

Interactive comment on “Yedoma Ice Complex of the Buor Khaya Peninsula (southern Laptev Sea)” by Lutz Schirrmeister et al.

Anonymous Referee #2

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General comments

This is the new study by the experienced team of authors working in North-Eastern Siberia on paleoenvironmental proxies preserved in permafrost. We know their examinations of several sections of Quaternary deposits in this region. Their studies are usually accomplished on a highest level technological basis, and large amount of various field data. This paper is not the exclusion. It deals with the new exposure of Late Pleistocene to Holocene permafrost. To our knowledge no one worked on Buor Khaya peninsula stratigraphy before. A number of physical and geochemical methods to study permafrost sequences were applied, along with consequent analysis. So this contribution is valuable for the new data and high quality analysis of the data. However, we see only pure geological nature of this study, having poor relation to life issues or biogeochemical cycling which are in the scope of the Biogeosciences journal. We recommend

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this paper to be published in a profile quaternary geology/stratigraphy/permafrost journal (like Cryosphere), and would like to focus the Editorial board's attention on that fact. This statement is approved by the issues discussed below in this review, and the other reviewer's report.

Specific comments

Although thoroughly prepared and well written, this paper has several shortcomings, most pronounced of which are Figures. For most of them (except Figs. 2, 7, 13, 16) either the color scheme or the font size do not allow comfortable reading.

Figures 2-12 and 14-15 represent valuable data which lie in the basis of author's conclusions, but their representation looks excessive, like in the field report. Given the fact that very little was done in terms of statistical analysis of different sediment strata, I recommend authors to provide conventional means and standard deviations of the variables within the strata. The analysis of differences of the means between strata will also allow the readers to see how significant they are. We recommend placing the general cross-section with all three excavations into text, using the larger fillings of different types of the sediments and larger text, providing the sampling locations for dating on the cross-section. Separate figures or panels could be used to illustrate the changes in the means and standard deviations for every exposure/borehole.

Figure 7 should be redrawn as a column of sediments seen in the core. We understand that the ice-wedge is schematically depicted in such a 'carrot-like' way, but the way it behaves in sediments is unknown. Such form also reminds of the epigenetic ice wedge, which adds up to discussion on whether the layers below it were still Yedoma IC.

Next we would like to point out the language of the paper. Authors sufficiently explained most of terms related to permafrost structure to unprepared reader. However, the rest specific geological terms like magnetic susceptibility, and what the deuterium excess shows, and short essence of luminescence method, etc. From the other hand we find the term Yedoma Ice Complex very exact for the type of sediments under study, as it de-

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notes both the time of accumulation and type of sediments. The latter makes problematic to treat the sediments “salty Yedoma” as Yedoma IC at all. Authors did not provide data on water soluble salts for both exposures (despite it is mentioned in Figure captions) because “the hydrochemical data sets do not indicate relevant lithostratigraphical separations” (Page 12, Line 23) and show the increased salinity in the borehole to the reader as a specific case of non-interrupted accumulation of sediments in thawing bulb under a shallow pond, widespread during the time of accumulation. The analogous process of formation of the Holocene Cover layer above the ice wedge (see Shur, Y., Hinkel, K. M. and Nelson, F.E.: The transient layer: implications for geocryology and climate-change science, *Permafrost Periglac.*, 16, 5-17, doi: 10.1002/ppp.518, 2005) in close proximity of the sea did not have the same effect. Despite the sophisticated use of hydrochemical and isotopic data might support the hypothesis of such paleoenvironment, the rhythmic cryogenic structure of “horizontal alternations of fine lens-like reticulated and coarse lens-like reticulated interlayers and cm-thick ice bands” (Page 9, lines 24-25) did not. We doubt that epigenetic freezing (which was necessary to freeze 2 m of sediments) without inflow of marine waters or at least close occurrence of the sea could create two orders of magnitude higher salinity of the “salty Yedoma”. We recommend authors to treat the layer referred to as “salty Yedoma” as having problematic genesis, and maybe not the Yedoma at all but marine terrace of the same age as Yedoma or older since “the estimated age information from these analyses is of lower probability” (Page 10, Lines 20-21). We also suggest the corresponding conclusion should be reviewed. The paper might benefit if authors would provide the discussion on the reasons of higher salinity of Yedoma IC in BC8 compared to active layer, when during Yedoma accumulation the sea was much farther from the present.

