Interactive comment on “Ground-ice stable isotopes and cryostratigraphy reflect late Quaternary palaeoclimate in the Northeast Siberian Arctic (Oyogos Yar coast, Dmitry Laptev Strait)” by Thomas Opel et al.

M. Kanevskiy (Referee)

mkanevskiy@alaska.edu

Dear Mikhail Kanevskiy,

Thank you for your thorough review which has raised a couple of good points to improve our manuscript. Please find our replies to your referee comments below in → blue italics.

REVIEW

Thomas Opel, Sebastian Wetterich, Hanno Meyer, Alexander Yu. Dereviagin, Margret C. Fuchs, and Lutz Schirrmeister “Ground-ice stable isotopes and cryostratigraphy reflect late Quaternary palaeoclimate in the Northeast Siberian Arctic (Oyogos Yar coast, Dmitry Laptev Strait)”

This manuscript is focused on reconstruction of palaeoclimate and palaeonvironmental conditions in the Northeast Siberian Arctic over the last 200,000 years. It is based on complex study of perennially frozen Quaternary deposits of the Dmitry Laptev Strait region. The study included descriptions and sampling of coastal exposures, estimation of ice content, radiocarbon and IRSL dating, and stable-isotope analysis.

This study was performed by highly qualified and experienced team of researchers who have published numerous outstanding papers on cryostratigraphy of Quaternary deposits and paleoecology of Northeast Siberia. The paper makes a valuable contribution to our knowledge of permafrost history of this region and provides unique information on structure and properties of Quaternary deposits.

The manuscript is clearly written, the results of study are properly interpreted, and I strongly support publication of this paper. However, it requires some revision. My comments and recommendations are listed below (more comments and suggestions are provided in the attached file).

→ Thank you for this positive evaluation!

General comments:

My major concerns are related mostly to terminology and descriptions of cryostructures.

1. In the manuscript, you often use the terms “Ice Complex” and “Yedoma” but didn’t give definitions of these terms, which are commonly considered to be synonyms (e.g., Schirrmeister et al., 2013, in Encyclopedia of Quaternary Science). I understand that here you have to use both terms to describe similar deposits of different ages (following Tumskoy, 2012) but it should be explained. I recommend to discuss these terms in the Introduction, Page 2, after Line 6.

→ The term ‘Yedoma’ is through the paper only used in stratigraphic terms as Yedoma Ice Complex. We added a general explanation of the Ice Complex as follows in the Introduction (section 1): “Ice

Complex (IC) deposits formed in polygonal tundra environments with syngenetic ice-wedge growth during different periods of the late Quaternary in non-glaciated Beringia (Tumskoy, 2012; Schirrmeister et al., 2013). The most prominent IC of late Pleistocene age is named Yedoma IC (MIS4-3), but older IC formations are known such as the Yukagir IC of MIS7 age (Schirrmeister et al., 2002) and the Buchchagy IC of MIS5 age (Wetterich et al., 2016).“

2. Through the manuscript, you often use the term “stable-water isotope composition of ice”, but I don’t think you should mention “water” since you’re talking about ice. I recommend to use “stable-isotope composition of ice” instead.

→ We deleted „water“ accordingly.

3. I recommend you not to use the term “Permafrost deposits” (e.g., Page 2, Lines 2 and 4). It’s better to use either “permafrost” or “perennially-frozen deposits.”

→ We changed the terms and use only “permafrost” instead of “permafrost deposits”.

4. I recommend you not to use the term “thermokarst deposits” (e.g., page 4, Line 20). It’s better to use either “thaw-lake” or “thermokarst-lake” deposits.

→ We changed the terms and use now “thermokarst-lake deposits” and “thermokarst basin palustrine deposits” to differentiate between lacustrine and boggy deposition regimes and units.

