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Interactive comment on "Permafrost thaw couples slopes with downstream systems and effects propagate through Arctic drainage networks" *by* Steven V. Kokelj et al.

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AUTHOR REPLY We are grateful for Dr. Murton's detailed and thoughtful review and provide replies to his comments in the following document. We feel that Dr. Murton's comments have improved the clarity of the manuscript and we were able to implement his recommended changes through minor editorial modifications to the text and some figures. Dr. Murton's contribution to improving our manuscript is recognized in the Acknowledgements section.

R1. Dr. Julian Murton The aim of the manuscript is to elucidate the [geomorphic, hydrologic and, to a lesser degree, sedimentary] processes and feedbacks that drive the [decadal] evolution of thaw-related mass movements and hillslope-channel coupling in ice-rich permafrost terrain of northwest Canada (lines, L108-111). The objectives (L121-123) are: " to better understand the (A) processes that drive the intensification of thaw-driven mass wasting and slope to stream coupling, (B) the distribution of catchment effects, and (C) their propagation across watershed scales,". The aim and objectives are important directly to the fields of permafrost geomorphology and hydrology, and indirectly to the fields of biogeochemistry, terrestrial and aquatic ecology, as well as to landscape management and ecosystem services. Personally, I would rephrase the objectives as aims (because the objectives given are really general statements of intent or goals) and identify specific objectives that signpost the ways in which the aims can be achieved and evaluated (because this is clearer in assessing how successfully aims are achieved). But to some degree this is a matter of author and journal preference. The methods used apply high-resolution three-dimensional survey techniques (light detection and ranging, and drone-based structure-from-motion) and geographical information systems (e.g. to construct digital terrain models and determine stream ordering) to drainage basins whose area varies by orders magnitude in a study region of 1 million km2 in NW Canada. This allows the authors to address terrain characteristics and functional geomorphic-hydrological relationships at localized to regional scales. The methods are appropriate to the aims and objectives, and are presented clearly, systematically and rigorously as far as I can tell, though I am not an expert in GIS analysis, and so I cannot comment usefully on pages 1-8 of the supplementary information. The results are largely new, clearly structured and presented well. The data represent a major contribution to terrain analysis on ice-rich permafrost, and the authors should be congratulated for bringing together this large and complex dataset. In particular, the focus on location of mass movements within catchments of different area, achieved through simple application of Strahler stream ordering, nicely identifies the first and second-order basins as particular centres of landscape change, and takes up functional and historical geomorphologists' consideration of scale and morphometric issues developed mainly since the 1950s in other regions. The narrative is illustrated

by effective figures and tables, though some minor points need clarification (see below). The three videos provide valuable supplementary information. The length of this section is fine. The interpretation is generally excellent, leading this reader step by step through the reasoning and contextualisation within the wider literature. The latter was particularly strong, as there has been substantial previous research on thermokarst terrain and processes in northwest Canada. The length of the interpretation could perhaps be shortened by 10-20% to avoid repetition and bring out the key messages more clearly. Likewise, the conclusions, in my view, could you shortened to a number of key points, though again I appreciate that this is a matter of preference. Overall, I think that this manuscript makes a substantial advance in our knowledge and understanding of the impacts of thaw-related mass movements on hillslope-stream coupling ice-rich permafrost catchments in northwest Canada. The approach used could be more widely applied in other regions of ice-rich permafrost (e.g. northern Alaska, NW Siberia and NE Siberia). I recommend publication subject to mostly minor revisions concerning points of clarification and typos, as listed below. Only two points of moderate significance are raised for consideration:

REPLY to comments by Dr. Julian Murton (R1) Following the recommendation of Dr. Murton we now refer to our statements of project intent as “Aims” or “Goals”. We have also endeavored to cut the length of the discussion and conclusions sections to avoid redundancy by about 10%. Minor elaborations required to address a few reviewer comments made a greater reduction of manuscript length difficult.

