

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

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REVIEW

Lutz Schirrmeyer, Georg Schwamborn, Pier Paul Overduin, Jens Strauss, Margret C. Fuchs, Mikhail Grigoriev, Irina Yakshina, Janet Rethemeyer, Elisabeth Dietze, Sebastian Wetterich “YEDOMA ICE COMPLEX OF THE BUOR KHAYA PENINSULA (SOUTHERN LAPTEV SEA)”

This manuscript is based on complex study of Yedoma deposits of the Buor Khaya Peninsula and provides unique information on Yedoma structure and properties. The paper makes a significant contribution to circum-Arctic Yedoma studies. The manuscript is clearly written and contains valuable information and I strongly support its publication. However, it requires some revision. My comments and recommendations are listed below:

Thank you for taking time and effort to review the manuscript. We found your suggestions helpful and adopted most of your recommendations.

Page 1, Title, Line 19, and further. I recommend to use Yedoma instead of Yedoma Ice Complex or Yedoma IC, since these two terms (Yedoma and Ice Complex) are synonyms.

We decided to use the more specific Yedoma Ice Complex (IC) term in stratigraphic sense, because of other and older Ice Complex deposits that were already studied in the Laptev Sea region

- e.g. Buchchagy Ice Complex, see Wetterich et al., 2016 Ice Complex permafrost of MIS5 age in the Dmitry Laptev Strait coastal region (East Siberian Arctic), DOI: 10.1016/j.quascirev.2015.11.016, and*
- and Late Saalian Ice Complex, see Andreev et al. 2004: Late Saalian and Eemian palaeoenvironmental history of the Bol'shoi Lyakhovskiy Island (Laptev Sea region, Arctic Siberia) DOI: 10.1080/03009480410001974*

Page 1, Lines 17-18. I'm not sure it makes sense to write about these two methods in Abstract, especially when you mention below that “Two Yedoma IC exposures and one drill core were studied. . .” (Line 20).

We changed the abstract accordingly. And the sentence is now:

In this study, we investigate late Pleistocene permafrost at the western coast of the Buor Khaya Peninsula in the south-central Laptev Sea (Siberia); namely prominent eastern Siberian Yedoma Ice Complex (IC).

Page 1, Line 31. I recommend to explain term “proluvial” (or replace it with alluvial fan) since this term is not widely used in many countries (in US, for example).

We changed the sentence accordingly.

Page 1, Lines 36-37. I recommend to remove this sentence from the abstract (also see my comment for Page 14, Lines 21-24).

Local freezing conditions on Buor Khaya, however, differed, and created solute-enriched (salty) and isotopically-light pore water pointing to a small talik layer and thaw-bulb freezing after deposition.

We would like to leave this sentence like this. Because the characteristic of the saline layer is an important results of this study that should be mentioned in the abstract.

Page 2, Line 12. I recommend to add references, since you wrote “commonly found. . .”

We added here the reference Schirrmeyer et al. (2011b) that summarizes and reviews results from various Yedoma IC study sites in the Laptev Sea region and on the New Siberian Archipelago.

Page 2, Lines 16-18. I recommend to rewrite this sentence – it is not very clear.

The sentence was rewritten and simplified:

The modern periglacial surface exposes Yedoma IC remnants in Alaskan and East Siberian lowlands next to widely-distributed thermokarst basins (alasses). They were formed after Lateglacial and Holocene warming had eroded up to 70% of the original IC distribution by thawing in an area of more than 1,000,000 km² (Strauss et al., 2013).

Page 2, Line 19. I recommend to explain what do you mean by 85% – modern (mostly ice-wedge degradation), Late Glacial/Early Holocene thermokarst (alasses), or both?

The sentence was changed to:

*On the Buor Khaya Peninsula, 85% of the land surface is affected by **Lateglacial to Holocene and modern** thermokarst (Morgenstern et al., under review).*

Page 2, Lines 25-26 and further. — see my comment Page 4, Line 15.

We added two references to the sentence about the cryolithostructure in the method chapter (3.1. Fieldwork):

*The sediments and cryolithological structures were surveyed, described **(according to French and Shur. 2011 and Katasonov, 1975)**, photographed, and sketched.*

Page 3, Lines 12-17. You cite several papers on Yedoma in Alaska and Canada, but there are much more publications on Yedoma in Siberia. I recommend to add several more citations (major papers on Yedoma in Central and Northern Yakutia – the areas adjacent to your study area).

