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The Cryosphere Discussions

Interactive comment on "Stable isotope and gas properties of two ice wedges from Cape Mamontov Klyk, Laptev Sea, Northern Siberia" *by* T. Boereboom et al.

Anonymous Referee #1

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General comments The paper by T. Boereboom et al. presents interesting results (ice crystallography, stable O-H isotopes and occluded gases measurements) from two ice wedges and one ice-sand wedge exposed in coastal bluff along the Laptev Sea. The lowlands along the Laptev (east of Lena River) and East Siberian seas (west of Lena River) contain numerous exposures of ice-rich permafrost. To date, a large number of multidisciplinary paleoenvironmental studies based on stable isotope analysis of ice wedges has been performed from this region. Considering that the purpose of the Boereboom paper is to assess the source of snow feeding the ice wedges and paleoenvironmental conditions during the formation of ice wedges (I think as it is not clearly stated in the paper), it is unclear how this study contributes to further our understanding

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of the use ice wedges for paleoclimate / paleoatmospheric reconstructions in northern Siberia. Especially when a regional summary of paleoclimate / paleoatmospheric reconstructions based on ice wedges that formed in the “Ice Complex” was recently published (Wetterich et al., 2011; this study is not cited in the text). A general discussion of the results and interpretations of the Boereboom et al. paper to other similar studies performed on ice wedges in northern Siberia and else where in the Arctic would be beneficial (rather than a comparison with glacial firnified/basal ice). For example, the authors compare the ISW-28 “ribbon-like structure” to basal ice formation and ignore previous studies that described the formation of ice-sand wedges (i.e., Romanovsky 1976; Meyer et al., 2002). They also weakly link previous studies of isotope variations in ice wedges in the Ice Complex and underlying units in terms of broader regional paleo-climatic and paleo-atmospheric circulation in northern Siberia.

The structure of the discussion section needs improvement to achieve the objectives of the study. At the moment, the discussion is divided in three sections that relates to the methods used in the study. The reader has to read the paper in its entirety to understand the direction the authors are trying to reach. For example, they indicate that the ice wedges formed under warm/cold conditions in sub-sections ice crystallography and gases measurements, but it is not until the third sub-sections that they discuss the timing of formation of their ice wedges (Holocene/Pleistocene) and their potential formation by freezing of snowmelt water. In order to achieve the objectives of their study and facilitate the flow of the arguments in the text, the authors should re-organize the discussion: 1) mechanism of formation of each ice wedges (IW-26; IW-28; ISW-28) and clearly indicate if the ice wedges and ice-sand wedge formed by snow compaction, vapor-diffusion, freezing of spring snow meltwater; mixture of snow and snow meltwater; 2) sources of snow feeding the ice wedges; 3) occluded gas measurements and its significance; 3) paleo-climatic / paleo-atmospheric reconstruction and regional assessment with other studies in northern Siberia.

Chronology issues. Are the units A, B, C (ice complex) and D found along the vari-

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ous coastal bluffs in northern Siberia correlated between each other? In the Boereboom study, one ice wedge was sampled from the Late Pleistocene age "Ice Complex" (dated to between 55-28 ka years in Meyer et al. 2002 and between 27.2-14.5 ka BP in Schirmeister et al. 2008) and the second one from the underlying unit A. However, the age of Unit A is not provided in the text. Meyer et al. (2002) reports an age of 200 +/- 3 ka BP for this unit, whereas Schirmeister assigns an age of ca 60 ka BP to Unit A. Knowing the age of Unit C (Ice Complex) and Unit A in this study would greatly improve the scope of the manuscript as it would provide a relative chronological marker when discussing Pleistocene climate variations for the ice wedge (IW-28) and ice-sand wedge (ISW-28) found in Unit A. If the authors can't assign at least a "relative" age to the ice wedges, than the discussion on paleo-climate and paleo-atmospheric reconstruction becomes largely irrelevant because we can't constrain the timing of these changes. This is one of the conclusions of the paper "our ice wedges can be interpreted in terms of contrasting paleoclimatic and paleoenvironmental conditions between the Pleistocene and the Holocene" but we already know that environmental conditions during the Pleistocene were very different than during the Holocene.