Moderate points Slope thermokarst (lines, L135-137, 195-197): Active-layer deepening and surface subsidence beneath a hillslope could reasonably be included in ‘slope thermokarst’, so I think this study is focussing on the most visible type of slope thermokarst, i.e. mass movement types. Perhaps this distinction can be made. ‘Thaw-driven mass wasting’ (L657) is a more accurate description of the focus of this manuscript than is ‘slope thermokarst’, in my view.

REPLY. We agree that slope thermokarst could indicate processes that extend beyond

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mass wasting. In the introduction, we clarify that the thaw-driven mass wasting process including retrogressive thaw slumping, shallow and deep translational failures comprise the most dynamic forms of slope thermokarst and that these processes are the focus of our study (L104). In some cases, we still use the term “slope thermokarst”, for instance where we make general statements such as “climate-driven intensification of slope thermokarst”. We have made numerous adjustments throughout the manuscript and refer explicitly to thaw slumps when addressing this specific process, and to thaw-driven mass wasting, permafrost landslides, or thaw-driven landslides when referring to a wider suite of processes that are included in broad-scale fluvial network analysis (L133).

Stabilization: a couple of sentences might be added to comment on the contrast between the recent decadal intensification of thaw-related mass movement and the stabilization of presumably the same terrain after the early Holocene climate warming. A reader might infer from the present argument that the recent trends are here to stay, which may be true for decadal and centennial timescales, but I wonder if the early Holocene landscape suffered even more change over even longer periods (millennial), and then stabilized, preserving abundant buried ice. The authors insights into thaw and terrain change may help elucidate negative feedbacks the thermokarst system, as Lawson, Shur and others have done successfully in terms of thermokarst around ice wedges etc.

REPLY. We are not overly comfortable speculating on the negative feedbacks in great detail as it is beyond the scope of this study. Here we focus on contemporary processes and on providing a geological explanation for rates and patterns of slope thermokarst acceleration. The evolution of negative feedbacks is likely to be of relevance over time scales greater than a century. However, we acknowledge Dr. Murton’s point and feel that it is a topic for further consideration in research examining conditions in the late Holocene through paleoenvironmental methods. We have added a sentence in the discussion addressing Dr. Murton’s point on L783-L786 “Several feedbacks could counteract the present intensification of thaw-driven mass wasting and increasing sedimentary and geochemical fluxes including climate cooling, exhaustion of sediment supply and progressive loss of ground ice from the most sensitive slopes, and gradual thaw-driven decreases in slope gradients, however, these factors are likely to be most relevant at centennial time-scales or greater."

Minor points L59: Prince of Wales Strait: mark on Fig. 1 REPLY. Changed as suggested

L62: North America's largest delta may be the Mississippi, a few thousand km2 larger than the Mackenzie. Please check. REPLY. Minor editorial adjustment made indicating "...North America's largest Arctic delta and the Beaufort Sea."

L77: insert 'ice-rich' into this topic sentence, because thermokarst activity will not really affect permafrost with little or no ground ice, e.g. '...evolution of circumpolar ice-rich landscapes...' REPLY. Changed as suggested.

L85: specify the nature of 'Arctic change' in the topic sentence as this encompasses many things, e.g. 'of environmental change in Arctic terrestrial and aquatic systems'. REPLY. Changed as suggested.

L93: 'have' [subject is plural] REPLY. Changed as suggested.

L106: do you mean 'thickness' (an interval) rather than depth (a single point), i.e. permafrost thickness? REPLY. Changed as suggested.

L108-113: please shorten and simplify this long, complex sentence. It's a bit difficult to follow. REPLY. Changed through editorial modification.

L121-124: this key sentence identifies the aims of the study. I think it would be clearer to simplify and rephrase along the lines 'The aims of the present study are (1)...' rather than squash them into a long introductory clause. The geographical region is of secondary importance relative to the more generic aims. Also, please specify the type of processes in (A), e.g. geomorphic, thermal ..., and the nature of the distribution in

(B), e.g. spatial and/or temporal. REPLY. Changed through minor editorial modification.

L130: append ‘climate’ to ‘cooling Holocene’. REPLY. Changed as suggested.