We added the sentence:

But studies of Yedoma IC in northeastern Siberia started already several decades earlier (e.g. Tomirdiario (1980), Kaplina (1981), Gubin and Zanina (2013, 2014) and quotations therein).

Page 3, Lines 22-23. I recommend to move this sentence to the next section (Study Site)

We changed it accordingly.

Page 4, Line 15 and further. What classification(s) did you use for descriptions of cryostructures? Unfortunately, your descriptions of cryostructures are not consistent through the manuscript, including Figure 3 legend (lens-like/lenticular, lattice-like/net-like/reticular/reticulate, banded/layered, etc.).

We added two references to the sentence about the cryolithostructure in the method chapter (3.1. Fieldwork):

*The sediments and cryolithological structures were surveyed, described **(according to French and Shur. 2011 and Katasonov, 1975)**, photographed, and sketched.*

Page 4, Lines 22-23 and further (e.g. Page 7, Line 36). There is no absolute ice content in this glossary (van Everdingen 1998). Do you mean gravimetric moisture content? In this case, it should be calculated on a dry-weight basis. Wet-basis gravimetric water content also may be used but this is not very common in the literature. If you want to use it here, you may cite Phillips et al. 2015 (GEOQuebec Conference).

We have cancelled the reference of van Everdingen (1998).

Page 7, section 4.2. I recommend to subdivide this section: 4.2.1. Buo-02 4.2.2. Buo-04 4.2.3. BK-8

We changed the structure of this chapter accordingly.

Page 7, Lines 21-35 and further. Cryostratigraphic descriptions are not very clear; it will be good to illustrate them with close-up photos. Line 28: I recommend not to use term “massive cryostructure” (this Russian term may be mixed up with massive ice).

We added close-up photographs of each subprofile to the supporting online material. And we changed the “massive cryostructure” to “structureless.”

Page 8, Line 2 and further. What do you mean by 80.6%? Mean explained variances for these five members make 100% in Table 1. If you mean R² total mean (0.806), this should be explained (probably in Methods).

The 80.6 % refers to mean total variance of the original data that can be explained by the EM model. It is calculated by the correlation of the original data against the dataset modelled by the averages of the robust end-members multiplied by 100. The explained variances of single EMs are scaled to sum to 100 % and refer to how much variance of all the scores is represented by each of the single EMs. We interpret this as a measure of contribution to the EM variances, i.e. how important an EM is within all the samples (see Dietze et al., 2012 and code in R package EMMAgeo). We added more explanation on this in the method chapter and modified Table 1:

Mean rEM loadings and a weight transformation limit of zero were used to calculate mean scores (i.e. relative contribution of an end-member to each sample and their scaled variances) and to model the dataset. Then, goodness-of-fit parameters could be calculated (mean total, class and sample-wise explained variances, i.e. r² of original vs. modelled data * 100, Dietze et al., 2012).

Page 8, Line 7 and further (e.g., Page 9, Line 4). These BD numbers look too low. Such numbers are more appropriate for dry density.

Thank you for this comment. We calculated the bulk density following Strauss et al.

2012 and defined an ice content >20 wt% as threshold for ice saturation. With this assumption, the absolute ice content gives an estimation of the pore volume, which was used to determine the bulk density. The pore volume is assumed to have a density of 0 g/cm³. Thus a high ice content leads to a high pore volume that leads to a low bulk density. Dry density would be, in our opinion, not the right word for this density. The lowest range of our bulk densities is not uncommon for organic soils.

Page 8, Line 22 and further (e.g., Page 9, Line 19). These numbers doesn't match Figure 7.

Figure 7 was redrawn, but it is only a schematic figure in order to show the internal relations within such a Yedoma Ice Complex profile.

Page 9, Lines 11-12 and further (e.g., Page 9, Line 22). Are you talking about five or six segments? You show six units in Figure 7, but here you describe five segments only.

This sentence was completed:

... the BK-8 core was sub-divided in the field into five segments completed by a sixth segment characterized by specific pore-water composition (Figure 7).

Furthermore we added the sentence:

The sixth segment is a subsequent part of segment five only visible in hydrochemical data and will be described more detailed in chapter 4.4..

Page 9, Line 13. I recommend to mention the vertical foliation and color of ice.

