The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2018-234-AC1, 2019

The Cryosphere Discussions

Interactive comment on "Rapid retreat of permafrost coastline observed with aerial drone photogrammetry" *by* Andrew M. Cunliffe et al.

Andrew M. Cunliffe et al.

a.cunliffe@exeter.ac.uk

Received and published: 2 March 2019

Rapid retreat of permafrost coastline observed with aerial drone photogrammetry - Response to Referees Referee #1 General comments The manuscript presents investigations of short-term coastal dynamics at a very rapidly retreating coastline using UAVs (drones) combined with data on long-term coastal dynamics of the same section according to satellite and aerial images. Although using multitemporal imagery analysis for coastal retreat measurements is common practice, and Herschel Island is a relatively well studied area in terms of coastal dynamics, the authors made the first attempt to provide very high temporal resolution observations of coastal erosion, including intra-seasonal dynamics presented by short-term periods (3-7 days during the summer of 2017). This is the principal novelty of the study, which gave new insights

into mechanisms and rate variability of coastal erosion and proved again its episodic nature, when a coastal segments can retreat by several meters in a few days during one storm. In this way, the investigated coastal segment gave a unique opportunity for such detailed analysis, as the rates of retreat in 2017 were unprecedented. Another strong point of the manuscript is the well described methodology, giving an example of using drones for coastal dynamics monitoring, which is already popular and will surely become one of the main tools in coastal investigations in the years to come. We would advise to reduce some general comments about the evident benefits of using drones and focus on giving more technical details that can be further used for elaboration of technologic standards (flight heights, required number of ground control markers, etc. - see in Specific comments below). Overall, the manuscript is a high quality study, with valid and appropriate methods, new trustful results supporting the discussion, fluent and precise language, well-readable figures and abundant supplementary material. The discussion can be re-grouped and some sections of it shortened (see below), however, this does not hinder the general good impression of the paper. Please also note the supplement to this comment: https://www.the-cryosphere-discuss.net/tc-2018-234/tc-2018-234-RC1-supplement.pdf

Author response: We thank Referee 1 for their very positive appraisal of our manuscript, and for their constructive suggestions. We have revised our manuscript in light of this feedback (responses to specific points of feedback provided below), and hope that the Referee will agree the manuscript is now greatly improved as a result.

Specific comments Abstract The abstract might be shortened, omitting information on the Kuvluraq – Simpson Point gravel spit, which is mentioned in the text shortly. The objectives can be shortened. The phrases: Lines 28-30 ("We found drone surveys analysed with image-based modelling yield fine-grain and accurately geolocated observations that are highly suitable to observe intra-seasonal erosion dynamics") and Lines 33 Page 1 - 2 Page 2 (We conclude that the data available from drones is an effective tool to understand better the mechanistic short-term controls on coastal erosion

dynamics and thus long-term coastline change, and has strong potential to support local management decisions regarding coastal settlements in rapidly changing Arctic landscapes") are somewhat repetitive, and one of them can be omitted

Author response: Thank you for these helpful suggestions, we have revised our abstract, shortening it by ca. 25%.

Introduction Page 2, Line 8 - "Coastal erosion is prevalent along the Western North American Arctic coastline and Eastern Siberia" - what about significant erosion rates in Western Siberia and in Western Russia along the Pechora Sea coasts? (Vasiliev et al., 2005, Kritsuk et al., 2014, Ogorodov et al., 2016, Novikova et al., 2018). Is there direct evidence that coastal erosion prevails over accumulation in the mentioned regions? Is the sum of erosional segments overall longer than the sum of accumulative segments? If not, would be better to rephrase, e.g., "rates of coastal erosion are considerable", or "the fastest coastal erosion was documented..." or "coastal erosion has high rates"

Author response: Thank you for highlighting this additional literature. We have refined the text here which now reads “Coastal erosion is prevalent along the Western North American Arctic coastlines, and all of Siberia, and is one of the major key processes degrading permafrost (Lantuit et al., 2012).” We have now read these papers, and where appropriate, have integrated them into our manuscript to strengthen the linkages with this body of knowledge on this topic. These papers show coastline segments characterized by erosion and accumulation with some coastlines characterized by a majority of accumulative segments (e.g. Novikova et al., 2018). Yet, the papers also showed that many of these segments have now become erosive over the past few years, reflecting a shift from accumulative to erosive coastlines also observed in the study region (Irrgang et al., 2018).

Methods Section 3.1. Page 4, Line 32 Artificial ground control markers were deployed along the shoreline and precisely geolocated to an absolute accuracy of centimetres using global navigation satellite system (GNSS) equipment (Leica Geosystems). If it

is possible it would be interesting to mention how the used number of markers was chosen, and how many markers are sufficient, depending on the study site characteristics?

Author response: We would recommend 13 ground control markers as the ideal number per photogrammetric reconstruction, which might cover a coastline reach of up to a few km in length. Ten markers would be used to constrain the bundle adjustment and three to evaluate the photogrammetric reconstruction (as recommended by James et al. 2017).

