanding IBPT deepening and feedbacks. This chapter could be significantly shortened and mostly reference Shakova et al. (2017) to provide the necessary background information. In chapter 3.2, the bulk characteristics are discussed in comparison to other high latitude regions including the ESAS as well as Alaska and Svalbard (for OC loading) and between different deposit types (marine and lacustrine sediments, soils). This broader geographical context is not discussed in any of the other chapters and I wonder how OC loadings in Svalbard active layer soils (Svalbard itself is a very different system) help to understand changes associated with subsea permafrost thaw in the three investigated cores? Chapter 3.3 provides constraints on lignin phenol sources/origin and an assessment of the degradation state of these lignin phenols both in comparison with other studies in the Buor Khaya vicinity. To tie it to the previous chapter, how does that compare to other high latitude settings? Also, how much do we learn from lignin phenol degradation state alone without additional information from other (less refractory) compound classes? Chapter 3.4 finally provides the discussion one would expect – the comparison of proxy data above and below the IBPT. This chapter is relatively brief and while the statistical analysis does not show significant changes between frozen and thawed subsea permafrost in these three cores, there is some variability that could be discussed in a little more detail (than the three sentences at the end of the chapter). Also, I would expect an assessment of whether significant changes across the IBPT can be expected in the first place given that the deposits are

so heterogenous? Are 30 years of thaw enough to expect a significant change in bulk OC characteristics and lignin phenol abundances (even in a homogenous deposit)?

2) I am not quite convinced by the argument made regarding the IBPT depths and sediment age in cores 2D-13 and 4D-13. The jump from 51ka at 17m depth to 8.5ka at 15m in core 4D-12 is explained as either a period of low deposition or an erosional event (page 6). The IBPT in core 2D-13 is at ca. 16m (the "peak jump" in AD-12), the IBPT in core 4D-13 at about 9m. Neither the lithological nor the organic proxies allow for any easy core correlation. This is likely due to the different depositional settings and indicates that the depositional history is different, thus, also invoking a period of low deposition or and erosional event and expecting younger ages for cores 2D-13 and 4D-13 is not unequivocally justified. These two cores may not be affected by the same processes and sedimentation rates might be very different. Please provide additional age constraints (OSL or maybe 14C?) or discuss this much more carefully.

3) you may want to consider additional lignin phenol data from the Buor Khaya Bay: Ulyantseva et al. (2018) The Molecular Composition of Lignin as an Indicator of Subaqueous Permafrost Thawing. doi: 10.1134/S1028334X1810029X.

Specific comments Page 2 l.3-5: it would read nicer to combine the subordinate clause with the remainder of the sentence. l.6: "destroyed by erosion" or eroded? l.7: "This process" - which process are you referring to? Erosion or inundation? l.8: add superscript to unit. l.10: "due to the changing thermal gradient"?

Page 3 l.7: capitalize "delta". l.12: please define the origin of Yedoma (vs. fluvial and alluvial deposits). l.20: change header or include additional header after l.26. Most of the chapter does not reference the field work, but either IBPT deepening rates or laboratory methodology/sample processing.

Page 4 l.11-12: move last sentence to introduction or discussion.

Page 5 l.11: is OC not total OC? The distinction is made for N, but not OC. l.30-31:

please use common italics notation for phenols.

Page 6 l.1-9: please use common italics notation for phenols. l.5: add information that all phenol concentrations were normalized to g OC.

Page 7 l.1-3: what is the threshold value to differentiate unimodal and bimodal distributions? Based on Fig. 2, 4D-12 also has substantial clay contribution (some 25% or so) in those intervals dominated by silt, but this does not qualify as a bimodal distribution? l.9-27: it would be nice if the argument order in this paragraph was reversed, starting e.g. with the sentence in l.20 so the connection to the above paragraph is more obvious. l.19-20: does TC allow citing articles in review? l.22-23: more similar in comparison to?

Page 8 l.8: change to "normalized to" l.15: what can we learn from comparison with active layer soils in Svalbard? That is a very different Arctic setting. l.18: O'Leary (1988) does not provide δ13C values for plants in East Siberia. l.18-21: please provide endmember values for the cited high latitude references. l.30-32: you are only looking at lignin phenols (not the molecular composition), so the data provide a very limited view on microbial degradability, since no other compound classes are assessed and lignin is a very refractory material to start with.

Page 9 l.18-22: while the acid to aldehyde ratio is used to determine the degree of aerobic decomposition, isn't the fact subsea permafrost in this area is anoxic below the SMTZ (which coincides with the IBPT; e.g. Winkel et al. 2018, Scientific Reports, doi:10.1038/s41598-018-19505-9) suggesting that this is rather a function of age/exposure time to aerobic conditions? Irrespective of transport distance and duration, if the OC from the boreal forests in the South was stored in soils prior to erosion and export, it is likely much more degraded. l.26-27: this statement contradicts with the previous statement arguing for a lower degree of lignin degradation in subsea permafrost samples vs. riverine and surface sediments (l.18-22). Also, based on Fig. 6, the heterogeneity in terrestrial deposits and surface sediments is large and within SD

at least agrees with the concentrations in core 4D-13.

Page 10 l.2-3: I would argue that the data set is too limited to assess OC qualities. It allows to assess lignin quality; for OC quality, a comprehensive data set including various compound classes would be needed. l.8-14: this entire paragraph is phrased as if these measurements were performed on actual density fractions, which is not the case. This should be re-phrased to acknowledge that "samples with finer or coarser grain size distributions. . ."

Fig. 1 Please add inset boxes and panel IDs and increase the font size in all maps and the legend. Add contour lines and legends to the small maps on the right. These also miss coordinate systems.

Fig. 2 What exactly is the reference point for the distances? The coastline? l. 4: change "has not been" to "was not"

Fig. 4 Please include abbreviations in figure caption or use unabbreviated labels. There are many more endmember values available for %OC and OC/TN, why are they not included? One example, several marine sediments are referenced in chapter 3.2, which could be added to the plot. Why are the own data shown as box-whisker plots, but the reference data are not? Comparing medians and means is not straightforward.

Fig. 6 Please include abbreviations in figure caption or use unabbreviated labels.

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