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.

G. Iwahana (Referee)

giwahana@alaska.edu

Dear Go Iwahana,

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.

This paper describes very complicated suites of permafrost deposits. The authors analyzed a number of rare samples from remote Arctic field sites and provided new valuable dating information, then summarized and extracted essence of paleoclimate and paleoenvironmental conditions of the targeted region. This work was done systematically by highly experienced team especially for paleo-environmental studies using permafrost. Selection of sampling profiles were carefully made in the complicatedly distributed Ice-complex and thermokarst-affected terrains so that they can recover past events happened in this region. The paper was will-organized and most necessary components of the study was written precisely in details. I believe that this paper helps and guides wide range of precedent researchers who want to further explore and understand history and future not only in the Dmitry Laptev Strait area but also in other ice-rich permafrost regions. The results show that permafrost deposits on the both sides of the Dmitry Laptev Strait indicate comparable records of climate change if we consider uncertainty in age determination of older deposits. This results strengthen usage of permafrost as an important proxy of past climate and environmental change in periglacial regions. Two IRSL dating points provided large contribution to paleo-environmental study of old permafrost that cannot determine ages by radiocarbon method. I recommend this manuscript be published with minor revisions. Below, I included some suggestions that I believe they will improve the presentation of this work and help readers to grasp the authors results and conclusions.

→ Thank you for your positive evaluation!

(Minor comments, suggestions, and questions)

First two paragraphs in Introduction: There is no mention about hoar development in cracks as a possible main ice-wedge forming process and fractionation due to evaporation/sublimation after the meteoric sources of ice-wedge (snow-melt water or hoar) deposited on land surface. It would be good to interested readers to have brief explanation here why the authors excluded or didn’t mention about those processes here or in later discussion.

→ Thank you for this suggestion. Indeed, climate and site-specific environmental conditions play an important role for ice-wedge formation, i.e. frost-crack filling. It is, however, commonly accepted that snowmelt is the most important crack-filling source. Minor sources may include varying proportions of densified snow or hoar-frost accretion (St-Jean et al., 2011; Boereboom et al., 2013). The effects of isotopic fractionation processes in the snow cover and during snowmelt prior to frost-crack filling on the stable-isotope composition of wedge ice are yet neither fully understood nor well constrained. Both issues are considered to be negligible for the purpose of this study but should be subject of more detailed studies in the future.

Following your recommendation, we added some information to the first two paragraphs of the Introduction (section 1).

Section 3.1 and discussion: There was limited information about relative locations of horizontal sampling profiles within the sampled ice-wedges and surrounding sediments. I understand it is extremely difficult to know the 3-dimentional distribution of ice-wedge network. Although sampling widths were shown in Tables, additional information about relative location of sample line edges and ice-sediment boundary in the cross sections would be helpful to infer representativeness of the samples for individual ice-wedge. In other words, how much your horizontal sampling profiles cover the actual ice-wedge widths?

→ We sampled horizontal ice-wedge profiles that cover the entire widths of the studied ice wedges. However, the sampling resolution was different between ice wedges (but constant within individual ice wedges) and varied from 1 cm for Holocene ice wedges (quasi-continuous sampling) to about 30 cm (single point sampling) for ice wedges of the Yedoma Ice Complex, depending on field conditions, sampling tools and research questions. The width of individual samples was between about 1 cm and 3 cm, depending on sampling tool (chain saw, axe and ice screw). When sampling with ice screw and axe, we usually took the first sample about 10 cm apart from the ice-sediment boundary to minimize the effects of post-depositional fractionation processes between sediment and ice. When sampling with the chain saw, we took the first sample directly at the sediment-ice boundary (but in several cases had to remove these samples from interpretation because the showed clear indications of secondary fractionation processes between the wedge ice and the surrounding ground). Furthermore, sampling took place perpendicular to ice veins. We are, therefore, quite confident, that our stable-isotope datasets are representative for individual ice wedges, in particular for Holocene ice wedges.

We added some of this information to section 3.1 (Fieldwork).

How did you distinguish individual ice-wedge from the vast massive of Yedoma Ice complex (especially Unit IV)? It looks continuous thick ice network within the 30m thick deposit in the photos (Fig. 3).

→ The ice wedges of the Yedoma Ice Complex (Unit IV) were sampled between two respective sedimentary polygon fillings, carefully selected either at the bottom of the headwall of the thaw slump or from above (ground surface). All sampled Yedoma ice wedges were at least several tens of meters apart from each other and in most cases at different altitude levels (see Table 5).

P. 2 / L. 25: You started here to use intra-sedimental texture ice, and used “texture ice” afterword. I recommend to state your usage of “texture ice” for ice for pore ice as well as segregated ice lenses and layers in this sentence.

→ Based on the very detailed referee comments of reviewer 1 we changed the terminology and use now “pore and segregated ice” instead of ”texture ice”.

P. 3 / L. 13: Why you mention about CO2 in air-bubble and DOC here if carbon-dead ages are topic of the sentence?

→ This information was given just to show that there is some methodological progress in the application of wedge ice in paleoclimatic research. Even though we use only POC in this study we

prefer to have this information included. We slightly changed its position and moved it into an earlier sentence in the Introduction (section 1).

P. 4 / L. 21: The Kuchchugui Suite is MIS5 in Table 1 in contrast to MIS6; Tumskoy (2012). Is this correct?

→ Yes, this is correct. Tumskoy (2012) interpreted the Kuchchugui Suite as MIS6. Table 1 includes new IRSL dates from our study pointing to Kuchchugui formation during MIS5.

