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# *Interactive comment on* "Contribution of Coastal Retrogressive Thaw Slumps to the Nearshore Organic Carbon budget along the Yukon Coast" *by* Justine L. Ramage et al.

**Anonymous Referee #1**

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This paper entitled, "Contribution of coastal retrogressive thaw slumps to the nearshore organic carbon budget along the Yukon Coast," by Ramage and others uses repeat analysis of satellite and LiDAR imagery to assess the number, area, and volume of retrogressive thaw slumps. They found that the number of slumps increased from 1952-2011, but the area affected by slumps changed little. Slumps displaced a large volume of soil and dissolved organic carbon. This study produces an data set htat is very relevant to an important source of uncertainty in understanding how permafrost landscapes and the organic matter they contain are responding to climate change: thermo-erosion. This process has proven difficult to model and the geophysical and ecological consequences of thermos-erosion on landscape and regional scales remain uncertain. I have a few questions and comments about the methodology, but my main concern is that the current paper quickly gets into the details of these sites and then remains largely descriptive and stops short of positioning these findings in a broader ecological/landscape perspective. If revised with a broader focus, I think this paper would be a valuable contribution to this journal and the larger discourse on the effects of thermo-erosion features on permafrost landscape evolution during climate change. I outline my main questions and concerns below, followed by line edits:

1. This study presents valuable data that are difficult to acquire about the extent and volume of sediment affected by thermo-erosion on decadal timescales. However, I felt it did not fully exploit these data, remaining largely observational and not providing a clear discussion of how these data relate to larger questions about ecosystem carbon balance, links between geomorphology and climate, and permafrost ecology. Given the spatial and temporal richness of this data set, in addition to describing the changes in thermo-erosion area and volume, are there underlying mechanisms the authors could explore? For example, do differences in precipitation, aspect, or other parameters affect rate of thermo-erosion? How representative is this area compared to other Arctic coasts? How different were changes in air temperature for the two periods and is this associated with changes in thermo-erosion? How much of the slowdown in feature formation is due to depletion of ground ice versus external forcing?

2. At the end of the study, I was left wondering what the conclusions were in relation to the core questions/purposes of the study (how is thermo-erosion changing through time). Clearer statement of the purpose of the study would help this, as currently the results quickly get into comparisons within the dataset (e.g. % of sediment reworked done by an individual feature), leaving me confused as to whether thermo-erosion is expanding in this area and if formation is accelerating. The issue of units (addressed below) compounded this confusion.

3. I found the units of sediment and carbon counterintuitive and difficult to compare with other studies. Results are presented in absolute terms (total amount of carbon or





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sediment displaced from the whole study region) and it would be useful to state units normalized to area. Expressing material balance in terms of m2 would immediately let researchers unfamiliar with this area relate to the units and assess how important this process is. That would allow comparison of thermokarst mobilization of SOC and DOC to carbon release via active layer deepening. In this same vein, the number of features, which is focused on in the abstract and throughout the paper, seems immaterial compared to changes in area and volume. Ultimately, I had a hard time concluding at the end of the paper if thermo-erosion was increasing, decreasing, or remaining stable.

4. It is unclear how/if uncertainties were propagated through this exercise. Absolute numbers are given, rather than ranges or estimates of center and standard deviation (e.g. all the tables and figures). Without measures of uncertainty, it is difficult to assess the reliability of these estimates or identify sources of that uncertainty in the analysis.

5. There are multiple issues with visualizationsâĂŤparticularly the stacked bar plots using a logarithmic y-axis and the reliance on tables. Stacked bar plots on a logarithmic scale are visually misleading since the ice volume, which represents the majority of material lost, appears negligible. Additionally, could the x-axis of these plots be organized by some salient ecological parameter (e.g. precipitation, climate, surficial geology) instead of by geographic position? This would help provide insight into processes driving these patterns. The use of tables is fine in some cases, but I wanted a figure showing rate of thermo-erosion (normalized by area) for the two time periods (1952-1972, 1972-2011), which seems like one of the key punchlines of this paper. The tabular form makes it harder to rapidly compare changes and trends and ultimately is not more compact than a (non-logarithmic) stacked barplot of those time periods.

6. To cryosphere scientists, the subject of this paper is immediately of interest, but I fear that the abstract and introduction do not provide enough context for a non-specialist to see the need and implications of the study. Defining key terms (e.g. active layer) and providing more context for why this process is of general interest would increase the impact of this paper.





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7. The paper builds on many previous studies, but sometimes relies too heavily on explanations given in those studies. Especially on key issues like determining pre-formation ice content, DOC, and SOC, enough methodological detail should be given for the reader to assess the approach. At the bare minimum, given that many of these estimates are highly uncertain (e.g. reconstructions of ice content), an explicit treatment of uncertainties and how uncertainties were propagated is necessary.

