

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.

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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 palaeoenvironmental conditions in the Northeast Siberian Arctic over the last 200,000 years. It is based on

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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).

General comments:

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

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.

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.

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.”

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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.
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.”
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.
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).
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.
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 \pm SD, number of samples, etc.

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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).
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.

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).

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.

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

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).

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

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

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

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.

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?

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

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).

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

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

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).

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.

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

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).

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.

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.

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.

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

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.

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

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flooding, so amount of snow is not so important.

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

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.

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.

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

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

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

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

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).

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.

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

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obtained from modern and recent wedges, which are shown in this figure separately.

MORE COMMENTS AND SUGGESTIONS ARE PROVIDED IN THE ATTACHED FILE.

Good luck!

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Please also note the supplement to this comment:

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

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2017-1, 2017.

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