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Interactive comment on "Dissolved organic carbon (DOC) in Arctic ground ice" *by* M. Fritz et al.

M. Fritz et al.

michael.fritz@awi.de

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Reply to Anonymous Referee No.2

We are grateful for the review and acknowledge your comments and suggestions. You will find all replies or changes that have been made below. Reviewer comments are cited in italic font.

Best regards,
Michael Fritz

(on behalf of the co-authors)

Main comments: 1. *The first of my main comment regards a combination of terminology and the overlooking of another pool of carbon in ground ice: particulate OC. As the authors describe, the Pleistocene ice wedges are yellowish-brown to grey in colour, clearly visible on the photos in Figure 2. My guess is that the ice also contains particulate matter and carbon, which is currently not assessed and neither mentioned, as the authors have not provided any information on the organic carbon that remained on the filters. I would find it very valuable if this information is included, and if not, at least that this pool is described as a component currently not addressed. Linked to this, the authors use POC to describe the OC pool in permafrost soils. In de "aquatic community", POC is often used to describe the particulate OC fraction in water. The current use of POC in the manuscript is confusing and in most permafrost literature not used like this. I suggest to use soil OC, or just OC, or soil OC (SOC) or something like this.*

We used 0.7 μm GFF filters attached to a syringe. These filters are sealed and cannot be analyzed afterwards. We acknowledge that the particulate fraction is important but it is not the objective of the paper.
The differentiation between dissolved organic carbon (DOC) and particulate organic carbon (POC) is simply a matter of particle size. DOC is defined as the organic matter that is able to pass through a filter of 0.7-0.22 μm pore size. Conversely, particulate organic carbon (POC) is that carbon that is too large and is filtered out of a sample. Unfortunately, there is no global agreement on DOC and POC size differentiation which would guarantee direct comparisons of data from different studies. Mostly, the two pore sizes are used (0.7 and 0.45 μm). Once more, permafrost seems to a special and understudied case. The terms organic carbon (OC), total organic carbon (TOC) or soil organic carbon (SOC) always pool the dissolved and undissolved carbon fractions. However, studies inside and outside permafrost research have shown that DOC plays a special role in the carbon cycle.

2. *My second point concerns the availability of data. Can the data for DOC, water*

isotopes, ICP-OES etc. be presented in a supplementary table? Currently no individual sample info is available, and, for example, d18O and dD values are not included either.

Changed accordingly. We added the table and referenced the supplementary table in the text and figure captions. See **Supplement Table S1**.

Further comments:

p.78, L4: *It's not clear what "Their" refers to.*

Replaced by "permafrost"

p. 78, L19: *Can something be "rapidly stored"? I suggest to rephrase into "rapidly frozen and stored".*

Changed accordingly.

p. 78, L22: *"4172 km3" is a number with too many significant numbers given the estimates this has been calculated from. I suggest to replace with "4170" or even better "4200" (please also replace this at a few other occasions in the text).*

Changed accordingly.

p. 78, L22: *See first, main comment: replace particulate OC with something else.*

See comment above.

p. 79, L4: *"degradation forms as thermokarst", you (also) mean alas deposits?*

Yes, but we do not want a large annotation of landforms and sediment types. The idea is to mention that not only Yedoma is ice-rich but also degraded Yedoma deposits.

p. 79, L10: *100 % volume? Isn't ice just ice? This is a bit confusing. Also, this paragraph lists many % numbers, some in weight, some in volume, can these be presented slightly more consistent?*

This relates to your first comment. The ice content of ice wedges is a little less than

100 %. From an unpublished master's thesis we know that volumetric ice content of ice wedges from Herschel Island in the western Canadian Arctic is >98 %; mostly above 99 %.
Unfortunately, we cannot present the data on ice contents more consistently. In the past many studies have measured ice contents only gravimetrically. For example, leaving out the Schirrmeister et al. (2011c) review paper with the gravimetric ice content presented here, we would leave out the best data set for east Siberia.

