

Manuscript Review

Title: Patterns and persistence of hydrologic carbon and nutrient export from collapsing upland permafrost

Authors: Abbott et al.

Journal: Biogeosciences Discussions

General Comments

The authors examine carbon, nutrient, and major ion concentrations in outflows from three different types of upland thermokarst in northern Alaska. A nice contribution of this manuscript is a classification scheme used to bracket degradation state of each feature. The manuscript is well written and the findings are bolstered by many new and relevant citations. Most of my comments are relatively minor.

Specific Comments

1. Page 2064, Line 24 – Need to clarify here that you are referring to **soil** organic C pools.
2. Page 2065, Line 14 – Clarify text here that you are actually referring to increases in active layer thickness (top-down is vague).
3. Page 2065, Line 17 – **May** cause subsidence. Note that even some ice-rich soils can be thaw stable due to their texture (e.g. gravelly soils). See Jorgenson & Osterkamp 2005 classification.
4. Page 2065, Line 28 – “Fueled” – reconsider word choice. Also clarify what you mean by “ground ice types”.
5. A more general note: I think you should say upfront that your are going to be using abbreviated terminology for thaw type (slumps, gullies, slides) throughout the manuscript. These terms are general, but are actually referring to very specific features.
6. Page 2066, Line 1 – Provide citation for “transition zone” – Shur et al.?
7. Page 2066, Line 15 – Provide reference for “adsorb DOC”. Many studies seem to think sorption may be key factor with thaw (e.g. Kawahigashi et al. 2006) but stabilization is clearly dependent on soil type, mineral surface reactivity and DOM character.
8. Page 2067, Line 14 – insert “organic matter” mineralization
9. Page 2068, Lines 18, 21 – Replace “average” with “mean”, the appropriate convention
10. Page 2071, Lines 4-5 – Collecting ice scrapings seems like a good way to get contaminated samples. Taking an ice core from the exposure would have provided a much better representation of the ground ice chemistry.

11. Page 2071, Lines 8-9 – Define “reference water”
12. Page 2071, Line 13 – It would be nice to see what these “channels” look like where discharge was measured. Perhaps add a figure with representative study site pictures.
13. I like that you included a link to your dataset.
14. Page 2074, Line 11 – “Permafrost ice” Were you able to distinguish between the origin of the ground ice (e.g. buried glacial ice, yedoma deposits?)
15. Page 2074, Section 3.3. – While I like the examination of land-surface age effects, it would really be nice to have some constraints on “time since thaw” of the actual features. There is an underlying assumption that the degradation classes are linked to time, but that connection has not been explicitly made. What remote sensing tools are available to bracket thaw age class?
16. Page 2075, Line 21 – The use of “thermokarst DOC” is a little confusing. Are you only referring from recently thawed permafrost, or does this including DOC pools from the active layer that have been mobilized or affected by subsidence?
17. Page 2076, Lines 10-20 – How does this paragraph relate to the findings observed in this study. Did your sampling design adequately capture seasonal dynamics? The methods are unclear on this point: did you just take one grab sample from each site once?
18. Page 2079, Line 13 – Insert “up to” 6 degrees C in “the active layer”
19. Figures – I think the manuscript would benefit from including a figure with pictures of representative thaw types.
20. The captions and figures for Figures 6 and 7 are mixed up.