L141: Tuktoyaktuk Coastlands [with an ‘s’] REPLY. Changed as suggested.

L144: clarify what is meant by ‘fluvial patterns’, e.g. river channel morphology, bedform architecture, sediment transport. . .? REPLY. Changed through minor editorial modification.

L145: indicate Mackenzie Delta on Fig. 1 REPLY. Changed as suggested.

L151: indicate Amundsen Gulf on Fig. 1 REPLY. Changed as suggested.

L178: replace ‘middle’ with ‘medium’ REPLY. Changed as suggested.

L181-184: indicate approximate depth of mean annual ground temperatures as much of the deeper layers of permafrost on Banks and Victoria islands etc. will be much warmer than -10oC. REPLY. Depth reference is now indicated as the mean annual temperature at the top of permafrost (TTOP).

L192: insert ‘other’ before ‘glacigenic materials’ as tills are glacigenic. REPLY. Changed as suggested.

L200: datasets: it’s essential to identify all of the datasets used in the study rather than the non-specific word ‘include’. REPLY. A new supplementary table has been added (Table S1) to make explicit the datasets used in this study and the research questions that they address. “To examine processes driving the intensification of thaw-driven mass wasting and the patterns of effects across hydrological networks we applied multiple methods involving field study and mapping at slope, catchment, and watershed scales described in the following sections and summarized in Table S1.”

L210: clarify what is meant by ‘a continuum of slump features’. Continuum in what sense: activity, size, aspect. . .? REPLY. Minor editorial adjustment made to clarify that the slumps represent a size continuum.

L228: subsidence: did some of the volumetric change on the slopes resulted from permafrost thaw and thermokarst subsidence beneath slump-floor sediments (cf. Burn 2000, CJES 37:967–981) or can this process be discounted? REPLY. We make a minor editorial adjustment to clarify that volume changes within the scar zone include both thaw-subsidence and slope erosion.

L238: volumetric erosion: careful, the study is not directly measuring erosion but inferring erosion based on measurement of volume change. So an explicit parameter such as 'volume change' or 'disturbance volume' (L344) is more appropriate. There may be a better term, as I'm not familiar with GIS methodology. REPLY. We replaced "erosional" with "disturbance".

L240: active or recently-active scar and debris tongues: on what criteria were these identified as such? e.g. lack of living vegetation or some indirect evidence of vegetation? L493 mentions bare or sparsely vegetated landforms. I find it difficult to know from many GIS studies what actually is being observed directly and what is being inferred. REPLY. We clarify in the text that active or recently-active scar and debris tongues were interpreted by a distinct scarp and bare or sparsely vegetated scar area determined with the support of high-resolution orthomosaic imagery.

L245, and supplementary L277: 'including': it is clearer to identify all of the criteria for designating a slump as '2'. Were there any other criteria besides the two mentioned, e.g. turbidity in rivers, as per caption of Figure S3ii? REPLY. The text is clarified to indicate that evidence of downstream deposition is expressed as a debris tongue deposit in a valley bottom, or a sediment lobe protruding into an adjacent lake or coastline.

L275: insert 'and' after '2010'. REPLY. Changed as suggested.

L299: "All mapping was reviewed for accuracy and consistency." Please explain how or cite a reference that does. REPLY. Reference is provided to Kokoszka and Kokelj, 2020, which provides a detailed description of methodology and QA/QC procedures.

L319: data are treated as plural in this sentence; previously (e.g. L166, 209) they are treated as singular. Please ensure consistency. REPLY. Minor adjustments on L166 and 209 ensure that data are treated as plural.

L331: routing . . . 'was' REPLY. Changed as suggested.

L365: intensifying slope thermokarst: this implies that the rate of growth or the increase in number of slumps or both factors is accelerating in all cases. Is this correct for all slumps or just for some, e.g. CB? REPLY. Minor editorial modification was implemented to clarify the sentence.

