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Joanmarie Del Vecchio et al.

Author comment on "Patterns and rates of soil movement and shallow failures across several small watersheds on the Seward Peninsula, Alaska" by Joanmarie Del Vecchio et al., Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2022-43-AC1, 2023

Dear reviewers and Associate Editor,

We are pleased to present a greatly improved version of the manuscript, “Patterns and rates of soil movement and shallow failures across several small watersheds on the Seward Peninsula, Alaska” by Del Vecchio et al. We appreciate the thoughtful comments, questions, and suggestions from reviewers and have made edits accordingly.

Our revisions are primarily concerned with (1) clarifying figure data, annotations, and captions and fixings errors from the previous iteration, and (2) re-arranging of some analyses such that all field and data approaches are listed in Methods, findings are listed in Results, and thoughtful, clear analysis the of findings and their significance are found in Discussion. Of note here are clarifications with InSAR methods, GPS methods, and results of surveys of interannual movement of lobate features. We have also moved figures previously in the Discussion to the Results, so there is some re-numbering of figures.

We have updated the Zenodo DOI to reflect the updated codebase for this work, which includes scripts to produce figures and calculations included in these revisions. We have added some additional figures to the Supplement for additional insight.

Our response to reviewers’ comments can be found below. Thank you for the opportunity to revise the manuscript for resubmission.

- J. Del Vecchio

Anonymous referee #1

This manuscript is in a good shape. It goes over the measurements of soil motion and failure in a permafrost environment and looks for potential explanations (slope, precipitation, permafrost, …).

Response: Thank you!

However, here is a list of comments I would like to see addressed:

l.74: "topography model" -> "digital elevation model (DEM)"

Response: Revised.

l.78: add latitude?

Response: Added.

l.114-124: how long were the GNSS observations at each marker corner?

Response: The antenna was held at each survey position for at least 30 seconds. We have added this information to the text.

Figure 2: You should write that the outline color of the sub-panel is related to the color of the area on panel (a). It took me while to realize. Is there no precise location for (f) to be shown by an arrow? I thought it was indicating the green box first. "The translation of a group of shrubs downslope is demonstrated with their position outlined in yellow" it is hard to see the yellow color, maybe we just say thin/thick lines? Is there also two groups of shrubs (two outlines)? If so it should be mentioned. Also are the UAS survey covering the entirety of the panel (a)? If not you should show the coverage, otherwise we can think there were no failure in some areas while it is only because there is no observation over these areas.

Response: We have made the colors of the boxes and the associated letters clearer by adding letter labels to the boxes on part (a). We have added a precise location for (f). We have emphasized the outlines of shrubs and have clarified the second outline pertained to another incipient failure in 2018. We have also added the footprints of the 2018 and 2019 UAV surveys.

l.157-170: Your area is small, why not process the data at a higher resolution? Where there no available Sentinel-1 observations with a westerly looks to observe the Teller 47 hillslope?

Response: The relatively low resolution (multilooking of 5 by 13 SLC pixels) was necessitated by the low coherences in this wet and shrubby environment.

Images from the opposite look direction, i.e., from the ascending pass, are only acquired sporadically by the Sentinel-1 satellites. For instance, not a single ascending image was acquired from June to August 2019. We now state this explicitly in the manuscript:

"Unfortunately, a dense time series of Sentinel-1 observations from the opposite look direction is not available." (Lines 236-237)

l.159: "11-day intervals" Aren't the Sentinel-1 images acquired every 12-days?

Response: Corrected to "12-day intervals"

l.246: "negative interannual vertical movements (inflation)". Usually negative vertical motion mean subsidence, I would change the text to "upwards interannual vertical movements".

Response: Revised

Figure 3, caption: "(a) Map of displacement vectors…" for what time period?