p. 79, L13: *I suggest to change "other sediments in Yedoma" with "other ice in Yedoma sediments", because this sentence is about the total ice content, right?*

changed into: "other ice types in Yedoma deposits. . ."

p. 79, L17-18: *I think you can remove the definition of massive ice, it just adds to the confusion. p. 80, L24: "particulate OC", see the main comment above.*

We would like to keep the definition, because not everybody is familiar with massive and non-massive ice classification. For now, we mostly deal with massive ice and have not touched non-massive intrasedimental ice.

p. 80, L2: *"DOC from permafrost is chemically labile", I think it would be more correct to write "DOC from yedoma sediment and yedoma ice wedges is chemically labile".*

We added the reference of Dou et al. (2008) who showed large quantities of low-molecular-weight DOC in surficial permafrost horizons in northern Alaska, which corresponds to labile DOC. With that we would keep the original statement because labile DOC is not restricted to Yedoma.

p. 80, L7: *"that" should be removed.*

Changed accordingly.

p. 80, L25-26-27: *Yes, this study measures DOC at the source, but only in ice wedges, not in the (total) permafrost. Maybe specify?*

Changed into “(i.e. ground ice in permafrost). . .”

p. 81, L25: *Only here you briefly define Yedoma. I think this should come a bit earlier in the manuscript.*

We added information on Yedoma origin in the introduction.

p. 81, L16: *and in many other places in the manuscript: please have a thorough look at your use of hyphens. In this sentence you correctly write "ground-ice conditions" with a hyphen between "ground" and "ice" because they together are an adjective to "conditions". At some other place (e.g. p.79, L5, 8, 14, 15, 25 and 28) you do not do this.*

Changed accordingly.

p. 82, L2: *You use both "late glacial" but also "late Pleistocene" throughout the text. If they mean the same thing, I suggest to just stick to one of these.*

The “late Pleistocene” (MIS 2-5) spans a longer time than the “late glacial” (Bölling to Younger Dryas). In some occasions we have the possibility to narrow the age of the studied ice bodies.

p. 82, L14-15: *You only included three surface water samples from thermokarst lakes. Compared to the rest of the pretty extensive dataset, I find this number a bit poor. There must be more data available in the literature.*

The purpose of showing the data on modern surface water was to get an idea about the magnitudes of DOC concentrations we are dealing with. Now, we can see that they are comparable. Further reading is recommended to Walter Anthony et al. (2014) where they show more data on DOC concentrations in bottom water from thermokarst lakes in Yedoma and non-Yedoma landscapes in eastern Siberia near Cherskii. Unfortunately, the hydrochemical data sets were not comparable so that external data could not be included into the statistical analyses.

p. 82, L18: *I suggest to move "(purified water)" to directly behind "pre-cleaned".*

Changed accordingly.

p. 83, L10: *What other standards besides VSMOW did you use for your d18O and dD analysis? A standard that is sufficiently depleted such as SLAP should be included to be able to calibrate the ice wedge stable isotope composition that could reach values of e.g. -260 for dD.*

For all stable isotope measurements, a three-point calibration is used with an internal standard set selected from the list below related to the expected values of the samples. All standards have been calibrated against V-SMOW and SLAP. Since the isotope laboratory in Potsdam works since more than 15 years with depleted ground ice samples (also Antarctic ice) it is especially well-equipped for analyses with depleted isotope signature. Standards are generally selected from the pool of standards below covering the complete range of natural water isotope composition. This pool is routinely renewed and several new standards were calibrated to replace the older ones.