5. I recommend you not to use the term “texture ice” (it is used everywhere in the paper). This term is probably originated from “teksturoobrazuyushchiy (texture-forming) ice,” which is common in Russian literature, where it is associated with the term “cryotexture.” The latter is widely used in Russia, but not in the international literature, where the term “cryostructure” is used instead (see van Everdingen 1998). I recommend you to use “ice lenses” or “pore and segregated ice” or just “segregated ice” instead of “texture ice.”

→ Thank you for this advice. We followed your suggestion and replaced “texture ice” by “pore and segregated ice”.

6. Cryostratigraphic descriptions are not very detailed (though there is “cryostratigraphy” in the title of your manuscript), and it will be good to illustrate them with close-up photos.

→ Following your recommendation, we extended the cryostratigraphic descriptions by more details in the Results section 4.1 although close-up in high quality suitable for publication are unfortunately not available for all studied units. We therefore feel unable to provide illustrations as suggested.

7. Descriptions of cryostructures are not consistent. You stated (Page 5, lines 4-5) that your descriptions are based on classification suggested by French and Shur (2010) (actually, this classification was proposed by Murton and French, 1994, and I also recommend you to cite Murton, 2013, in Treatise on Geomorphology), but sometimes you use different terms (e.g., massive cryostructure, lens-like reticulated cryostructure).

→ Changed accordingly. We added the recommended references by Murton and French (1994) and Murton (2013) in Fieldwork section 3.1. We furthermore made the descriptions consistent to the referred literature.

8. I don't recommend you to use the Russian term "massive cryostructure" because it may be mixed up with massive ice. It is equivalent to "structureless cryostructure" (Murton and French 1994); French and Shur 2010), or "pore cryostructure" (Shur and Jorgenson 1998). I recommend you to name this cryostructure "pore (structureless)" after Murton 2013, and mention that such sediments do not contain any inclusions of ice visible by naked eye.

→ Changed accordingly.

9. For the ice content of different units (Section 4.1), you give only the range of values. It will be good to add a table with the ice-content data for each unit, including the average values ± SD, number of samples, etc.

→ We followed your suggestion and added a table (Table 2) with minimum, mean, maximum values, standard deviation and number of samples per stratigraphic unit.

10. Several times you mentioned pollen-based temperature reconstructions (with references to previous studies), but did not provide any information. I believe the manuscript will benefit if you add a short summary or a table with the pollen data for each unit (such table may be based on Table 2 from Andreev et al. 2011).

→ Pollen data are unfortunately not available for all units of the Oyogos Yar study site, only for some (Wetterich et al., 2009, 2016). All other pollen information originates from Bol'shoy Lyakhovsky Island. Therefore, we decided not to include detailed pollen information into this study. As the focus of this study is the ground ice (stable isotopes and cryostructures), we believe that references to previous studies are sufficient.

11. Your descriptions of different units (Section 4.1) are not consistent and in many cases incomplete. I recommend you to use the following pattern, uniform for all units: 1) description of sediments (soil type and origin, color, thickness, lamination, inclusions, etc.); 2) ground ice: a) prevailing cryostructures (type, if possible – thickness of ice lenses and spacing between them, and/or photos of cryostructures; gravimetric ice content and, if possible, visible-ice content), b) massive-ice bodies (for ice wedges: width, vertical extent, color, bubbles, inclusions, type – syn- or epigenetic, if possible – volumetric content of wedge ice); 3) age of sediments.

→ See also our reply to point 6 above. We extended the cryostratigraphic descriptions and followed the recommended structure to ensure consistency and enhance readability.

Specific comments and suggestions:

Page 2, Lines 25-27. ". . .ice. . .originates from freezing of soil moisture in the seasonally thawed active layer" – This statement applies only to syngenetic permafrost. ". . .melt water of the active layer ice" – do you mean "from the degrading upper permafrost?" (water of the active layer usually forms from summer and winter precipitation, and you already mentioned these sources in the same sentence).

→ As suggested in the edited supplement we added "in syngenetic permafrost" to the first sentence. We decided to keep the second sentence in its current form as melt water of the thawed active layer contributes to the soil moisture as well as winter and summer precipitation (we just added a

"thawed" to active layer). The soil moisture resulting from the different sources is then subject to evaporation and freeze-thaw cycles.