Response: We have revised the caption to clarify that 3(a) represents data from 2018-2019 (in order to compare lobes to GCPs), whereas 3(b) represents data from two years of surveys (because lobe data only exist from 2017):

(a) Map of **2018-2019** displacement vectors including horizontal displacements (arrows) and elevation changes (squares for lobes, circles for UAS targets). Arrow and symbol size scales with magnitude. Elevation data are the same as Figure 1b. Figure 3b location shown as dashed white box. (b) Zoom-in to orthoimage derived from UAS imagery to vectors associated with lobate features. **Arrow and square size represents average annual displacement vectors across the two years (2017-2019). (Lines 360-364)**

l.274-275: Isn't "mid-August" acceleration for 2018 similar to 2019 acceleration (beginning of august for one month)? If so why use "conversely"?

Response: With this sentence we wished to highlight the contrast in the timing that in 2019, accelerated displacement rates began earlier. We agree that this can still be communicated without "conversely" so we have removed the word.

Figure 4: Would it be possible to change "day of year" to an approximate date instead? It would make it more easy to compare with the text (where you mention "day of year" only once).

Response: You know, this took a lot longer to code than I expected but we got it!

l.293-294: Can you explain what "unconditionally unstable", "conditionally stable", and "unconditionally stable" mean?

Response: We use the nomenclature of Montgomery and Dietrich (1995) in which "unconditionally unstable" represents a force balance in which the soil column should be unstable even without the addition of water to the soil column, "conditionally stable" conditions mean that depending on the volume of water added, failure may or may not be expected, and "unconditionally stable" means that even a saturated soil column should not fail. We have revised the sentence as follows:

"Thus, we consider landscape positions for which q_{cr}<10 mm day^{-1} to represent unconditionally unstable positions **(failure of the soil column is predicted to occur without the addition of water),** q_{cr} between 10-60 mm day^{-1} to be conditionally stable **(failure dependent on water volume added)**, and q_{cr}>60 mm day^{-1} to be unconditionally stable **(failure not predicted at saturation).**" (Lines 427-430)

l.451: "Thermokarst" this word is only used here in your text. You should say more about it or remove it.

Response: Revised to process-agnostic word "disturbance"

Figure 9: You mention trend over the last century often like "Both (a) and (b) appear cyclic over the past century." You should show the data and trends then.

Response: We have clarified that the data being discussed are Figure 9(a) and 9(b) in the text with figure references (and have emphasized in the figure caption that these data are from the Alaska State Climate Center; see other responses), and we adjusted the language to be trend-agnostic because fitting the data to a trend, cyclic or otherwise, is beyond the scope of this paper. The edited text reads:

"Decadal trends in **annual maximum precipitation (Fig. 9(a)) and cumulative precipitation (Fig. 9(b)) vary over time**, perhaps driven by the Pacific Decadal Oscillation (Bring et al., 2016a)…" (Lines 703-704)

Anonymous referee #2

The paper presents an impressive and novel study on how soil moves in the Arctic and how its movement is affected by a changing climate. To investigate this, a variety of techniques is applied, including DGPS, UAV and InSAR measurements and modeling. As many previous studies used statistical approaches, this is an original approach which substantially contributes to scientific progress by presenting important results on Artic soil movements and controlling factors on different scales.

While I did enjoy the detailed descriptions of field observations, including field photos, and the discussion of the findings in a wider context, the manuscript is overall quite long (> 9000 words excluding references). It discusses so many different results and aspects that it is hard to find the key messages in the text – which are very nicely stated in the conclusion. The addition of new methods and results in the discussion section does not help the structure and the discussion sometimes seems to drift a bit too far from the results that are actually presented. It would be great if the structure of the manuscript could be improved (clearly separate method, results and discussion) and the key findings better presented in a tightened discussion. Shortening the manuscript could help to better convey its key messages.

Please find details below. I am looking forward to read a revised version of this nice and interesting paper!

Response: Thank you for your detailed comments. We hope the revised manuscript, which includes edits made in response to proceeding comments, reflects a tightening of the text, and the logical restructuring of sections assists in reader comprehension. Specifically we have taken your suggestions and re-arranged sections that were misplaced in methods, results and discussion. We hope that the improved clarity of the results justifies the information we include in the discussion.

