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Interactive comment on "Patterns and persistence of hydrologic carbon and nutrient export from collapsing upland permafrost" *by* B. W. Abbott et al.

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We would like to thank both reviewers and the editors for their important contributions to this manuscript. We have attached our responses to reviewer comments, the revised manuscript including all revised figures, and the supplementary material in the attached PDF file. Additionally we have pasted our responses below for convenience.

Sincerely,

Ben Abbott on behalf of all co-authors.

Issues raised by Reviewer 1 Issue 1. The first general comment is on the potential for temporal and/or seasonal variability between the samplings. As the current study is presented, this is apparently not necessarily considered. The various features and their outflows are monitored at different times over different years. How have you controlled for seasonal and between year variability in biological activity or wetness across the sites? This impact could be rather large given the inherent connection between, for example, DOC and wetness and temperature. Further, there must be variability in the antecedent conditions (e.g., heavily vs. light snow years). As it is currently presented, the reader gets the impression that the differences in time between observations from the 83 features over the span of 2009 to 2012 and/or the span June to August within a given year are largely ignored. This most certainly cannot be the case, correct? How have you accounted for these impacts or (alternatively) how have you justified to ignore the variability? Clear discussion and clarity is required for these issues since they appear rather central to me.

Response 1. This study had a spatially intensive focus with a goal of identifying patterns across landscape types and feature morphologies. We completely agree with the reviewer that accounting for seasonal and inter-annual variability is crucial to understanding the functioning of these features, and have clarified our methods in the text and added supplementary figures to bring this issue to the foreground. Because of the remoteness of these features, most sites in our study were sampled a single time. However for the five most accessible sites near the field station, we collected two or three outflow samples each season. In the current statistical analysis, we included site (individual feature) as a blocking variable (a random variable in the terminology of mixed models) meaning variation between sampling dates was incorporated in the estimates of variability for that development class (see section 2.3). Variability between sites sampled in different seasons or years is also included in the estimates of error. The fact that strong trends were still apparent between development stages despite variability between years and sampling dates is evidence of the robustness of these patterns relative to the magnitude of seasonal variation. We considered testing for seasonal trends across the dataset, but results would not be representative because samples were collected during discrete sampling campaigns creating a strong spatial-temporal dependence (e.g. all the samples from the Feniak Lake region were collected over a few days in July). However, we have added supplementary figures showing seasonal variation in solute concentrations for the five sites where repeat measures were taken (Figures S1-S8). Most importantly we cite several recent studies which have performed temporally intensive monitoring of thermokarst outflow (Kokelj et al. 2013, Malone et al. 2013), to support our conclusions and discussion.

Issue 2. The second general comment/concern are the seemingly arbitrary classifications. For example, the 0-3 system for development extents and the age groupings (P2069). How robust are the findings presented in the face of the uncertainty and subjectivity of these groupings? There needs to be a simple sensitivity analysis to justify that the grouping definitions did not have strong influence on the significance of the results. This would strengthen the study and provide rigor. A simple methodology could be to randomize the data considered in each group or explore the impact of group boundary definitions. The primary goal of any analysis should be to show that the statistical significance is not purely a function of the definition of data groupings (that is fundamental). The current study does not convince me that this is the case for these analyses.

Response 2. Ideally, we would have tested for trends through time based on absolute feature ages. However, identifying a reliable time since formation is non-negligible, as was addressed at length by others on our project (Krieger 2012, Balser and Jones 2014, Pizano et al. 2014). Many features are undetectable in satellite imagery (particularly thermo-erosion gullies, which make up the majority of total upland thermokarst area). Instead we used quantitative (e.g. percentage of headwall length in an active state of decay) and qualitative (e.g. visual assessment of outflow turbidity) criteria to determine a development stage for each feature. While the absolute age of most of the features is unknown, we do provide estimates of duration of feature activity based on an exhaustive search of the literature in Table 1, and discuss how development stage likely maps onto absolute age. This provides a framework for assessing "lifetime" thermokarst impacts. While the development stage scale is coarse, features were classified in the field prior to any chemical analyses, precluding the possibility of bias in classification based on chemical signature (the response variable of interest). Furthermore, before our initial statistical tests, we performed a sensitivity analysis by randomly excluding a third of the data points from each development stage, which did not substantively change the results or interpretation. We have added a description of this analysis to the methods section and added a figure showing how most features were objectively classifiable into one of the development stages (Fig. 3). Issue 3. The final concern/comment is the lack of consideration of the size of the various themokarst features. It is difficult, from the current presentation of the study, to assess the extent of size of the landscape features and further their size relative to the size of potential drainage areas or regions of water accumulation. This is the case for both the 83 features and the 61 adjacent sites. This makes it difficult to gauge the impact of the changes estimated in biogeochemical fluxes against the full body of literature since many other studies cover many different (relative) impacts of thermokarst features. This simply need to be handled better so the data presented can realize its full potential relative to previous work. This is particularly true given the structuring of the discussion. Are the estimates presented valid only for very small thermokarst features that cover a majority of their own drainage areas such that any relationships discovered here tend to dissipate rapidly as we move away (downstream) from the features? Full consideration is need here to help put the findings in context of their landscape extent.