Stable O-H isotope and origin of ice wedges. The authors are using stable O-H isotope measurements to identify the source of water and origin of ice wedges. This is of extreme value in an effort to derive a paleo-climatic and paleo-atmospheric record from ice wedges as only those that formed from compaction of snow and that show no isotope fractionation during freezing of liquid water inside the ice wedge can be used. But the origin of the two ice wedges and the ice-sand wedge is not clearly stated in the text. In the ice texture and fabric section, the authors suggest that IW-26 formed from an increase in meltwater, ISW-28 from freezing of liquid water and IW-28 show evidence of freezing by liquid water in the early stage of ice wedge growth. In the gas composition section, they indicate that all three ice wedges formed by meltwater freezing in snow. In the stable O-H isotope section, the authors indicate that IW-26, for which the samples are distributed along a regression slope of 6.6, it formed by freezing of liquid water under warm climate conditions of the Holocene. However, for IW-28 and

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ISW-28, they do not discuss their mechanism of formation and immediately interpret the results in term of climate and atmospheric changes ("water cycle processes") and "post-depositional processes". The authors should first discuss "initial" depositional process from which IW-28 and ISW-28 formed. They indicate that during formation of ice wedges by freezing of liquid water (snow meltwater) that there is no isotope fractionation (based on the study of Michel, 1982). But, why did IW-26 plot along a freezing slope and not IW-28 and ISW-28? Conversely, the study of Lauriol et al. (1995) clearly indicate that there is isotope fractionation during formation of ice wedges formed by freezing of liquid water and that the 18O-D composition snow meltwater from which they form is higher than that of the initial snow. This isotope fractionation during freezing of late stage snowmelt water will also cause a reduction in d values. In this case, the 18O and d values in the ice wedges can difficultly be used as climatic and atmospheric proxies. The authors should explain why IW-26 plots along a freezing slope and not IW-28 and ISW-28 (could it be related to: 1) sampling interval in the ice wedges? i.e., if the complete individual ice vein is sampled, than the effect of isotope fractionation during freezing would not be observe as mass balance would be preserved (this was observed in the case of pingo by sampling of yearly growth layers); or 2) freezing conditions during formation of ice wedges?). Only when the authors prove that no isotope fractionation occurred could they discuss past climate records. They might be right, but at the moment, the authors are using a circular argument.

How do the 18O-D compositions of the three ice wedges in this study compare to ice wedges in the Ice Complex (Unit C) and Unit A in other studies? Considering that there is a vast amount of similar studies in northern Siberia, it would be nice to see if paleo-climate and paleo-atmospheric reconstructions from this site is consistent with other regional studies (rather than a comparison with Greenland ice cores).

Gas measurements. The authors attribute the CO2-CH4 gas concentration in the ice wedges to microbial activities (methanogenesis under anaerobic conditions). Although it is a possibility, they make a weak case by comparing their occluded gases results to

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glacial ice rather than to the extensive literature on potential microbial processes in ice wedges. The authors could improve their case by discussing microbial diversity studies from Late Pleistocene age ice wedges (which mainly identified aerobic microbes – i.e., Katayama et al. 2007; Steven et al., 2007; 2008), or the biodiversity of northern Siberian permafrost and cryopegs beneath Unit C (the suite of Gilichinsky et al. papers). They also do not make reference to the study of Lacelle et al. (2011 in EPSL), the only study thus far that combined both microbial diversity and gas measurements to infer microbial activities in ice wedges (and other types of ground ice).

Specific comments P.2 Line 19: Can the authors provide details about which aspects of permafrost are changing because not all aspects are changing and the rate of changes is not equal across all permafrost regions. P.2 Line 24: "depth of permafrost table has increased over the last decade". This statement is not true for all continuous permafrost regions. Based on CALM website, active layer thicknesses has not been increasing in Arctic Canada and a reduction in active layer has been observed in Scandinavia over the last 5 years. Authors should specify which region they are referring too. P.3 Line 1: Please specify how an increase in liquid water will help to produce more methane in these regions. Rate of methane production is limited by anaerobic conditions. P. 4 Line 24: "IW-28 has been attributed to unit A" What do the authors mean here? Either IW-28 is part of unit A or it is not. P.8 section 4.4. Is it possible for the authors to quantify the amount of organic matter and carbonate in their samples rather than a visual description of bubbly activity (i.e., LOI method).

Technical corrections P. 3 Line 3-11: The 9 lines long sentence is incomplete. P.3 Line 12-14: "In this paper, we propose a multi-parameter approach using stable isotope composition, total gas content, gas composition, texture and fabric of two ice wedges from the Laptev Sea coast." This sentence is also incomplete. If you propose a multi-parameter approach, you must specify for which purpose. Maybe re-phrase to say that you USE a multi-parameter approach, consisting of stable isotope..., for the study of 2 ice wedges? P.3 Line 25: "outcrop" is the wrong word here. Replace with "coastal bluff"

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P.5 Line 9: Insert “are” between analyses derived P.5 Line 25: Replace “given” with “presented” P.6 Line 20, Line 24; P.7 Line 2, Line 6: Replace “co-isotope” with “D-18O” P.6 Line 24-25: The sentence “This distinction . . . two facies” repeats what is mentioned in the previous sentence. Remove this sentence. P.8 Line 14-15: Replace “values” with “concentration” P.11 Line 4: Remove “living” P.12 Line 4: Replace “are currently not available” with “were not performed” Unless, if these analyses are available now, they could be inserted in the text.

Interactive comment on The Cryosphere Discuss., 5, 3627, 2011.