Page 6, Lines 19-20: Total shoreline uncertainties were calculated as the sum of georeferencing, pixel and digitising errors (Radosavljevic et al., 2016; Río and Gracia, 2013), and survey parameters and shoreline errors are given in Table 1. Why aren’t the total uncertainties calculated as the root mean square error (square root of the sum of the squares of independent errors)?

Author response: We used additive rather than quadratic error propagation because the pixel, georegistration, and digitising errors are not independent, with high pixel error (due to coarse resolution) resulting in higher registration and digitising errors (Table 1). Consequently, it is more appropriate to use the more conservative additive approach to error propogation (as also used by Radosavljevic et a;. 2016). In any case, there is minimal difference between the total uncertainties in either shoreline position or end point rates between additive or quadratic approaches to error propagation. We have added explanation for our choice here in the methods section.

Page 6, Lines 13-14 "Shoreline digitising errors were derived from the estimated accuracy of operator vegetation edge detection, informed by reference to finer grain aerial imagery" - not sure I understood well from this fragment how exactly the digitising errors were calculated. Was it by comparison of digitising by different operators? Why are they the same for all drone images from 2017?

Author response: The digitising error was estimated by an experienced operator, and

we believe that the estimated error terms are conservative in relation to the spatial resolution of the classified images (e.g. errors of 0.10 m when spatial grain is 0.02 m). We believe that using multi-operator comparisons to estimate digitisation error can sometimes be incomplete characterisation of error, as they do not evaluate against a true position and can fail to account for all operators making the same mistakes. We have revised the manuscript to explain this approach more clearly; it now reads "Shoreline digitising errors were estimated by the GIS operator, and ranged between 0.1 m and 4.0 m depending on image spatial resolution (Table 1)."

Page 7, Lines 8-9 "To inform qualitative interpretation of the erosion dynamics at this location, a time-lapse camera was installed at the location indicated on Figure 1 between 2017-07-29 and 2017-08-03." - this goes to section 3.1 (it can be called "Fieldwork and UAV image acquisition") or to section 3.2. Anyway, it's neither meteorological nor oceanographic data

Author response: We have moved this information as suggested by the reviewer (to section 2.1 in the revised manuscript).

Results After the drone surveys, DEMs were built, from which profiles are provided in Figure 4. Why are there no calculations of volumes of the material eroded in 2016-2017? Would be good to provide pictures in 3D. The authors faced some problems with the destroyed ground control markers; however, there could be some conclusions on the volume with smaller accuracy, and/or for the periods between surveys with good quality referencing only.

Author response: Following this constructive suggestion, we have extended our analysis of surface elevation changes to including estimates of volumes of erosion. We have updated the methods, results and discussion sections of the manuscript to reflect these changes.

Page 7, Lines 12-15. Are you speaking about average values of retreat for the 500-m coastal segment? What was the spatial variability of coastal erosion? If 14.5 $\pm$ 3.2

m was an average distance of retreat in 2017, were there locations with greater or smaller retreat, and what were the extremes? You are showing that coastal retreat was episodic in time, and saying it was also episodic in space - could you highlight examples in the text?

Author response: Yes, this text described average values across the 500 m segment. We have updated the text to include more description of the spatial variation in shoreline change, including the maximum (22 m) and minimum (6 m) retreat rates observed over this 40-day period.

Page 8, Lines 23-25 "A timelapse video illustrating the erosion at this coastline over five days from the location marked in Figure 1 is presented in video S1" - could you please describe here very briefly what exactly the video shows?

Author response: As recommended, we have expanded the text here to describe the contents of this video and added a camera symbol and viewing angle in Figure 1 (c).

Discussion The grouping of the Discussion is not always logical and needs to be revised. One of the suggestions is to move Section 5.1 to the end of the discussion. Otherwise, the introductory paragraph (page 8, Lines 27-31, Page 9 Lines 1-2) should be put after it. According to our opinion, Section 5.1 is too long and contains much obvious information that can be omitted without harm to the general content. Part of this is somewhat repetitive to the Introduction, other information can be moved to the Introduction. Lines 10-15 belong to other sections of the Discussion, e.g., Section 5.3.

Author response: Thank you for highlighting this area for improvement. We have extensively revised the structure and content of our discussion in line with these constructive recommendations. We now have four sections, entitled: 4.1. Rapid shoreline change, 4.2. Drivers of rapid shoreline change, 4.3. Rapid coastal erosion as potential threat for the Territorial Parks infrastructure, and 4.4. Using drones to quantify fine scale coastal erosion dynamics.

Page 10, Lines 9-10 "Fine spatial grain measurements from drone products are especially useful for isolating the drivers of coastal erosion events" - would be good to provide exact examples from the study site where you could isolate the drivers of separate coastal erosion events you are describing Section 5.2 There is no discussion on spatial variability of coastal erosion rates during short periods (e.g., 2017) and its reasons. Would be good to add it. Could you state precisely, what is the main short-term driver, according to your findings? Is it the wind speed? Might be a good idea to try to build a quantitative correlation between the wind speed and the erosion rates during the investigated period?

