

Interactive
Comment

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

Anonymous Referee #1

Received and published: 22 January 2015

Review of Fritz et al. “Dissolved organic carbon (DOC) in Arctic ground ice.”

This is a well-constructed study, the paper is well written paper and the topic will be of interest to cryospheric scientists, biogeochemists, and climate modelers. The topic, fluxes and fate of carbon in permafrost is one that it is currently receiving a lot of interest. I have no major problems or suggestions for this paper and recommend it be accepted with minor revisions.

Comments keyed to the text:

Title: I recommend a little more info. How about: “The chemical composition and fate of organic carbon (DOC) in Arctic ground ice”

Abstract: Line 4: The “their” is confusing. I think you mean “Permafrost” but the pre-

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vious sentence ends with info on nutrients so it seems like that is what “their” refers to.

Line 9: “using biogeochemical data”

Line 17: just a curiosity: why refer to snow melt as “pristine” and not just as “snowmelt”?

Line 20: Perhaps start the “In the yedoma” with “We found that in the yedoma. . .”

Lines 22-25: This is the first time particulate OC is mentioned and it is a main focus of this last sentence. I recommend introducing some of the POC results, data, etc, to set this up. Maybe one sentence to do that?

Page 80: Line 7: “several studies have shed light”

Page 82: Line 10: “It is” is vague. The previous sentence covers a lot of topics so you have to be more specific.

Page 83: The Section title has “component” but line 25 has “components”. Be consistent.

Page 85: Line 10: do the “mean concentrations” refer to DIC? If so then state that for clarity.

12-13: the sentence that starts with “It is obvious” is unnecessary. Move the “Basal glacier ice” sentence to the end of the previous paragraph.

Page 86: Line 20: “those with more of a continental”

Page 89: Line 26: “sources that have been”

Lines 26-28:

Page 90: Line 4: “into frost cracks”?

7: Unless any of the “leachable components” are close to saturation in other precipitation/water courses (which is highly unlikely) the “initial purity” of the snow is irrelevant.

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I recommend removing this sentence.

Line 8-10: The “Snow melt feeding” sentence: See previous comment and: 1) There are a ton of papers on the age and lability of the DOC in rivers at melt and some suggest this surface flow (ie at snow melt) has bioavailable C. So it is not necessary that the bioavailable C has from lower down in the soil column.

2) As the spring snow melt waters trickle downward toward the ice wedges they interact with basal soil material (frozen or not) and this could leaches out carbon.

3) Also- since wedges take thousands of years to form and the location of their upper surface likely changes with time there are plenty of spatial and temporal ways that deeper soil pore waters can get integrated into the wedge ice.

I recommend some of these ideas be introduced or discussed. The fact that the snow has little ionic strength is not a potential reason for this. And I agree the waters at snow melt start at the surface but they do trickle downward and are not likely frozen until the following winter so there is ample time for subsequent waters, interacting with a deeper active layer, could incorporate deeper carbon.

Pages 89-90: Somewhere in here or elsewhere (?) it is worth noting the potential mineral weathering signature to which their samples provide context. For example- could the major cations and bicarbonate could be used to decipher silicate versus carbonate weathering. Since carbonate weathering occurs more rapidly with silicate weathering (particularly where “fresh” mineral surfaces are available) their study might be able to use this to identify where in the active layer some compounds are sourced? They could explore a few quick ternary diagrams of Ca, Mg, and Na+K and of SO₄, HCO₃, and Cl. I suspect there will be some unique trends and if/where the signatures are more carbonate based they could be able to decipher the location in the soil column? IE the “fresh” mineral surfaces are likely toward the base of the active layer (exposed to weathering the least amount of time on an annual basis). Caveat: the marine localities may have a swamped HCO₃ and Cl signal so it is possible that this will not work. But I

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recommend they make the plots to explore it.

This is in no way a requirement by me for acceptance, etc. Just that they have a unique data set and I am trying to see if there is more info that can be teased from it.

Page 92: Line 20: “while deposition occurred”

Page 93: Line 20: I do not like use of the word “overproportionally” I am sorry I cannot provide a better word to use but it has a lot of chemical and physical connotations and I am not sure it is clear enough. Perhaps keep it but then provide what is being “overproportionally” loaded?

Table 2 and discussion in the text: Are there any data from non-thermokarst ponds? Or perhaps find it in the literature? It would be good for a comparison in this Table and for the study because thermokarst ponds likely have an outsized amount of mineral weathering (ie ions), and carbon (ie leaching from exposed blocks of soil and degraded permafrost). As such, comparing to non-thermokarst ponds could help identify whether there are significant differences?

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

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