Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2019-470-RC1, 2020

Interactive comment on "Organic Carbon Characteristics in Ice-rich Permafrost in Alas and Yedoma Deposits, Central Yakutia, Siberia" *by* Torben Windirsch et al.

Anonymous Referee #1

Received and published: 27 January 2020

The paper "Organic Carbon Characteristics in Ice-rich Permafrost in Alas and Yedoma Deposits, Central Yakutia, Siberia" by Windirsch et al. reports detailed analyses on carbon and ice contents, stable water isotopes, soil grain size distribution, and age estimates for two long ground profiles in the Central Yakutia, Russia. The presented materials are rare and highly valuable to understand landscape development and contents in the permafrost of the Eastern Siberia. Although I expect this paper to be finally published in Biogeosciece, the authors have not fully utilized, described, and discussed the data presented. The importance of this paper is the rareness of the sample core. I encourage the authors to enrich their descriptions about each core unit as a valuable drilling log of a permafrost region. Some portions in Discussion are not logically constructed. For the main datasets of stable water isotopes and carbon parameters, a more in-depth and quantitative discussion is anticipated. Although the authors want to focus on the organic carbon characteristics as indicated in the title, more comprehensive discussion and interpretation must be done using available water geochemistry, cryostratigraphy, grain size distribution, magnetic susceptibility, etc. The usage of references or previous studies is poor and is often not clear or inappropriate. The discussion of sedimentation rates and their changes is problematic and too speculative. Radiocarbon age of bulk soil organic matter (SOM) usually much less reliable than that of macro plant remains. In permafrost environments, old (easily more than ten thousand years) carbon can co-exists in the same sedimentation stage, or younger carbon can be incorporated in a deeper layer. High mobility of SOM and geomorphological processes such as thermokarst and cryoturbation give a large uncertainty to sedimentation processes. Without rigorous consideration about the validity of the age model based on bulk SOM, the discussion about the changes in sedimentation rates does not make sense. As the authors explained, the upper part of the Alas1 accumulated by Yedoma deposit thaw. The Yedoma thaw involves differential ground subsidence and mixing of ground material through the thermokarst processes, which induce destruction of original stratification. The authors should discuss the limitations of the obtained dating data and build further discussion only based on reliable information. Below, I listed individual points to be revised or to be clearly explained in the revised manuscript to achieve a publication quality.

P1-L25-26: What is “a potential theory of Holocene influence”? P1-L30: What do you mean “. . . the Yedoma core can be duplicated.”? P1-L31-36: The authors describe using numerous “different” and “differ” works, but I suggest to revise to explain them more concretely. I could not understand how different. L36: How the Alas core gives clear insights? P2-L16: Why Yedoma deposits highly vulnerable to thaw? Usually, high-ice content permafrost is more robust to thaw because of larger latent heat storage than drier permafrost. We can say Yedoma is highly susceptible to thermokarst though. P2-L24: “That these processes. . .” ??? P3-L25: active layer thickness should be larger/smaller rather than higher/lower. P4-L24: What do you mean by "...if 'previously' unfrozen,..."? What is "artificial roots (Rhizones)"? Please describe more details about the water extraction and explain how you avoid evaporation from the sample water. P4-L26: Please add information about subsample intervals. P4-L28: Do you have a reason to use "absolute" ice content? I think gravimetric ice content makes clearer what you are dealing with. P4-L29: I suggest to distinguish liquid and solid water more clearly. There needs more explanation to "water which froze after drilling." P5-3.2.5: Please describe the accuracy of the stable isotope analyses. P6-3.2.6: Please explain briefly about the bootstrapping approach. P7-L21: What do you mean by "...dated with an infinite radiocarbon sample."? P7-L26: Please describe more about ice characteristics in this unit. Add some photos in the manuscript. P8-L26: If the active layer thickness reaches to 200 cm, the seasonal thaw depth touches the top of the talik (160cm). Does this mean the thickness of the seasonally freezing layer is 160cm? Section 4.3: Why some YED1 points plot above the GMWL on the LEL? Could you add some explanation in Discussion? Which portion of the core are they? P10-L27-29: Similarities between which units of them? P11-L2-5: Please rewrite this sentence. It is hard to understand probably because of the usage of too many conjunctions. P11-L7-8: I see varying values of d13C with depth in the profiles. What does your "constant" mean? You are comparing one data point in the active layer and data from other depth? I think it is not valid to compare decomposition rates between organic carbon currently decomposing in the active layer and ones formerly exposed to decomposition for an unknown period then frozen. As far as I see the profile, both C/N ratio and d13C fluctuate with depth even in the same unit. Is this reflected by varying decomposition rates? P11-L16-20: "The similarity of the low C/N ratios from both cores..." Which both cores? Your YED1 and Alas1? Or are you comparing with other Yedoma sediments found near the Arctic sea? I could not understand why this similarity supports the sedimentation of organic-poor materials. Physical conditions (such as climate and hydrology) between the comparing regions are quite different.