Page 3, Lines 29-32 – Page 4, Lines 1-6. Since you are talking about more than 100 years of studies, it will be good to add several more references, e.g. Bunge 1887, Toll 1897, and of course Romanovskii 1958.

→ We added references to Bunge 1887, Toll 1897 and Romanovskii 1958.

Page 4, Lines 7-8. I recommend you to rewrite this sentence – it is not very clear.

→ We removed the sentence from the text.

Page 4, Lines 9-11. You mentioned here such terms as "thermokarst" and "taberite" but did not provide any references (for "taberite," I recommend Kaplina 1987, 2009, Romanovskii 1993 or other Russian sources).

→ Changed accordingly. We added the reference of Kaplina (2009) which is at least available online and contains a comprehensive overview of Soviet and Russian literature on thermokarst and taberites.

Page 4, Lines 13-14. I recommend you to rewrite this sentence – it is not very clear.

→ We rephrased the sentence to make it clearer.

Page 7, Line 11. I recommend you to replace "cryolithological" units with "cryostratigraphic."

→ Changed accordingly.

Page 8, Line 4. Please check spelling: according to Tumskoy 2012, it is Bychchygyi (or Bychchygyiskaya – if you transliterate it from Russian) Suite.

→ We would prefer to keep the term as spelled in the English publication of Wetterich et al. (2016) co-authored by V. Tumskoy.

Page 8, Line 22 (and at several other places). Lens-like reticulated cryostructure – does it mean poorly developed reticulate cryostructure? The term "coarse" should be explained, otherwise you can just present thickness of ice lenses and spacing between them (range in mm). Close-up photos of typical cryostructures will be very useful.

→ According to Murton and French (1994), we changed the term "lens-like reticulated" to "irregular reticulated" in the revised cryostratigraphic descriptions. The term "coarse" refers ice lens thickness > 1 mm. We added accordingly this information in brackets to the descriptions. As written in our reply to point 6 close-up in high quality suitable for publication are unfortunately not available for all studied units. We therefore feel unable to provide illustrations as suggested.

Page 9, Lines 17-18. "Unit V is completed by a paleosol layer . . ." – Do you mean this paleosol layer is located on top of Unit V?

→ Yes, the paleosol layer is the topmost layer of Unit V. We rephrased this sentence to make this clearer.

Page 9, Line 25. There should be ice wedges in this unit, at least epigenetic (see Fig.2)

→ Yes, partly the toes of epigenetic ice wedges (related to Unit VII) could be found. We added this information.

Pages 9-10. 4.1.7 Unit VII. Description of this unit is incomplete. Please describe the peat horizon and present its thickness (Page 10, Line 7).

→ Changed accordingly.

Page 10, Lines 4-5. I recommend you to rewrite this sentence – it is not very clear.

→ We rephrased this sentence.

Page 10, Lines 10-11. "Unit VIII was only found in places and associated with initial thermokarst." – Please clarify.

→ We refer to the fillings of small initial thermokarst ponds (bylary). We rephrased the sentence accordingly.

Page 10, Line 12. How could you identify syngenetic wedges? Small wedges on top of Yedoma sections are mainly epigenetic. What was the vertical extent of these wedges? Anyway, the thickness of this unit is so small that it's really difficult to recognize the nature of wedges (syngenetic vs epigenetic).

→ The small syngenetic ice wedges were related to the fillings of small initial thermokarst ponds (bylary) and therefore easy to identify. We rephrased the sentence

Page 10, Lines 13-15. "Moreover, Holocene cracking activity characterised by milky white ice veins was also observed in the upper parts of the huge ice wedges of the Yedoma Ice Complex". You already mentioned (see the previous sentence) that the Holocene ice wedges penetrate into the Yedoma wedges. If you mean something different, you should clarify this.