25: What does topographically smooth mean? No landform? So not only no solifluction lobes but also no solifluction steps? Could this be a solifluction sheet? Please clarify, e.g. using the solifluction terminology in the IPA glossary (https://www.permafrost.org/wp-content/uploads/Glossary/Glossary_of_Permafrost_and_Related_Ground-Ice_Terms_1998.pdf).

Response: We have clarified that these measurements were made in locations without solifluction lobes as defined by the glossary referenced (isolated, tongue-shaped features). We will also adjust the text to clearly define solifluction lobes (and to highlight the existence of solifluction terraces at the site as well) as such. The text in the abstract now reads:

"Despite clear visual evidence of downslope soil transport of solifluction lobes, we find that the interannual downslope surface displacement of these features does not outpace downslope displacement of soil in **locations where lobes are absent** (downslope movement means: 7 cm yr^{-1} for lobes over two years vs 10 cm yr^{-1} in landscape positions **without lobes** over one year)." (Lines 23-26)

27: What is a comparable temperature landscape? Comparable in terms of which properties? Please clarify.

Response: In the Discussion, we write,

The low end of these estimated diffusivities is higher than >80% of diffusivities compiled by Hurst et al. (2013) and igneous and metamorphic landscapes by Richardson et al (2019) in middle latitude landscapes. (Line 532)

We have clarified for the abstract "and volumetric sediment fluxes are high compared to temperate landscapes **of comparable bedrock lithology**"

Background and study area: Is is a bit unclear which "background" is presented, maybe the title "study area" would be sufficient? Please add a short overview of the different landforms and geomorphic processes (solifluction lobes, sheets, which type of other slope failures?) you investigated in your study area.

Response: We agree and have adjusted the section title to be simply "Study area." We have added the following text to describe the landforms to a first order, because our results delve into more detailed descriptions of the landforms and processes:

Unglaciated hillslopes support a rocky, thin (up to 1-2 m) soil mantle that is commonly organized into **turf-banked** solifluction **lobes (defined as isolated, tongue-shaped features with relatively smooth upper surfaces)** and sweeping **solifluction** terraces **(steps or benches with straight fronts)** (**Harris, 1988;** Kaufman et al., 1989). (Lines 121-124)

110: "of what appeared to be solifluction lobes" – where they solifluction lobes or not? In the abstract, you talk about solifluction lobes, so I assume these were indeed solifluction lobes?

Response: Revised accordingly; see previous comment for additional context.

Can you maybe shortly outline in this section how the many different methods you used come together for your analysis? What exactly did you measure with which methods on which spatiotemporal scales and which results do you compare/integrate?

Response: We have revised the paragraph to read as follows:

"We collected field and remotely sensed data from several small watersheds along the Teller Road near mile point 47 and hence we refer to the site as "Teller 47." **We collected differential GPS data in June and July 2017, July 2018 and August 2019, and during the 2018 and 2019 campaigns imagery was collected via uncrewed aircraft system (UAS)**. Our August 2019 field season began five days after an unusually intense rainstorm between 1-3 August; **detailed field observations were made at the site to document the geomorphic and hydrologic consequences of the storm.** A meteorological station located within the study area (442937.59 E, 7206428.18 N UTM, 67 meters above sea level; Busey et al, ongoing from 2018) that records data every half hour recorded 77 mm of rain in 65 hours, with a peak rainfall intensity of 9.6 mm h-1 on 2 August; **these data were supplemented with historical climate data from the Nome Regional Airport (Alaska Climate Research Center). We also mapped subseasonal surface movements across the landscape using InSAR (Interferometric Synthetic Aperture Radar) from satellite data from 2014 to 2019. We synthesized these datasets to connect discrete and continuous measurements of surface displacement to climate and topographic drivers**."
(Lines 146-156)

3.1 Differential GPS: The GPS setup remains partly unclear: How was ensured that the corners of the plastic rectangle don't shift? This depends on the position of the rebar rods, which is not clear. How could the tops of the rebar rods be measured if they were hammered 50 cm below the ground surface?