L366: 'eroded volume': again, can you be sure that thermal erosion accounted for all of the missing volume, or might thermokarst subsidence have contributed to the missing volume? REPLY. We clarify that volume loss associated with thaw slump development is a function of both sediment erosion and subsidence due to ground ice thaw.

L383: delete 'retreat' because the photograph shows the headwall but not its retreat. REPLY. Changed as suggested. Please note that based on comments from both Reviewers we have rearranged Figure 2 and the caption to increase its clarity. We have added elevation normalized slope profiles to the debris tongue profiles to better illustrate the connection between thaw-driven erosion and deposition. We also have created a supplementary figure (S1) to show terrain models of the slumps and locations of these transects.

L384: Ditto 'erosion'; the photograph showed 'eroded glaciofluvial deposits', not their erosion. Also, add 's' to 'deposit'. REPLY. Changed as suggested.

L387: again, the photograph in panel d does not show 'initial stages of incision', but an incised channel. Panels c and e may show evidence of side valley erosion, but they don't show any erosion itself. REPLY. Changed as suggested.

L390: please indicate (e.g. with an arrow) the snow patch, as it's not obvious to me at least. REPLY. Caption was adjusted to increase clarity and reference to the snowpatch was removed.

L383 & 390: please clarify the caption ‘Elevation normalized debris tongue profiles. . .’. The y axis of the plot shows thickness, so I think this should be added to the caption, e.g. ‘elevation normalized profiles of debris-tongue thickness’. Also, there are in total three white dashed lines on panels b, f and g, but four lines depicting profiles on the plot in panel h. REPLY. Figure 2 has been adjusted to increase its clarity and white dashed lines have been removed.

L394: if this refers to slump area as opposed to e.g. headwall height, then it is clearer to rewrite, e.g. ‘. . .the area of FM2 was an order of magnitude greater than . . .’ REPLY. Changed as suggested.

L397-8: ‘Increasing thaw-driven sediment flows. . .’: please clarify if this refers to their number, magnitude, rate or . . . REPLY. Minor editorial adjustment was implemented to clarify the text.

L401-402: ‘pinning of the stream channel to the valley wall (Fig. 2c)’: please indicate this (e.g. with an arrow) on Fig. 2c, as it’s not very clear to me where the stream channel is. REPLY. Figure 2 has been adjusted and photograph was removed.

L402: better to replace ‘massive deposits’ with ‘thick deposits’, as the former, in the context of sedimentary deposits, suggests that they lack sedimentary structures, which may or not be the case, as they are not described. REPLY. Changed as suggested.

L406: ‘abrupt transition from small valley-side thaw slumps into larger, more dynamic features’: I’m not sure that the data on area of slump CB support this, as within 7-9 years of slump initiation CB was 25,900 m2 (by 2011), i.e. growing at a few thousand m2 per year, whereas 4 years later it was 33,370 m2, which suggests a broadly similar rate of expansion. What does look to have been abrupt, is the sudden evacuation of slump-floor deposits since 2017. REPLY. A slight adjustment in the topic sentence was implemented to better characterize the transition in thaw-driven slope evolution observed in the study area. “The abrupt transition of shallow valley-side thaw slumps into more dynamic failures connected to downstream environments is transforming the geomorphology of ice-rich glaciated landscapes.”

L434: delete comma after ‘geomorphology’ REPLY. Changed as suggested.

L493, 525-6 and Fig. 6: large translational slides: what criteria are used to identify these landforms and to distinguish them from thaw slumps? Are they different from active-layer detachments? How do you identify bedrock control? REPLY. The morphology of rotational or translational failures differs from thaw slumps. Field observations throughout the Willow River catchment confirmed the interpretation of remotely sensed imagery. The text has been adjusted accordingly.

L495: Fig. 5a is first mentioned after Fig. 6 (L493). Please correct numbering. REPLY. Changed as suggested.

L498: depth of maximum thaw: how is this value determined? Do you mean the maximum concavity depth in L506? REPLY. Changed to “maximum concavity depth”.

L511: ‘from 2002 to 2018’ REPLY. Changed as suggested.