P11-L22-: Please show both profiles in one graph for better comparison. The heights of 0 m of the graphs of d18O profiles differ in Fig. 7 b/c, but not 9 m illustrated in Fig. 2. You can reflect the relative height difference in the same graph if you want. Similarly, please add a graph showing d-excess profiles of both cores in Fig. 7 (not in supplemental figures) and discuss evaporation and freeze-out fractionation processes that might have affected to your profiles. I think Fig. S7 should be shown as the main figure, probably, combining with Fig. 7 b/c and d-excess profiles if you want to discuss dD profiles. P11-L21-26: This entire paragraph is not logically described and I do not find sounding discussion. Why the age inversion and stable water isotope signals are related? Why particularly 35 K BP is the timing of the shift in sediment input? “This would have been . . .” What “This” indicates here? P11-L27: “permanently” means always and forever. You should assign a more specified period of time when you discuss topics like in your manuscript. It is impossible to be in a permanently frozen state throughout the depositional history. P11-L28: Please clarify depths for “the lower Yedoma pore ice” and “the uppermost sample.” “the uppermost sample” is the data at 0m in YED1? P11-L31: “The very similar..” to what? P12-L1: The original data were from Popp et al. (2006)? P12-L2: “offset” of what? Your ice-wedge values against what? P12-L4: As I mentioned above, the age constraints around 40-50 K BP need reconsideration. Furthermore, isotope signals of pore water do not simply show climate change. Lots of complicated fractionations and changes in seasonal meteoric water components could affect the signals. P12-L5-: This topic sentence doesn’t make sense to me from your discussion above. What do you mean “lake converge”? Your discussion mixes decomposition rates during Yedoma deposition and during alas formation. P12-L25-: Cryostructure of frozen sediments mainly depends on soil grain size distribution, freezing rate, and moisture content. Sandy sediments usually have fewer pore spaces comparing to sediments consist of finer soil particles. Sandy sediments tend to have structureless cryostructure because sandy particles have a limited amount of unfrozen water under cryotic conditions. Section 5.2 the first paragraph: I think the authors are misunderstanding or misinterpreting the papers Wetterich et al. (2009), French and Shur (2010), and Iwahana et al. (2014) as to the mechanism of cryostructure formation and sediment/carbon movements in the ground. Section 5.2 the second paragraph: Discussion in this paragraph makes sense to me. This discussion should be placed in Section 5.1 as it is tightly related to carbon accumulation and loss at this site. P13-L23: Why "organic-bearing"? I thought you are discussing the transportation of organic-poor sediments. Could you discuss this sandy deposit could be Aeolian or not using your grain size data? P13-L25: Where the "7000 yr" came from? Aeolian materials cannot be organic-poor? P13-L29: What do you want to refer to Diekmann et al. (2017)? The climate changes? P13-L4: What the "this" indicate? The deposition of silty organic-bearing material on top of the sandy layers, decreased water availability, or a climatic backshift to colder conditions? In any case, I could not understand the logic. P13-L5: Why you refer to Grosse et al. (2013) for "The underlying ground began to thaw"?? P13-L7: "as visible in the . . ." should be explained concretely. P13-L12: "shrunk . . . from . . . 2200 to 1200cm.." Please discuss the possibility of subsidence using excess ice volume information. Is there enough excess ice content in the initial Yedoma to reduce its volume from 2200 to 1200cm thick? P13-L12-13: Again, what "this" indicates here? P13-L15-16: I don't understand why you started to discuss the tipping point suddenly. Is this relevant and supported by your results? Section 5.3 the first paragraph: How the 4.40 kg/m3 was derived? Please explain the relation to 4.48 and 6.93 for the two sites? This could be the first carbon stock measurement below 3m for two sites in the Central Yakutian Yedoma in English literature, but have you checked Russian literature? Please discuss how representative your two cores for the Central Yakutian Yedoma. P16-L1-2: I could not understand what you wanted to indicate from this sentence. What do you mean by "drainage"? How did the alas core reveal its composition and stratigraphy before lake formation? Fig. 2: Yedoma lake should be thermokarst lake? Or by local name, "dyuyodya" (Soloviev, 1973) or "dyede" (Crate et al., 2017)? Soloviev, P. A. (1973), Thermokarst phenomena and landforms due to frost heaving in Central Yakutia, Biuletyn Peryglacjalny 23, 135-155. Crate, S., et al. (2017), Permafrost livelihoods: A transdisciplinary review and analysis of thermokarst-based systems of indigenous land use, Anthropocene, 18, 89-104, doi: https://doi.org/10.1016/j.ancene.2017.06.001. Fig. 3: Include photos of each unit including ice-wedge layer. Put “YED1” in the figure. Fig. 5: Please use the same white balance and adjust the exposure/brightness of the core photos as in Fig. 3. Include core photos of the rest two units too. Put “Alas1” in the figure. Fig. 4 & 6: Please used different markers for absolute ice content and bulk density so that people without color printer environment or color-blind can easily distinguish the two. Large caption YED1 and Alas1 within the figures would be helpful. Fig. 7 & S7: Combine Fig 7 b/c and S7 and add a graph of d-excess. Please use open triangle markers for ice-wedge points. The half-open triangle hinders instant discrimination between markers. Add labels “YED1” or “Alas1” on top of each graph. Fig. S2: The blue markers with age uncertainties are not discernible. Please revise them for a clearer presentation. I do not think the information on the model confidence by black shades is meaningful and necessary in your radiocarbon age of soil organic matter. For all graphs with depth axis: Please use the meter scale, not cm.

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