Response: The shift in target corners is in fact the displacement we are interested in measuring. We did not observe internal deformation of the targets themselves such that any movement should be a wholesale translation of the target. Because we average the position of the target by its corners and center rebars, any rotation of the targets would not be observed as displacement, but a net downslope and/or subsidence of the target would be registered.

We have clarified the nature of the rebar securing the targets in the text as follows:

The markers are white plastic rectangles measuring 40 x 70 cm and were secured with two rebar rods in the centre of the marker**. The bottom of the rebar was hammered to a depth of up to 50 cm, and the remaining protruding rebar at the surface was capped with a rebar cap. When measuring the height of the target at the rebar, we measured the heigh of the rebar caps and subtracted the height of the rebar to calculate the position of the target flush to the tundra surface.** (Lines 159-163)

GPS accuracy (1.8 cm) is already in the range of solifluction movement for several sites in the Arctic (see Matsuoka 2001, Table 1), this should shortly be mentioned, although luckily your slope is moving quicker.

Response: We have added a reminder of the nature of the uncertainty to the results:

Mean annual horizontal and vertical change of lobe targets from the two-year period of 2017-2019 was 7.1 ± 4.3 cm (1 st.dev.) and 8.7 ± 5.6 cm, respectively (Figure 3) **(with uncertainty deriving from the 1.8 cm uncertainty of DGPS measurements**). (349-351)

127: Can you elaborate on which landforms (if any) you placed your targets? This would help to understand why they move more/differently than the lobe targets.

Response: We have revised the text where there are references to merely "lobes" to specify "solifluction lobes;" see previous comments about adding definition earlier in text.

139: How were the additional georeferenced UAS images collected? In a same way as in 2018 (flight + DGPS points) or differently? Please clarify.

Response: We have expanded on the details of this method as they are important for understanding the resulting data:

We also collected an additional ~300 georeferenced UAS images of slope instability features. We performed an initial orthomosaicing of the 2019 images based on the georeferencing of the images, after which resulting orthomosaics were georeferenced to the 2019 imagery based on identifiable control points in both images such as shrubs, rocks, trees and other presumably stable features. Because we targeted 2019 photography for failures, a complete orthomosaic of the 2018 survey area was not collected. Any failures mapped in the 2019 imagery that were also present in the 2018 imagery were manually removed from the 2019 inventory. (Lines 199-205)

138: With which resolution did you measure permafrost depth using tile probes? E.g. probing every 10 m, every 100 m?

Response: Permafrost depth probing was done opportunistically as we surveyed the site, and we mostly focused on contouring the apparent lack of permafrost near the channels. We have updated the text accordingly and the paragraph now reads as follows:

"In August 2019, we surveyed the Teller 47 watershed with GPS points, photographed changes and measured depth to permafrost using tile probes… Permafrost depth probing sites were selected opportunistically as we traversed the watershed and thus the permafrost-free extent mapped in Fig. 2a is approximate." (Lines 198-213)

144: Can you maybe add where you dug trenches in Fig. 2?

Response: We have added trench location to Figure 1(b) as the focus of Figure 2 is mapped failures.

144: Please justify why dense shrubs and trees indicate absence of permafrost using references.

Response: Through field surveys and probing at our site, we observe a strong association between deeper active layers and/or nonexistent permafrost and the presence of shrubs and grass. This trend was also observed at a nearby site that has similar vegetation, topographic, and permafrost attributes to our site (Uhlemann et al., 2021). We can therefore reasonably extrapolate and estimate permafrost presence or absence to areas that we did not directly probe based on surface vegetation patterns. We also emphasize the qualitative and opportunistic nature of the permafrost mapping, which has been addressed in another comment and manuscript text clarified.

We have added detail to the text:

These observations were used to map permafrost-free locations and inform estimates of the extent of thawed areas along drainages based on both the presence of dense shrubs and trees and the presence of grasses instead of tundra, **a robust pattern observed at both our site and with geophysical observations at a nearby site (Uhlemann et al., 2021)**. (Lines 207-210)

167: What does "LOS" mean? Please clarify.