We changed it accordingly:

Between 3.4 and 8.35 m bs (30.6-25.65 m asl) the core cut an ice wedge of clearly colourless ice with gas bubbles <1 mm in diameter and parallel-oriented vertical sediment stripes up to 5 mm thick.

Page 9, Line 26, 34, Figures 4, 6, 8. It looks pretty strange: moisture contents in this core are MUCH higher in comparison with both exposures (see Figures 4, 6, and 8), while you describe similar ice-rich soils in all your sections. BD for BK-8 are also much higher (see the same figures), though it is impossible for soils with higher moisture contents. Definitely something is wrong here. I strongly recommend you to check all numbers for moisture content and density (see also my comments for Page 4, Lines 22-23 and Page 8, Line 7).

The absolute ice content in Buo-04 is between 22 and 60 wt%, in Buo-02 30-90 wt%, and in BK-8 between ~ 60-90 wt%. That means, the ranges are overlapping. There are two explanations for this:

- 1. The higher values in the BK-8 are favored by the ice wedge nearby.*
- 2. Sample-bias, because one is surely not the large ice lenses in the profile wall, if one is interested in the sediment. There is no choice when drilling. For the profiles the ice contents were measured in 2 ways (aluminum boxes in the field, frozen samples in the laboratory), the orders of magnitude corresponded.*

Page 10, Lines 6-7. I recommend to add this information to Table 2.

We changed it accordingly and added the following sentence in the capture of table 2:

The grey marked samples are from the ice wedge in BK-8.

Page 11, Discussion. I recommend to subdivide this section in a different way: 5.1. Chronostratigraphy 5.2. Cryostratigraphy and analytical data interpretation 5.3. End-member analysis ????? 5.4. Polygenetic origin of Yedoma soils ????? 5.2 → 5.5. Regional context of Yedoma Ice Complex in eastern Siberia

Thank you. We changed the structure of this chapter accordingly.

Page 11, Line 35. You didn't mention the youngest age: 5.1 ± 0.05 (Table 2)

We added a sentence in the chapter 4.3 Dating:

The four ages from the ice wedge showed a broad range, between 22.1 ± 0.1 and 5.1 ± 0.05 kyr BP. The youngest age can be considered as contamination during the drilling process.

Page 12, Lines 15-17. These sentences are not very clear.

We cancelled this sentence.

Page 12, Line 18. Wrong numbers – it should be Figures 3 and 5 or Figures 3 to 6.

We changed it to (Figures 3 to 6).

Page 13, Lines 23-38. I recommend to move these lines to the end of this section and to add references (for the concept of polygenetic Yedoma formation). I also recommend you not to mention glacial hypothesis (Line 34), since almost nobody supports it now.

We changed it accordingly and added the reference (Schirmeister et al., 2013).

Page 14, Lines 21-24. 2.5 m is too deep for the active layer in silt, and the wet climate does not support deep seasonal thawing. Maybe it's better to interpret this unit as a refrozen shallow talik. An analysis of cryostructures may help to determine freezing conditions. According to your description (Page 9), this unit looks like ice-rich syngenetic permafrost, but it's hard to tell without detailed sketches and photos.

We changed the sentence to:

During the warm and moist summer conditions of the MIS 3 interstadial period (Andreev et al., 2011, Wetterich et al., 2014) a talik horizon more than 2.5 m thick developed that then re-froze.

Furthermore, in SOM 5 (former SOM2) "Cryolithological examples of different units of the BK-8 permafrost core" the saline layer is represented.

Page 25, Table 1. "... performance of original vs. modelled data (using the mean robust end-members)" – This is not explained in Methods.

We added more information in the method chapter (see above) and slightly adapted the heading of the Table and expressed the performance (goodness-of-fit) also as explained variance (class, sample-wise and mean total $R^2 \cdot 100$) to be consistent with the text: Comparison of robust grain-size end-members, their explained variances of scores and performance of original vs. modelled data (using the mean robust end-members, mean $R^2 \cdot 100$)

Conclusions (File bg-2016-283-AC1). I suggest two small changes: 1. The timing of IC formation as well as its cryolithological properties (and temperature) are similar to known Yedoma IC sequences in the Laptev Sea region and beyond. —I recommend to remove "and temperature" 2. Three robust end-members (rEMs) represent fine silt, medium silt, and fine sand fractions in grain-size distribution data (and support) WHICH SUPPORTS THE CONCEPT OF MULTIPLE transport and depositional processes. . .