→ We found in the ice wedges of the upper part of Unit IV (Yedoma IC) several milky-white ice veins of Holocene origin (i.e. epigenetic to the Yedoma Ice Complex ice wedges). We rephrased this sentence. This finding could be confirmed by the respective stable-isotope values.

Page 14, Line 18. A presence of what? Zyryanian floodplain deposits?

→ Yes. We rephrased this sentence.

Page 14, Lines 25-34. Please clarify: are you talking about the gap in sedimentation or presume that Yedoma formation continued till the end of the Pleistocene, but the upper part of the sequence was eroded later (or consider both options).

→ Both options seem to be possible. Based on our available data it is not possible to solve this question entirely. We rewrote this paragraph and referred to Cape Mamontov Klyk and Bykovsky Peninsula as sites with Yedoma IC formation until the end of the Pleistocene.

Page 15, Lines 3-4. "Dated taberal Ice Complex deposits of unit V and the overlying lacustrine deposits of units VI and VII prove widespread permafrost degradation related to the development of

vast thermokarst basins during the last deglaciation." – Dates from taberal deposits are not related to thermokarst development and reflect only the time of Yedoma formation.

→ You are of course right. We changed the sentence to correct this.

Page 15, Lines 13-14. "A predominantly lateral ice-wedge growth in the last two millennia can be concluded from radiocarbon ages of actively growing ice wedges of unit VII (Table 4) indicating rather stable surfaces in the thermokarst basins with low accumulation." – First, it is not very clear, how did you come to this conclusion. Do you have the data on radiocarbon ages of peat in thaw-lake basins? Second, in Table 4, there is no information on depth of sampling.

→ All samples were taken in a similar depth (about 1-1.5 m below surface). We added this information to the figure caption. Five out of 19 samples had to be excluded as they represented redistributed material. The remaining samples indicate the lateral ice-wedge growth (oldest samples in the outer parts, youngest samples in the central parts of the studies ice wedges). Together with the youngest age of palustrine deposits (3.6 kyr b2k) and the shape of the ice wedges this leads to the conclusion of rather stable surface conditions and a predominantnly lateral ice-wedge growth.

We rewrote the paragraph and added a reference to a paper addressing this in more detail (Opel et al., 2017).

Page 16, Lines 2-5. This explanation looks rather strange. Do you mean that the entire 6-m-thick layer was freezing from below just in one event? If so, I don't think this explanation is correct. Syngenetic permafrost formation is going rather slowly, step by step, and only small portions of the active layer (its basal horizon) join the permafrost at a time, following a slow rising of the permafrost table.

→ You are right. It is very unlikely that the entire layer was freezing from below in just one event. There may have been a lot of events as well freezing from above. However, as this issue is not finally resolvable using our data we decided to remove the hint to the freezing direction and just state that the distinct $\delta^{18}O/\delta D$ minimum and the respective d excess maximum are the result of freezing and does not reflect climate signals.

Page 16, Lines 10-12. I recommend you to rewrite this sentence – it is not very clear.

→ We deleted this sentence.

Page 16, Lines 20-21. In case of very slow sedimentation, and especially gaps in sedimentation, age offsets may be much more than few hundreds to few thousands years.

→ We changed the wording slightly to make clear that we discuss age offsets between host sediments and syngenetic ice wedges with reference to a distinct altitude level within a unit such as the Yedoma Ice Complex (Unit IV). However, considering the mentioned very slow sedimentation or gaps in sedimentation, we agree that age offsets may be much bigger.

Page 16, Lines 27-28. I'm not sure think this explanation is correct. If deposition occurs at the floodplain, we may presume that frost cracks are filled mainly during the spring flooding, so amount of snow is not so important.

→ We assume that frost cracks may be already filled before spring flooding. Nevertheless, we agree that the availability of sufficient sedimentary material may be more important than the amount of snow. We therefore deleted the reference to dry conditions.

Page 16, Line 31. What do you mean by "rounded"? Concave, convex?

→ We added convex.