Response: We have added "The line-of-sight (LOS) motion" to the first reference to line-of-sight in the text in section 3.3 InSAR.

For me it remains somewhat unclear what you actually measured with InSAR. Does it mean you measured displacement only for the specific sites J1-J5? Those are rather far from Teller 47 and their geomorphology, permafrost conditions etc. remain unclear. So how are those large scale displacement measurements comparable to the lobe scale displacements measured with DGPS?

Response: We agree that the discrepancy in the spatial scales presents a challenge that we did not acknowledge in sufficient depth. Despite this challenge, the primary motivation for studying the InSAR data is that they allow us to identify subseasonal patterns in movement, of which those related to the large 2019 precipitation event are of greatest interest. The InSAR data's synoptic view (i.e., measurements over a large area at a given time) is essentially impossible to replicate with field-based methods and thus offer a complementary (but not validation) dataset to the DGPS data.

We have revised the InSAR methods to clarify this point. "While these observations cannot resolve fine-scale processes such as the shallow failures in Fig. 2, we studied them to identify their association with rainfall events on the hillslope scale"

Please provide information on InSAR accuracy, resolution of InSAR results and more information on InSAR processing in general (see e.g. Rouyet et al. 2021).

Rouyet, L., O. Karjalainen, P. Niittynen, J. Aalto, M. Luoto, T. R. Lauknes, Y. Larsen, and J. Hjort. 2021. Environmental Controls of InSAR-Based Periglacial Ground Dynamics in a Sub-Arctic Landscape. Journal of Geophysical Research: Earth Surface 126:e2021JF006175.

Response: We have greatly expanded the description of the InSAR processing. The most relevant paragraph has been revised to:

To estimate the line-of-sight movements, we used a short baseline (SBAS) approach (Berardino et al., 2002). We first formed the interferograms at a resolution of 100 m by multilooking with a rectangular boxcar filter (13 range x 5 azimuth samples). Owing to the rapid loss of coherence, we only processed interferograms with temporal separations of up to 24 days. andand the interferograms were then unwrapped using the SNAPHU package, which is based on a network optimization approach (Chen and Zebker, 2001) interferograms with temporal separations of up to 22 days for each year. WeAfter geocoding using the TanDEM-X 90 m DEM, we referenced the interferometric phases at bedrock outcrops, which we assumed to be stable. We then estimated the movement time series from the interferogram stack by weighted least squares, with the weights determined by an the Cramér-Rao coherence-derived estimate of the phasespeckle-induced variance (Zwieback and Meyer, 2021). The standard error of the Sentinel-1 displacement estimates in Northwetsern Alaska was estimated at approximately 1cm by Zwieback and Meyer, 2021. We consistently adopt the sign convention that positive movements correspond to increasing LOS distance between the satellite and the surface. (Lines 249-259)

It remains unclear here how you want to find out the role of topography without using terrain data. You mention only later in the discussion that you also analyzed terrain data. This needs to be added here.

Response: Our terrain data used to process the InSAR data derive from resampled ArcticDEM smoothed to 15 m. We have added this detail to the text in section 3.3 InSAR.

We have clarified the terrain criteria we applied to selected the locations and why the observations from the Teller-47 location are unreliable.

To constrain the subseasonal movements of hillslopes in the study area, we focused on five locations close to the Teller 47 site. The reason we did not analyse the movements at the Teller-47 site are the unfavourable viewing geometry: steep southeast-facing slope with destructive interference of downslope and surface-perpendicular motion, and low coherence due to foreshortening. Instead, we selected five nearby sites of predominantly west-facing aspect (determined using the TanDEM-X DEM) with similar surficial characteristics (Fig. S1). we chose 5 nearby sites (labelled J1-J5 in Fig.1A) with more favourable LOS. We checked these locations with favorable viewing geometry manually to rule out phase unwrapping errors, which manifest as regions where the displacement estimates have an offset (Figure S1).

Infinite slope model of stability: Model setup: You used an overall soil thickness of 1 m, however, you found in your field survey that there are large permafrost free areas (Fig. 1a). Wouldn't it make sense to incorporate the spatial permafrost patterns you are aware of?