We changed the sentences accordingly

Figures 1-8. I suggest several small changes – see attached file.

We have incorporated all the changes accordingly

Figure 13. For BK-8, I recommend to use different symbols for the dates obtained from soils and ice wedge.

Thank you. We changed it accordingly.

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 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.

The paper is part of the BG Special Issue on "Climate-carbon-cryosphere interactions in the East Siberian Arctic Ocean: past, present and future". And we mentioned already in chapter 1 (Introduction) the role of the Yedoma IC in the study region:

Eroding Yedoma IC deposits are considered as a major source of terrigenous organic carbon transfer into the Arctic Ocean (Vonk et al., 2012, Winterfeld et al., 2015, Bröder et al., 2016) and represent the terrestrial end-member of organic matter (OM) on the East Siberian Arctic Shelf (Bischoff et al., 2016).

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.

We believe that the presentation of single profiles and core data provides insights into the internal variations covering different periods of Yedoma IC deposition on Buor Khaya Peninsula. A generalization of all data into one cross section would not correspond to the observed variability in proxy data. However, to meet the reviewer's recommendation, we summarized mean and standard deviation data in SOM 1 and added the according information in chapters 4.2.1 to 4.2.3

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.

Thank you. Figure 7 was redrawn to highlight the syngenetic character of Yedoma Ice Complex.

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.

The magnetic susceptibility was already explained in Chapter 3.2 (Laboratory work): "Mass-specific magnetic susceptibility (MS) indicative of magnetic and magnetisable minerals was

measured using Bartington MS2 instruments equipped with the MS2B sensor type.”
We added a sentence to chapter 3.2 (Laboratory work): The deuterium excess d (Dansgaard, 1964) reflecting isotopic fractionation or exchange processes was calculated by $d = \delta D - 8\delta^{18}O$. Concerning the luminescence method we completed the sentence in chapter 3.2 (Laboratory work) to:

Three frozen segments of the drill core were separated for luminescence dating in order to determine sediment deposition ages (e.g. Aitken 1998). Luminescence dating was applied because the method allows dating of material beyond the limits of radiocarbon dating or evaluating results, when the radiocarbon method reaches the non-finite range of ages.

From the other hand we find the term Yedoma Ice Complex very exact for the type of sediments under study, as it denotes both the time of accumulation and type of sediments.

Thank you.

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.

We changed the term of “salty Yedoma” to “saline layer” which was probably formed as a small talik below a deeper polygonal pond in order to separate this core segments.

That is the reason why we wrote in the abstract:

Local freezing conditions on Buor Khaya, however, differed, and created solute-enriched (salty) and isotopically-light pore water pointing to a small talik and thaw-bulb freezing after deposition.

as well as in the discussion chapter:

During the warm and moist summer conditions of the MIS 3 interstadial period (Andreev et al., 2011, Wetterich et al., 2014) a talik horizon more than 2.5 m thick developed that then re-froze.

For clarification: we do not suggest that sediments accumulated in a thaw bulb, but rather that thawing occurred following sediment accumulation. The original deposition regime may have been syngenetic, but the reviewer is correct (below) that the profile gives evidence of post-depositional epigenetic permafrost.

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 agree with the reviewer, that there is no indication for the effect of the proximity of the sea in the Holocene cover layer above the ice wedge. We also, however, assert that there is no evidence for proximity of the sea for the lowermost saline layers in the core BK8. Neither the composition of the solution nor the distribution of solutes vertically indicates a marine source.

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 changed the term of “salty Yedoma” to “salty layer” which was probably formed as a small talik below a deeper polygonal pond in order to separate this core segments.

However, a marine terrace is difficult to imagine, since the coast in the late Pleistocene was more than hundred kilometers further north. We have added sentences to the discussion explaining the problem of salt source and distribution:

The presence of high but variable concentrations of solutes in the pore water throughout this layer is generally consistent with concentration effects in slowly freezing soils (Chuvilin, 1999), but the source of the solutes is not clear, since their concentration exceeds what would be available in the Yedoma IC sequence.

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.

We have expanded our discussion of why we think the source of the salt is NOT marine.

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 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.

BK8 encloses sediments from the onset of Yedoma IC formation (>50ka BP) to its youngest stages (about 10ka BP), which is confirmed by numerical dating and hence, very likely provides strong evidence of covering the whole formation period. We disagree with the need of further data as also two coastal exposures complement the picture.