There is a huge gap in sedimentation when looking at the BK-8 borehole, which is due to the Ice wedge interval. There are also some dates from the organic matter in the wedge itself, which “confirm that IC formation occurred between about 30 and 14 kyr BP”. However, “these dates are not present in the obtained sedimentary records and ages” (page 11, line 40). We think readers would like to check this. Overall we do

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not have enough evidence in the paper to see if the borehole disclosed the deposits of Yedoma IC. We would like to ask authors to provide more data. When compared to other sections of Yedoma, authors refer to New Siberian archipelago which is 350 km away. We recommend to include also the well-studied section Mus-Khaya on Yana River just 150 km SE of Buor-Khaya (refer to Konishchev V.N. Nature of the cyclic structure of Ice Complex, East Siberia // Earth's Cryosphere 2013 V. 17 no. 1, p. 3-16 for grain sizes and the links therein for the section structures, dating etc.)

Permafrost temperature is usually characterized with the temperature at the depth of zero annual amplitudes. Here authors did mention, that they did not reach it, since it was “around 0.1 °C at the bottom of the borehole at 18.5 m” (Page 7, Line 10). The Figure 2 allowed us to deduce that it was around -10.6 degrees Celsius. So according to the data of Romanovsky et al., 2010 it is absolutely in line with the regional temperatures for Yedoma. We suggest there is no need in discussion about the trends of temperature (Page 15, Lines 1-3) based on only three years of monitoring. As Romanovsky et al. (2010) shows: “The comparison of temperatures measured in 1984 within the Yedoma watershed landscape (Grigoriev, 1993) with recently obtained data shows a difference of 0.18C, and that is within the accuracy of measurements during the 1980s”.

Technical comments.

Some of the comments below contain questions. We expect authors would understand those questions as ambiguity of understanding the readers might face and improve the text rather than give answers.

P.7 L.14 Does “the cliff edge” means “the top of the cliff”?

P.7 L.23 “Fine sandy silt” please give reference to soil classification used in the paper

P.9 L.15-18 How would authors reason the absence of the 2 m thick cover layer in the exposure?

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P.10 L.25 Please provide data on Dsal – salinity – dry residue in mg/l

P.11 L.11 No data shown or discussed for Fe and Mn

P.11 L.37 References given are not the first works on thermokarst initiation times (Russian co-authors could provide links to the works of Kaplina, Lozhkin from 1980s. The English references could be found in the Proceedings of International conferences on Permafrost (<http://ipa.arcticportal.org/meetings/international-conferences>))

P.12 L.7-9 Disagreement: Please, use one of the terms either infinite or non-finite dates. Also infinite ones as shown in Table 2 and Figure 13 are only presented in Buo-04.

P.12 L.16-17 Tell readers more about that. How could it be random when authors know the coordinates and the absolute height?

P.12 L.30-31, L.39-41, P. 14 L.27-29 We would ask You not to use the link to this exact unpublished data because during the preparation it might change, be corrected, etc. The latter remark is critical for acception/rejection of the hypothesys "of salty Yedoma", so I suggest not to use the links to unpublished data here especially.

P.12 L.36-37 Please use the following reference Konishchev V.N. (2002) paleotemperature conditions of formation and deformation of Ice Complex Layers // Earh's Cryosphere 2002, V. 6, no. 1. P. 17-24 or any other earlier reference.

P.13 L.23-24, L.29-30 Significant repeats in these lines

P.14 L.13-14 Please provide more information about the described freezing process. Was it a single layer which froze, or cyclic accumulation/freezing episodes?

P.14 L. 20-21 So, that means this part is epigenetic permafrost and should not be considered as Yedoma IC. One-dimensional freezing usually comes with migration of cations/anions from the freezing fringe, however the salt source is needed. Please refer to Chuvilin, E. M.: Migration of ions of chemical elements in freezing and frozen

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soils, *Polar Rec.*, 35, doi: 10.1017/S0032247400026346, 59-66, 1999.

Interactive comment on *Biogeosciences Discuss.*, doi:10.5194/bg-2016-283, 2016.

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