Page 17, Line 7. What do you mean by ". . .indicate very cold winter climate for the initiation of ice-wedge genesis. . .?" Cold climate during the early stage of ice-wedge formation?

→ Yes, we accepted your suggestion.

Page 17, Lines 22-23. "The variability of stable-isotope values with respect to altitude indicates changing conditions from very cold to moderate winter temperatures." – For what unit(s)? Fig. 5 doesn't show big changes in stable-isotope values for Unit IV.

→ This statement refers indeed to Unit IV (Figure 5) that shows significantly varying stable-isotope compositions. We added a reference to Figure 5 to this sentence as well as our tentative classification scheme based on $\delta^{18}O$ values (extremely cold: -38‰ to -35‰, very cold: -35‰ to -32‰, cold: -32‰ to -29‰, moderate -29‰ to -26‰, warm: -26‰ to -23‰, very warm: -23‰ to -20‰) to the discussion.

Page 18, Line 2. I recommend you to explain that mean values for Unit VII (Fig. 6a) were calculated without taking into account values obtained from modern and recent wedges, which are shown in this figure separately.

→ This is not the case as the high-resolution horizontal ice-wedge profiles contain also recent and modern ice veins. Therefore, we decided not to follow your suggestion.

Page 19, Lines 33-34. I recommend you to rewrite this sentence – it is not very clear.

→ We rewrote this sentence to make it clearer.

Page 21, Lines 35-36. Please transliterate the title of this paper.

→ Changed accordingly.

Page 26, Line 3 and Table 1. I recommend to present information on epigenetic ice wedges as well.

→ We haven't considered epigenetic ice wedges in this study due to their complicated chronological attribution. Therefore we decided not to follow your recommendation for Table 1. However, in the description of Unit III and Unit V we added the information on epigenetic ice wedges, presumably related to Unit IV and Unit VII, respectively.

Page 29, Table 4. Depth of sampling (or height asl) is not shown.

→ All samples were taken in a similar depth (about 1-1.5 m below surface). We added this information to the figure caption.

Page 30, Table 5, title. “Ice wedges marked with an asterisk contain samples attributed to Unit IV and Unit VIII, respectively.” – What does it mean? You didn’t mention segregated ice in the title, though you present the values in the table (texture ice).

→ As pointed out in section 4.1.8 we observed Holocene ice veins in two ice wedges of the Yedoma Ice Complex (Unit IV). As described in section 4.2.3 we attributed samples with a Holocene stable-isotope composition (i.e. $\delta^{18}O$ values higher than -28‰) to Unit VIII. Samples of the respective ice wedges (Oy7-06 IW2 and Oy7-08 IW3) are therefore attributed to Unit IV and Unit VIII and the two ice wedges are marked with an asterisk in Table 5. We added the reference to section 4.2.3 to the table caption. We additionally added “pore and segregated ice” to the caption.

Page 32, Figure 1. I recommend to add a larger scale geomorphic map of the Oyogos Yar coast (based on satellite imagery) showing yedoma remnants and alases, and the position of the coastal exposure presented in Fig. 2.

→ Thanks for this suggestion. We added such a map to Figure 1.

Page 36, Figure 6. I recommend you to explain (in the caption or in the main text) that mean values for Unit VII (Fig. 6a) were calculated without taking into account values obtained from modern and recent wedges, which are shown in this figure separately.

→ This is not the case as the high-resolution horizontal ice-wedge profiles contain also recent and modern ice veins. Therefore, we decided not to follow your suggestion.

MORE COMMENTS AND SUGGESTIONS ARE PROVIDED IN THE ATTACHED FILE.

Good luck!

→ Thank you!

Mikhail Kanevskiy, Institute of Northern Engineering University of Alaska Fairbanks

Please also note the supplement to this comment:

http://www.clim-past-discuss.net/cp-2017-1/cp-2017-1-RC1-supplement.pdf

→ Thanks for these detailed comments. We accepted most of them during revision of the manuscript.