Standard	Target value sample vs. SMOW [‰]	Tolerance [±]
NGT	-34.40	0.1
NWH	-48.25	0.1
HDW2	-12.70	0.1
dc1	-54.05	0.1
KARA	-0.10	0.1
Sez	-27.00	0.1

p. 85, L13-14-15: *This sentence is superfluous I think because it overlaps mostly with L3-4. Maybe expand L3-4 instead?*

Also according to the comment of reviewer 1 we removed one of the two sentences and moved the last sentence to end of the previous paragraph.

p. 86, L22: *Maybe write "climate conditions of formation"?*

Changed accordingly.

p. 86, L22-25: *This is interesting I think. Can you elaborate a bit on the reasons why you see this pattern?*

More interpretation and discussion of these points can be found in section 5.2 (page 90). In this chapter we added the following sentences that discuss the reviewer point towards stable water isotope characteristic: “A similar differentiation of ice wedges (and all ground ice types) is done along the second PCA axis (Fig. 5). Differences in stable water isotopes indicate the predominant climate variations between the late Pleistocene and the Holocene which are also reflected in the landscape (i.e. distance to sea; maritime vs. continental).”

p. 87, L4-5: *Is there a simple way to explain what these percentages mean?*

The percentage value here is based on the cation spectrum only. This means that all measured cations sum up to 100 %. This is statistically more robust than using individual sample concentrations which can differ in magnitudes. We added this information to the text.

p. 87, L16: *I think you should here instead of the McGuire paper refer to Holmes et al. 2012, which gives an updated estimate of pan-arctic DOC of 34 Tg/year (Holmes, R. M. et al. Seasonal and Annual Fluxes of Nutrients and Organic Matter from Large Rivers to the Arctic Ocean and Surrounding Seas. Estuaries and Coasts 35, 369-382, doi:10.1007/s12237-011-9386-6 (2012))*

We added the number of Holmes et al. (2012) and the reference.

p. 88, L13: *You’ve used WIV before, so I suggest to replace "wedge ice volumes" here with "WIV" to avoid confusion with volume of ice in square kilometers.*

Changed accordingly.

p. 88, L27-28: *I suppose it makes sense that non-massive ice in soils is more DOCrich, but it would not harm giving a brief explanation here why you think this is the case.*

We added: “Higher DOC concentrations in intrasedimental ice than in massive ice are certainly due to the long-term contact of soil moisture with soil organic matter prior to freezing.”

p. 89, L5-9: *What is this calculation based upon? I suggest to clarify this or leave it out. Also, I do not understand the last sentence here.*

This is a back-of-the-envelope calculation which is based on an overall average for sedimentary TOC values from the literature and our overall DOC average on massive ground ice. We slightly changed the used values and added some references of TOC values in permafrost deposits.
We added some information towards a better understanding of the last sentence. “. . .because TOC comprises both POC and DOC.” The message is that we need to differentiate permafrost TOC into POC and DOC because they react differently in time and space (e.g. transport, deposition, residence time, degradability, bioavailability).

p.89, L12: *replace "on" with "in"*

Changed accordingly.

p. 89, L15: *the term "mineralization" is a bit confusing as this word is also used as "degradation" sometimes. Maybe use "ion content" or "conductivity" or so instead?*

Changed accordingly.

p. 89, L26: *Only here you explain how ice wedges are formed. Would it be more appropriate to explain this earlier in the text? You only mention they are formed syngenetically but don’t say anything more.*

We added the following information to the introduction:
Ice wedges are one of the most common types of ground ice in permafrost. They form

when thermal contraction cracks open in winter, which are periodically filled with snow melt water in spring that quickly (re)freezes at negative ground temperatures to form ice veins and finally vertically foliated ice wedges.

p. 90, L21-23: *Is the sentence "Marine ion .... into coastal ones" really needed? I feel it mostly holds information that has already been stated elsewhere in this paragraph.*

This is the only occasion in the manuscript where we present arguments why present-day coastal sites can host ice wedges with a completely different hydrochemical signature. A difference in age is accompanied by a difference in distance from the coast, which seems to be relevant for ion transport mechanisms.