Response: We do not know the nature of the relative timing between how long permafrost-free areas have lacked permafrost and when the pre-2018 failures within those boundaries occurred. We also do not have good constraints on the depth to bedrock for the permafrost-free zones.

The model is largely sensitive to the value chosen for the transmissivity (hydraulic conductivity) of the soil. For demonstration purposes we show model results that compares the critical rainfall which is necessary to cause failure where depth to impermeable bedrock is 3.0 m versus our base case of 1.0 m:

These results demonstrate that, when using a deeper active layer, we have merely shifted parts of the landscape that have low upslope contributing areas (meaning they are less likely to be saturated) from "conditionally stable" to "unconditionally stable," though we note that the daily rainfall values for failure are physically unreasonable. For a given rainfall volume, deeper active layers have a stabilizing effect on model results (rainfall to induce failure must increase), but this effect is negligible if hydraulic conductivity is so low that water cannot infiltrate and take advantage of increased storage. In simple words, these model results say, "No matter how thick the active layer is, impermeable soil will always promote slope instability and overland flow."

Movement rates from targets: It remains unclear how you derived the movement rates from the GPS targets on the lobes and for the UAV survey. Please add a short explanation.

Response: We have added additional text (in bold) to Section 3.1:

To compare interannual movement we averaged the 4 XY positions at each marker corner to produce a single coordinate for a target each year with an uncertainty (calculated via standard error) of ± 1.8 cm horizontal and ± 3.2 cm vertical. **We then calculated the distance between each years' target-averaged point in both the horizontal and vertical to determine annual displacement**. (Lines 130-133)

208: Looking at Fig. 2c, it seems like the failure occurred on the lobe tread, not the lobe front/riser. Please clarify in the text and Fig. 2c (see later comment on Figure)

Response: We have edited "fronts" to "treads."

213: This appears to already be an interpretation, which seems better suited for the discussion.

Response: We have edited the text to read as more clearly an observation:

Where failure occurred on lobe treads, portions of the tundra mat were translated downslope but remained intact. (Line 241)

257: Did you test statistically if there was a significant difference in movement between the lobes and the GCPs? If not please test or rephrase.

Response: We have performed a t-test for independent samples with the SciPy package to test the difference between 2018-2019 movement in lobes and GCPs and found no statistically significant differences. We have included this detail in the text and supply resulting figures in the Supplement:

A t test for independent samples applied to 2018-2019 displacements (Virtanen et al., 2020) found no significant difference between magnitudes of movement of lobe targets and magnitudes of movement of GCPs laid out in a grid (see Supplement). (Lines 369-371)

267: What exactly is the "satellite watershed"? I cannot find it in Fig. 3A.

Response: We have updated both Figures 1 and 3 to show the location of the smaller, instrumented watershed to the south and west of the "main" watershed.

281: "Figure" is missing before the references to the figures.

Response: Edited.

308-311: This section belongs into the methods description. Please adjust.

Response: We have moved the description of the correlation between displacement rates and topographic metrics to the end of Methods section 3.1 Differential GPS.

312-316: This section belongs to the results. Please adjust.

Response: We have moved the description of the results of topographic metrics versus displacement to the end of Results section 4.1 Differential GPS interannual displacement measurements.

This discussion of the controls on your slope scale soil movements should be more comprehensive. You measured permafrost on the slope scale, so does this play a role? And does it maybe matter where on the slope/lobes you actually placed your targets?

Response: We agree that the differential displacement across a lobe with more than one target or clusters of targets should warrant some discussion. We include additional text in the Results to describe intra-lobe patterns, and have also included zoom-ins of lobe target clusters as Supplemental figures. The added text is:

We did not observe a systematic pattern of movement with position on lobes across the landscape. On two lobate features, targets closest to the lobe front moved faster than targets upslope on the tread (Fig. 3(b)), but this pattern was not observed at other locations where a lobe had more than one survey target (see Supplement). (Lines 352-356)

Previous studies showed that soil movement differs strongly within solifluction lobes and that parent slopes are usually moving more slowly than the solifluction landforms on them (e.g. Ballantyne, 2013, Eichel et al. 2020, Harris et al. 2008). Yet, you interestingly found a different pattern, with similar movement rates for lobes and parent slopes, which needs to be discussed.