L515-16, 739-41: “Normalizing by catchment area and differencing with the preceding time interval, the thaw slump component of surface lowering amounts to 0.1 mm yr-1 for 1986-2002 and 0.8 mm yr-1 for 2002-2018.” This seems to me to be a rather strange and spurious parameter to calculate because surface lowering in thaw-slump terrain is not uniformly distributed, but focussed in discrete locations. An alternative, perhaps more meaningful parameter to calculate would be volume lost per unit area (cf. sediment yield), because this does not imply that the lost volume is uniformly distributed across space. REPLY. We have reported as a surface lowering amount (mm yr-1) and also in terms of sediment yield (m3 km-2 yr-1). We initially did not report the latter (cf. sediment yield) because it too integrates estimates across the entire catchment source area and assumes that materials are getting to a catchment outlet. Regardless, we

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now report both values and remind readers that a significant portion of the volumes is attributed to ground ice (50 to 80%) and that a large portion of the sediments mobilized from thawing slopes are placed into transient storage in valley bottoms (see Fig. 2).

L529: were [km are plural] REPLY. Changed as suggested.

L546: box and whisker plots: please state what each part shows, e.g. horizontal line denotes median, ... dots indicate outliers ... REPLY. Minor editorial adjustment implemented to clarify text.

L547: narrative specifies 'concavity thicknesses' whereas Y axis on panel f is ... 'depth'. Please ensure consistency. REPLY. Changed as suggested.

L555: the proportional circles are grey rather than black. REPLY. Changed as suggested

L559-560: 'Willow Lake (outlined in Orange)': where is this on panel c? Lower case for orange or simply add a label 'Willow Lake'. Please renumber panels to avoid three panels all labelled b, and three labelled c. 'The abandoned channel is shown in dark blue': in panel c the lakes look to be coloured dark blue in Fig. 6. Or are you referring to the unlabelled panel? This is difficult to follow the caption without sequential labels on all panels and text placed accordingly. REPLY. We have adjusted the caption and made minor edits to the figure. All elements of the main map are indicated in the first part of the caption, including reference to the inset boxes. The panels are labeled sequentially and explained in the caption.

L586-591: This summary of literature is more appropriate for a discussion than a results section. REPLY. References are removed from the text.

L589: indicates (with 's'; compilation is singular) REPLY. Changed as suggested.

L602 & 604: both 2017 and 2018 are indicated in caption but only 2017 is shown on panels a to c. REPLY. Editorial clarification implemented. The Sentinel-2 data are for 2016-2017 and text and figures have been checked for consistency.

606: streams and rivers: what is the difference? Insert 'of' before 'the Peel. . .' REPLY. Changed to "streams".

L661: suggest [plural subject] REPLY. Changed as suggested.

L672: 'rapid aggradation of channel beds': perhaps 'rapid aggradation of valley fills or sediment bodies' is more appropriate. Deposition of the valley fill in Fig 2 looks to have been mainly by debris-flow processes rather than channel processes (cf. L716-20). The channel shown in Fig. 2d has incised its bed. REPLY. Changed as suggested.

L693: is complex [subject is singular] REPLY. Change as suggested.

L752: is magnified [subject ('the significance') is singular] REPLY. Changed as suggested.

L775: what is a 'discordant volume'? REPLY. Minor editorial modification implemented to clarify text.

L823: 'persistent perturbation': please specify the timescale of persistent or omit. Over decadal and possibly centennial scales, the perturbation may well be persistent. But geologically (multi-millennial and longer scales), the perturbation is certainly major but transitory, as the conceptual framework proposed by Ryder, Church, Ballantyne and others infers a pulse of sediment movement that declines over time. REPLY. Minor editorial modification implemented to provide a timescale (decadal to millennial) over which perturbations are likely to persist.

L839: again, please clarify what timescales are referred to as 'long-term'. L844 identifies centennial timescales. REPLY. Minor editorial modification implemented to provide a timescale (centennial to millennial) over which perturbations are likely to persist.

SuppL381: correct to 'cloud-free' REPLY. Change as suggested.

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