P. 5 / L. 7: ". . . Values higher than 100wt% indicate ice over-saturation. . ."; is this true only for your samples in this study? Did you extract texture-ice water only from those samples with supernatant water, and the results are only for them?

→ Yes, this is valid for all permafrost samples. Yes, we took samples of water for pore and segregated-ice stable isotopes only from thawed sediment samples with supernatant water. Hence, not all sediment samples resulted in a pore and segregated -ice stable-isotope sample.

We changed the wording accordingly.

P. 5 / L. 17: This means the sampling lines were not horizontally or parallel to the ground surface line? The frost cracking veins were not always near vertical?

→ No, the sample lines were horizontally (=parallel to the ground surface). Furthermore, ice wedges were selected where the exposure and consequently the possible sampling profiles perpendicularly cut frost-cracking direction and hence, ice veins. In general, the ice veins were near-vertically oriented

We changed the sentence accordingly.

P. 13 / L. 17: Refer Table 3 after the radiocarbon ages.

→ Changed accordingly.

P. 13 / L. 10: Please briefly explain why there is a possible attribution of unit I in the eastern part of the study region to the Zyryanian.

→ We believe you are referring to P.13 / L. 20. The main reasons are (1) that we do not have a good age control for ice wedges of Unit 1 below the Buchchagy Ice Complex deposits in the eastern section and (2) the sediments are similar to the (dated) Kuchchugui sediments of Section B and to the Zyrianian stadial floodplain deposits on Bol'shoy Lyakhovsky Island (roughly about 60 to 80 kyr b2k).

The sediment radiocarbon ages are infinite and/or close to the limit of the method whereas the ice wedge $^{36}CL/Cl^{-}$ ages reveal large uncertainties that might also allow an attribution to the Zyrianian stadial instead of an attribution to Unit 1 (Kuchchugui). We could not compare ice-wedge isotopes of the section A ice wedges with ice wedges of Unit 1 (Kuchchugui) in Section B because we did not observe the latter. So, the attribution in the eastern Section A is based on the litho- and chronostratigraphy of Section B.

P. 14 / L. 15: What do you mean a temporal coexistence of Ice Complex accumulation plains, thermokarst, and floodplains in the same region during MIS5? This means all three processes existed

at different time frames during MIS5 or they coexisted at the same moment during MIS5, or something else?

→ All three landforms and related processes may have coexisted at the same time in the same region (i.e. several km^2), but not in the very same study location. Similar situations may be observed today. Based on the available age determinations, this cannot be resolved. Further information of our understanding of MIS5 periglacial landscapes is given in Wetterich et al. (2016), referred in the paper. The crucial point, however, are large uncertainties in the available and applicable dating (Th/U of peats).

We slightly changed the wording.

P. 14 / L. 23: Table 4 ! Table 3?

→ Table 1 is correct as no ages from Bol'shoy Lyakhovsky Island are reported in detail in this manuscript. See Table 1 for according references.

P. 15 / L. 16: Except for Unit IV?

→ We meant mean stable-isotope compositions for which our statement is valid.

We changed the sentence accordingly and add a reference to Table 5.

P. 15 / L. 28: Refer to Fig. 4 and Table 5.

→ Changed accordingly.

P. 15 / Section 5.2: It would be kind to briefly explain about secondary fractionation processes. Somehow, it should be connected to the sentence beginning from L. 32.

→ The information on secondary fractionation processes such as evaporation and freeze-thaw cycles is given in the next lines of this paragraph.

We added some information on the effects of evaporation and freezing.

P. 18 /L. 20-23: Is it possible to display and compare the results from other areas listed here to OY and BL data in Fig. 7? Even though they cover down to MIS3 or 4, it will help to discuss spatial representability of ground-ice stable-isotope records.

→ This is indeed an interesting idea. However, we decided not to include the additional ice-wedge isotope data because the chronologies and isotope values for MIS4 to MIS1 are quite similar to our data and would be confusing in the figure (in particular with the new colour coding according to your suggestion below).

P. 18 / L. 30: Please add reference for the pollen study (Andreev et al., 2004?).

→ We added the missing reference (Andreev et al., 2004) and slightly changed the wording.

P. 19 / L. 1: I recommend to refer to Unit I after Kuchchugui ice wedges.

→ Changed accordingly.

P. 19 / L 33-34: I couldn't understand this sentence well.

→ We rephrased the sentence.

P. 20 / L. 6-9: This is interesting and provides strong support for your discussion about IW as winter climate proxy.

→ Thank you. More on this in Meyer et al. (2015) and Opel et al. (in press).

Table 1: Check the reference for unit III, "hi"

→ Should be "h". Changed accordingly.

Table 3: To which sequence the samples from Field campaign 2002 is attributed?

→ The samples from 2002 can be attributed to the sequences A and B. We added this information to Table 3.

Figure 2: the green colors for Unit VII and VIII are hardly distinguishable. Could you make them more contrasted between two?

→ Changed accordingly.

Figure 3: CSIW is different from CW?

→ Should be CW. Changed accordingly.

Figure 4: Please make grey crosses larger because some points overlapping with black dots are not clearly distinguishable for me.

→ Changed accordingly.

Figure 7: Please consider to use colors for markers here as used in Fig. 6. It helps readers to connect the markers to discussed Units.

→ Thank you for this great idea. We changed the figure accordingly and considered the color coding of Figure 6.