Response: We have added text to the Discussion that contrasts our findings against previous studies:

Previous studies have found that solifluction lobes tend to move faster than the surrounding soil (Harris 2008; Ballantyne 2013; Eichel et al., 2020). This contrasts with our results, in which soil transport rates are similar between lobes and surrounding hillslope locations (Figure 3(c) and Supplement). There are several possibilities to explain this discrepancy. First, although lobes likely move faster because they are thicker than the surrounding topography (Glade et al., 2021). welack constraints on presence or depth of permafrost and soil at survey sites over time, and it is possible that the difference in thickness between lobes and non-lobes is minimal, leading to similar transport rates. Second, it is well known that soil velocities vary substantially within a single solifluction lobe, with the highest velocities in the middle and near zero velocities at the front (e.g., Benedict 1970; Eichel 2020). This pattern is likely due to the dynamics of lobe movement, in which lobes roll over themselves at the front similarly to a fluid front flowing down a plane (Glade et al., 2021). Because we placed our targets at a variety of sub-lobe positions, it is possible that our data captures some of the within-lobe variability in deformation. (Lines 462-472)

Finally, vegetation is also important factor for solifluction lobe development and movement (e.g. Benedict, 1976, Eichel et al. 2017, Price, 1974). Does vegetation differ between lobes and parent slope and could vegetation play a role for observed movement patterns?

Response: Vegetation communities do not differ significantly between lobe target locations and nearby non-lobe target locations. Lobe fronts are often lined with shrubs, but we collect data from a range of lobe positions, and there is no systematic relationship between target location, vegetation, and displacement that we observe (see the new Supplemental figures that show lobe displacement vectors versus UAV imagery). We have added text to the Discussion:

Finally, vegetation is thought to be an important control on solifluction lobe deformation; different species prefer to grow on lobes, often concentrated at the front of the lobe where drainage is greatest (Price 1974; Benedict 1976; Eichel et al., 2017). While we did not conduct a comprehensive vegetation survey, all lobe fronts at the site have been colonized by shrubs, and it is possible that over time preferential plant colonization at lobe fronts has slowed down solifluction lobes relative to the surrounding topography. (Lines 472-476)

390: What exactly do you mean by "the tundra"- only above ground biomass or above and below ground (roots)? Please specify.

Response: We have added "the tears in the tundra **vegetation mat and associated belowground roots**" to clarify (Lines 562-563)

396-399: This section belongs to the methods.

Response: We have re-arranged the methods to contain section 3.4 Slope stability with a section on topographic controls on failures in addition to the infinite slope models. Text associated with the method can now be found in this section.

398: Could you shortly explain what a "logarithmic bin" is? This would be very helpful for readers unfamiliar with that term to understand the results in Fig. 7.

Response: We have clarified the utility of binning the drainage area data by powers of ten by emphasizing that this process elucidates whether the location is associated with hillslopes or channels:

To more clearly differentiate failure locations across different geomorophic regimes, we binned the total area of both the hillslope and failure polygons in logarithmic bins (i.e. powers of ten) for drainage area between 10^1-10^4 m^2 as well as slope bins between 7-25°. Casting failure locations in slope-area space assists in determining the dominant process regime (e.g. hillslope versus fluvial; Montgomery and Dietrich, 1994; Tucker and Bras, 1998) (Lines 269-273)

400-405: This section and related Fig. 7 and 8 belong to the results.

Response: We have also re-arranged the results section to include 4.3 Slope stability with corresponding text and figures, which have been re-numbered accordingly.

445: What exactly do you mean with "shear strength of the tundra mat"- shear strength of the above ground or below ground biomass (roots)? Please specify. Can you maybe provide references on the shear strength of tundra soils?

