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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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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

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Discussion paper

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.

(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. 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? 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). P. 2 / L. 25: You started here to use intra-sedimental tex-

ture 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. P. 3 / L. 13: Why you mention about CO2 in air-bubble and DOC here if carbon-dead ages are topic of the sentence? P. 4 / L. 21: The Kuchchugui Suite is MIS5 in Table 1 in contrast to MIS6; Tumskoy (2012). Is this correct? 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? 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? P. 13 / L. 17: Refer Table 3 after the radiocarbon ages. 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. 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? P. 14 / L. 23: Table 4 → Table 3? P. 15 / L. 16: Except for Unit IV? P. 15 / L. 28: Refer to Fig. 4 and Table 5. 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. 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. P. 18 / L. 30: Please add reference for the pollen study (Andreev et al., 2004?). P. 19 / L. 1: I recommend to refer to Unit I after Kuchchugui ice wedges. P. 19 / L 33-34: I couldn’t understand this sentence well. P. 20 / L. 6-9: This is interesting and provides strong support for your discussion about IW as winter climate proxy. Table 1: Check the reference for unit III, “hi” Table 3: To which sequence the samples from Field campaign 2002 is attributed? Figure 2: the green colors for Unit VII and VIII are hardly distinguishable. Could you make them more contrasted between two? Figure 3: CSIW is different from CW? Figure 4: Please make grey crosses larger because some points

overlapping with black dots are not clearly distinguishable for me. 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.

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