p. 91, L19-20 and before: *I agree with the last sentence of this paragraph but I do not follow how this statement follows from the above sentences. I find it a bit confusing, and the point you are trying to make unclear. First of all, you say both ice wedge DOC and DOC in runoff of later and rivers are both biolabile (right?) but in between these two things you use "In contrast" (L17). Also, the sentence "One destination of the fresh, young and therefore most bioavailable ...". You mean the destination of vegetation debris before ending up in the ice wedge OC? And (L13): I think concentrations of DOC are also lower because all the vegetation debris and surface soils have already been actively flushed out by the spring flood when discharge and therefore, then, also DOC is high. All in all, I propose to reconsider the arguments that you use to arrive at the final sentence of this paragraph "The highlights ... of bioavailable DOC".*

The reviewer has a point here and we agree that DOC concentrations are also lower because all the vegetation debris and surface soils have already been actively flushed out by the spring flood. This is exactly what we write. To improve the structure, we changed the order of arguments so that the “in contrast”-sentence is not a contrast anymore but an addition “In addition, dissolved organic matter compounds in runoff into lakes and rivers can become rapidly degraded by microbial communities and photochemical reactions (Striegl et al., 2005; Olefeldt and Roulet, 2012; Cory et al., 2014).“

Now the structure of arguments is the following:
1. Guo et al. (2007) have shown in experiments and from natural river samples that intensive leaching of DOC from young and fresh plant litter and upper soil horizons occurs during the snowmelt period (i.e. spring).
2. Later in the season (i.e. summer/autumn), they found that DOC yields decreased in rivers draining permafrost areas. This indicates that deepening of the active layer and leaching of deeper seasonally frozen soil horizons released lower DOC concentrations due to the refractory and insoluble character of the remaining organic matter compounds. In addition, dissolved organic matter compounds in runoff into lakes and rivers can become rapidly degraded by microbial communities and photochemical reactions.
3. Our conclusion based on sentences 1 and 2 is that the fresh, young and therefore most bioavailable DOC components will become incorporated in ice wedges which are basically fed by spring meltwater with a short transport pathway and small residence time.
We end this paragraph with the sentence: “This highlights the importance of ground ice and especially ice wedges as a vital source of bioavailable DOC.”

p. 91, L22: *replace "particulate fraction" by something else.*

This sentence is based on the dichotomy between dissolved and particulate. We replaced “particulate fraction” by “POC”.

p. 92, L21: *"while" sounds strange, maybe use "with" or "during"?*

Changed accordingly.

p. 92, L22: *"On the other hand" sounds incorrect here as this sentence adds up to the argument made by the previous sentence. Suggest to replace with "Also".*

Changed accordingly.

p. 92, L24: *What are "forbs"?*

forbs = non-graminoid herbaceous vascular plants (see Willerslev et al., 2014 Nature)

p. 94, L1: *Please replace "mineralization" with something else.*

We replaced “sparsely mineralized ice wedge meltwater” by “ice wedge meltwater with a low ion content”.

p. 94, bulletpoints: *Just an idea, is it possible to arrange these conclusions parallel to the bulletpoints you list in the introduction? A parallel construction of objectives conclusions would benefit the reader.*

Changed accordingly.

p.94, L16: *round up "4172 km3" to "4170" or "4200".*

Changed accordingly. 4200 throughout.

p. 95, L1-2: *Maybe rephrase into " we propose that future studies shall strive to".*

Changed accordingly.

p. 95, bulletpoints: *I think some of these points do not follow from the analyses/ conclusions in this paper, and/or I find the points that are made a bit unclear. For example, why should DOC from coastal erosion be better quantified?*

We re-arranged some of the conclusions and clarified some points in the outlook section to better match the open questions.

p. 95, L8: *"what remains POC and what is going to become DOC", maybe just write"what fraction of soil OC will be leached as DOC".*

Changed accordingly.

Figure 3: *capitalize holocene.*

Changed accordingly.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/9/C208/2015/tcd-9-C208-2015-supplement.pdf

Interactive comment on The Cryosphere Discuss., 9, 77, 2015.