Response: We have clarified that the roots of tundra vegetation impart cohesion (strength) to a slope experiencing disturbance and have included references that study how root density and cohesion impact water velocities needed to cause erosion and slope failures:

These observations show that while slope failures and gully locations are controlled by topography, both the permeability of the tundra mat and **the cohesion imparted by its roots (Gyssels et al., 2005) are likely important mediators of rainfall infiltration and slope stability that complicate topographic-based predictions. Both subsurface flow rate and root cohesion are important elements of the force balance on slopes and require extensive field characterization (Parker et al., 2016; Hales, 2018).** (Lines 641-645)

465: "do" instead of "does"

Response: Edited.

497: "Areas" seems double?

Response: Revised first "areas" to "locations" for clarity.

Figure 9: Please describe where you got data for this figure in the methods and describe the figure in results, so you can discuss it here.

Response: We apologize for the confusion about these data sources. The meteorological data come from the Nome Airport historical records hosted on the Alaska State Climate Center's Alaska Climate Research Center's daily data portal (Alaska State Climate Center). The active layer thicknesses are from the CALM network (Brown et al., 2000; Nyland et al., 2021). We have updated citations and clarified in the text and the caption of Figure 9 that these data are associated with separate studies or site measurements.

We were motivated to visualize these data together in the context of our Discussion section-based hypotheses (i.e. it helps us answer "Are ongoing climate trends in the Arctic making the landscape more susceptible to erosion?" (Line 684)). Motivated by our results that many slope failures occurred after a large rainstorm in the late summer, we merely wanted to know if this sort of weather event was getting more common with time (it is not). The plotting of regional datasets does not in our opinion constitute methods or results.

Figures in text are sometimes labeled 1a (like in the figures), sometimes 1A. Please adjust.

Response: Revised throughout.

Figure 1: Different units for Easting and Northing? Please check if this is correct (also in other figures). In 1a red "1B" has a capital B, while label of the figure is (b). Unclear: is one square one lobe as indicated in the figure legend, or is a square a GPS survey target as indicated in the figure caption, with possibly multiple targets per lobe? Adding a field picture or orthophoto of the investigated lobes would be helpful. Scale bars would be helpful. Can you maybe delineate the small watersheds you studied? Maybe add an additional Figure 1(c) where lobes are depicted and all GPS survey markers are shown? This cannot be seen in Figure 1(b).

Response: We have used Northing and Easting (in meters) for UTM Zone 3N, which should indicate both scale and direction in all images, and we have formatted the values for these to appear consistent throughout the text. We have fixed the lettering issue. We have also clarified that each square is an individual target that is associated with lobe measurements. We hope that Figure 2, 3, and new Supplementary figures zoomed into lobe targets clarify the nature of these measurements. The small watershed has been highlighted. All GPS survey markers are shown in Figure 1(b).

Figure 2: Add scale bar and north arrows for orthophotos. Add location of b,c,d to a) as

well. 2c: Please mark the lobe here for the reader.

Response: We have added scale bars, north arrows, and clarified locations of b, d, and c (see previous comment).

We do not identify 2(c) as strictly a "lobe" but rather describe it as "detachment of the tundra mat from the underlying mineral soil in arcuate shapes." We have added a white dashed line to accentuate the arcuate shape.

Figure 3: A,b: Please mark lobes and add scale bars. Please add "survey markers" to lobes and "UAV" to targets in the caption and the Fig 3c,d to clarify what is actually shown (points measured on lobes, not lobes tracked by themselves).

Response: We are hesitant to delineate lobes in this image as lobate features appear compound and complex in this part of the landscape; we have added speculative lobe fronts according to shrub growth. We have used Northing and Easting (in meters), which should indicate both scale and direction in all images. We have added "target" to the map explanation to clarify.

Figure 5: I cannot see any cyan slope failures in 5a. Please check. Maybe it helps to zoom more into study area?

Response: We apologize for the mix-up in for this figure; the proper image has been included in this version.

Figure 7: Please correct "containting' in b and f

Response: Fixed.