Line edits: Page 1 Line 10: An additional line introducing the general context would be valuable Line 17-18: Standard SI format for number should be used (i.e. 8.6 x 10ˆ6 not 8600x10ˆ3). There are issues with this throughout the manuscript. Line 18: 53% of which was ice Line 21: 0.3% of the total OC flux for the Arctic Ocean? Unclear why this is of interest at this point in the paper. What percentage of the SOC stocks in the affected areas of the study region was mobilized by these features? Line 25: I believe this estimate is for the entire permafrost zone, not just the Arctic Line 27: Is it meant that air temperature has increased by approximately 3-4 degrees C? Air temperature in Celsius is expressed on a relative scale and it does not make sense to say increased by a factor of 3-4 (unless referring to change relative to absolute zero) Line 31: Non-standard terminology for thermo-erosion features. Following Kokelj, Jorgenson, Fortier etc., thermo-erosion or thermal erosion are the blanket terms that include thermokarst (permafrost collapse) and other erosive processes associated with permafrost degradation.

Page 2 Line 5: Consider including more recent modeling studies such as Koven et al 2015, Kessler 2017, or Sudakov and Vakulenko Line 9: Consider citing Abbott et al. 2016 or McGuire et al 2016, which summarize current modeling uncertainties stemming from exclusion of these parameters. Both of these studies directly support the need for the current study by emphasizing the importance of constraining thermo-erosion. Line 25: Word choice (potentially control or influence rather than forcing) Figure 1: Really nice figure. Potentially put the specific reach names in the SI (not of interest to most readers)

Page 6 Line 9: “In order to” can always be replaced by “To” Line 8: How was uncertainty for the compound assumptions in these analyses dealt with? Need more detail generally. Line 12: Why were these processes not included? How does that affect the estimates?

Page 7 Line 6-26: With the presented information, it is not clear if these estimates were downscaled from measurements of fluxes at feature outlets or if they are inferred from the mass of SOC there previously multiplied by volume displaced. If the latter, how are vertical differences in SOC accounted for this this framework?

Page 8 Line 3: Focusing on the number of features doesn’t seem terribly relevant to the question of the permafrost climate feedback. The area and volume results are more informative. In general, a few clear figures would more effectively communicate the observed patterns. Table 1: This would be more compelling in figure form. If table is retained, no need to use cryptic acronyms in the first column (i.e. L, Mm, Mr)âĂŤthere is enough room to spell out the parameters

Page 9 Table 2 would also be more effective in figure format. As currently presented, it is hard to tease apart what is changing across the timeseries.

Page 10 Table 3: This should be normalized to area covered by the geologic units. Are some of the units displacing more material per unit area or are the differences due to different relative coverages? No estimates of uncertainty are given. Figures 4 and 5. Problematic to show a stacked bar plot with a logarithmic axis.

Page 14 Line 18: Good example of why estimates should be normalized by area (i.e. expressed on a kg m2 yr basis or in mm/yr for sediment) Line 27: Still not clear how this affects the analysis. If the question is about total sediment and carbon balance, these processes, which are caused or at least facilitated by thermo-erosion seem pertinent

Page 16 Line 20: I think the authors are referring to Abbott and Jones 2015

References: Abbott, B.W. & Jones, J.B. (2015). Permafrost collapse alters soil carbon stocks, respiration, $CH_4$, and $N_2O$ in upland tundra. Glob. Change Biol., 21, 4570–4587.

Abbott, B.W., Jones, J.B., Schuur, E.A.G., III, F.S.C., Bowden, W.B., Bret-Harte, M.S., et al. (2016). Biomass offsets little or none of permafrost carbon release from soils, streams, and wildfire: an expert assessment. Environ. Res. Lett., 11, 034014.

Kessler, L. (2017). Estimating the economic impact of the permafrost carbon feedback. Clim. Change Econ., 08, 1750008.

Koven, C.D., Schuur, E. a. G., Schädel, C., Bohn, T.J., Burke, E.J., Chen, G., et al. (2015). A simplified, data-constrained approach to estimate the permafrost carbon–climate feedback. Phil Trans R Soc A, 373, 20140423.

McGuire, A.D., Koven, C., Lawrence, D.M., Clein, J.S., Xia, J., Beer, C., et al. (2016). Variability in the sensitivity among model simulations of permafrost and carbon dynamics in the permafrost region between 1960 and 2009. Glob. Biogeochem. Cycles, 30, 2016GB005405.

Sudakov, I. & Vakulenko, S.A. (2015). A mathematical model for a positive permafrost carbon–climate feedback. IMA J. Appl. Math., 80, 811–824.

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