Response 3. Due to the extremely coarse elevation data for most of the study area, catchment delineation was not possible, precluding a direct analysis of the proportion of catchments impacted by thermokarst. However, we have now added elemental yield estimates to provide a way of assessing landscape-level importance of upland thermokarst. The yield estimates are based on change in solute concentration above and below thermokarst disturbance, feature size, and discharge. We initially did not include these estimates due to uncertainty surrounding some of the assumptions (see revised methods and discussion), but because we agree with the reviewer that it is difficult to put our findings in context with previous work, we have now added a figure and discussion in the text. While these estimates have considerable uncertainty as is clear in the standard error in the figure and the description of the method, they provide a first-order estimate of upland thermokarst solute export (Table S1).

Issues raised by Reviewer 2 (We brought up a conflict of interest with the editors since Reviewer 2 was a previous graduate student of J.B. Jones. After considering the nature of the conflict and the content of the review, the editors decided we could proceed.)

1. Page 2064, Line 24 – Need to clarify here that you are referring to soil organic C pools. Response: We are referring to all organic carbon pools so we have left it as is.

2. Page 2065, Line 14 – Clarify text here that you are actually referring to increases in active layer thickness (topâĂŘdown is vague). Response: Changed

3. Page 2065, Line 17 – May cause subsidence. Note that even some iceâĂŘrich soils can be thaw stable due to their texture (e.g. gravelly soils). See Jorgenson & Osterkamp 2005 classification. Response: Changed

4. Page 2065, Line 28 – “Fueled” – reconsider word choice. Also clarify what you mean by “ground ice types”. Response: Defined 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. Response: Added a parenthetical explanation for each feature type.

6. Page 2066, Line 1 – Provide citation for “transition zone” – Shur et al.? Response: Added citation defining transition zone.

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 stabiliza-

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tion is clearly dependent on soil type, mineral surface reactivity and DOM character. Response: We esteem that the two references already in the text suffice.

8. Page 2067, Line 14 – insert “organic matter” mineralization Response: Changed

9. Page 2068, Lines 18, 21 – Replace “average” with “mean”, the appropriate convention Response: Changed

10. Page 2071, Lines 4âĂŘ5 – 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. Response: Many features occurred on rocky substrate (glacial till or outwash) which precluded use of motorized corers. Taking ice scrapings with a stainless steel hand corer proved to be the most reliable way to obtain a sample of adequate volume. As an aside, the difference in “coreability” between uplands and lowlands may represent a potentially important bias in the distribution of soil samples at the pan-Arctic level. Added a justification of method.

11. Page 2071, Lines 8âĂŘ9 – Define “reference water” Response: Defined

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. Response: We added a supplementary picture with example features and a schematic showing sampling locations at a sequence of thaw slumps (Figures 1 and 3).

13. I like that you included a link to your dataset. Response: Thank you.

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?) Response: We classified permafrost ice type but had insufficient sample size to test for differences between them.

15. Page 2074, Section 3.3. – While I like the examination of landâĂŘsurface 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? Response: See Response 2 to Reviewer 1. We now make this connection more explicit in text.

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? Response: The distinction between thermokarst DOC and permafrost DOC is important and is treated in the introduction (second paragraph on page 2067). We have reworded to be clearer.

17. Page 2076, Lines 10âĂŘ20 – 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? Response: See Response 1 to Reviewer 1.

18. Page 2079, Line 13 – Insert “up to” 6 degrees C in “the active layer” Response: Added “up to”, however, the degree of warming was apparent in perennial-thawed soil (a talik) not the active layer.

19. Figures – I think the manuscript would benefit from including a figure with pictures of representative thaw types. Response: We included a supplementary figure with pictures of thaw types but agree that it is central to understanding these features and have added it to the main manuscript (Fig. 1).

20. The captions and figures for Figures 6 and 7 are mixed up. Response: Corrected

References: Balser, A. and J. B. Jones. 2014. Thermo-erosional Landslide Terrain Suitability in the Brooks Range Foothills, Northern Alaska, USA. . In prep. Kokelj, S. V., D. Lacelle, T. C. Lantz, J. Tunnicliffe, L. Malone, I. D. Clark, and K. S. Chin. 2013. Thawing of massive ground ice in mega slumps drives increases in stream sediment and solute flux across a range of watershed scales. Journal of Geophysical Research:

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Earth Surface 118:681-692. Krieger, K. C. 2012. The Topographic Form and Evolution of Thermal Erosion Features: A First Analysis Using Airborne and Ground-Based LiDAR in Arctic Alaska. Idaho State University. Malone, L., D. Lacelle, S. Kokelj, and I. D. Clark. 2013. Impacts of hillslope thaw slumps on the geochemistry of permafrost catchments (Stony Creek watershed, NWT, Canada). Chemical Geology 356:38-49. Pizano, C., A. F. Baron, E. A. G. Schuur, K. G. Crummer, and M. C. Mack. 2014. Effects of thermo-erosional disturbance on surfaces soil carbon and nitrogen dynamics in upland arctic tundra. Environmental Research Letters 8.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/12/C2016/2015/bgd-12-C2016-2015-supplement.pdf

Interactive comment on Biogeosciences Discuss., 12, 2063, 2015.