Yedoma IC deposits by definition contain syngenetic ice wedges, which commonly disturb the continuous sediment sequence. By dating organic remains from the ice wedge we were able to capture periods of Yedoma IC deposition which are not represented in our sample set to enhance the understanding of Yedoma IC formation on Buor Khaya Peninsula.

The present data set derives from expeditions in 2010 and 2012. Since then further sampling campaigns have not been undertaken at this location. Therefore, we feel unable to answer the non-specific request of the reviewer to provide more data. All available data from core BK-8 are presented in the study. In addition, the coastal sections Buo-02 and Buo-04 are included. The deduced overall period of Yedoma IC formation spans from > 55 kyr BP up to about 10 kyr BP (see Figure 13), which most likely disclosed the deposition time of the Ice Complex.

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.).

Thank you for this comment. We added Konishchev (2013) as reference

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.

Thank you for this comment. We added the reference proposed by the reviewer:

According to the data of Romanovsky et al. (2010) our measured temperature is in line with the regional temperatures for Yedoma IC.

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 0.18°C, and that is within the accuracy of measurements during the 1980s”.

We decided to leave this part of the discussion, because the temperature chain is still measuring.

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”?

No, this is only the end of the coastal rim, not the highest position.-

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

We added one sentence to the chapter 3.2 (Laboratory work:

The used grain-size classification is according to Reineck and Sing (1980) and Wentworth (1922).

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

This is because of the situation of the coastal exposures, which exposed only the sediments directly at the wall. Further behind (as shown at the thermokarst mound in Figure 5) there would be also a Lateglacial-Holocene cover, which was not studied during field work in 2010.

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

Salinity is usually determined using electrical conductivity methods, as was done for samples from the exposures here (Buo-2, Buo-4). For these samples we can estimate dry residue, but only by making assumptions regarding the composition of the pore water solution. Since the composition is important for our analysis, we prefer not to take this step.

However, we can calculate dry residue for samples for which major ion concentrations have been measured (BK8) by summing the determined concentrations. These values may not be equivalent to what is obtained via dry residue methods. We have added these values in parentheses to the description of BK8 hydrochemistry.

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

We have removed mentions of Fe and Mn from the text since they do not add anything to the interpretation, and since they too dilute to be visible on the figure.

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 ()

We added some Russian references and changed the sentence accordingly:

The thermokarst onset during the Lateglacial period is a commonly observed feature of late Quaternary permafrost stratigraphies (Kaplina and Lozhkin 1979, Kaplina, 1981, Romanovskii et al., 2000, Wetterich et al., 2009, Walter Anthony et al., 2014).

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.

Thank you. We decided to use the term “non-finite” data generally.

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

This sentence was poorly worded. We agree that “random” is not a good word choice – we have changed the sentence to read:

The core material is also limited since it is difficult to choose a drilling location from the ground surface that gives a representative sample and avoids ice wedges. This study attempts to combine the advantages of both stratigraphic sampling methods.

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 acceptance/rejection of the hypothesis "of salty Yedoma", so I suggest not to use the links to unpublished data here especially.

Zimmermann et al.2016 is now published and we updated this reference.

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

Thank you for this reference, which is now incorporated.

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

L23-25: A comprehensive cryolithogenetic concept of polygenetic Yedoma formation combines cryogenic weathering, periglacial material transport and accumulation, and relief shaping under cold-arid climate conditions and considers two general formation processes

L29-30: The Yedoma IC formation comprises cryogenic processes, for instance cryogenic weathering, ice segregation, syngenetic ice wedge growth, secondary sediment deformation, and reworking due to cryoturbation.

The first sentence and the following both were moved to the end of this chapter as a sub-conclusion.

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?

We have changed terminology to refer to the freezing of a talik horizon rather than an active layer. This is also consistent with a comment from the other reviewer and changed the structure of this paragraph to better describe the freezing process

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

We changed the name of this horizon to "saline layer" in Figs. 7, 8 and 11 as well as in the text and added the reference of Chuvilin (1999). We agree with the reviewer that "epigenetic" may apply to this layer– it might have been sedimentologically syngenetic, but thawed and re-froze – we have added words to this effect. We have also added a sentence acknowledging that reasons for the distribution of cations/anions in the lower saline layer and the source for the salt are unknown.