Author response: Thank you for this question of attribution. In this study we unfortunately do not have sufficient continuous ancillary observations of key parameters to robustly extend this analysis further (i.e. sea level, sea surface temperature, wave direction and energy). We know that wind direction matters for erosion rates at this locale, we expect a non-linear relationship between wind speed and erosion (as found by Vasiliev et al., 2005), and we know that there are latencies between erosion (especially undercutting) and shoreline change as observed from a nadir perspective. This complexity in process interactions is confounded by there being just six short-term periods of coastline change, ranging from 3 to 17 days duration. Consequently, we feel that further attribution analysis is outside the scope of this manuscript, but we strongly agree that it would be valuable for future work in this area to use these tools we demonstrate to examine this question of attribution in greater detail. We have revised the text of our discussion for greater clarity on this point.

Page 11, Lines 9-10. Is there any quantitative data on sea-level fluctuations during the observations?

Author response: Unfortunately, there are no quantitative observations of sea level fluctuations available at this location, and so consequently, we used '. . .appeared to be. . .' to indicate the qualitative nature of our observations. A tide gauge has now been installed since 2018 so we are hoping to record this parameter in the future.

Section 5.3. Would be good to provide some brief information on hydrometeorological conditions of the past years and discuss why 2017 was characterized by such dramatic retreat rates compared with previous years. You are speaking about the ice-free period increase, temperature growth, increased wave height, war water discharge, but all of these factors were already present in 2016, 2015, etc. - what is your opinion of why coastal erosion accelerated so much namely in 2017?

Author response: Thank you for this question. Unfortunately, while we were able to detect substantial changes over our observation period and discuss our findings in the context of broader regional hydrometeorological conditions, the limited hydrometeorological observation available at this specific location limits our ability to attribute these rapid changes in 2017 to specific drivers. We have looked at the start of the open water season, inferred from MODIS observations (Nasa WorldView, https://worldview.earthdata.nasa.gov/). This suggests that the sea ice may have moved out earlier than normal in the years immediately preceding 2017 which could have helped to condition this permafrost cliff (Figure R1 below, now added to the Supplementary Information). However, this inference is highly speculative. Although it would be possible to extract data on sea ice coverage (e.g. from the National Snow and Ice Data Centre) and meteorological conditions (e.g. from the Government of Canada's observations), these records are temporally patchy and do not encompass important parameters such as sea level (the first tide gauge in the area was temporarily installed in July-August 2018) and wave regimes (the first Acoustic current doppler profiler was temporarily installed in 2018 for a period of <1 month and the closest NOAA buoy is more than 200 km away, https://www.ndbc.noaa.gov/station_page.php?station=48021). We are highly doubtful that intensive analysis of available hydrometeorological data would result in an explanation of the rapid change observed in 2017.

The name of Section 5.4 does not match its content. This section describes coastal erosion at Herschel Island in the context of long-term erosion rates at different locations

around the Arctic, rather than short-term coastal erosion in the context of long-term observations

Author response: We have integrated this material into section ‘4.2. Drivers of rapid shoreline change’, and believe that the revised text is much more coherent.

Technical corrections Page 1 Line 31 change to " Over a single four-day period"

Author response: Correction implemented.

Line 32 " exceeded 1 _ 0.1 m d -1" - Please be consistent with number formats, and the number of decimals. If you previously reported the number of "2.2 _ 0.2 m a-1", you should provide this number as "1.0 _ 0.1 m d -1"

Author response: Correction implemented.

Page 2 Line 11 - and affect?

Author response: Correction implemented.

Line 20 - "improved understanding is required"

Author response: Correction implemented.

Page 3 Line 6 - "repeated drone surveys"

Author response: Correction implemented.

Lines 5-11. I would advice to use the present tense, rather than the past tense (e.g., "In this study, we use...")

Author response: Following scientific convention, we will continue to use past tense in this report of our results.

Lines 10-12 "We demonstrated that lightweight drones and aerial photogrammetry can be cost effective tools to capture short-term coastal erosion dynamics and related shoreline changes along discrete sections of permafrost coasts." - This goes to the

conclusions

Author response: Correction implemented.

Figure 1c - remove "Text" from the top right side of the map?

Author response: Correction implemented.

Line 17 - please add a reference for Figure 1a

Author response: Correction implemented.

Line 20 - "the mean annual air temperature is..."; "the mean annual precipitation is..."

Author response: Correction implemented.

Line 22 - "between 2000 and 2011" or "in 2000-2011"

Author response: Correction implemented.

Line 24 - delete "in this region"

Author response: Correction implemented.

Line 28 – northwesterly and easterly winds; "they exert..." "and with easterly winds facilitating the transport of warm water from the Mackenzie River to Qikiqtaruk Herschel Island" - unfinished phrase? Facilitate?

Author response: we have rewritten this paragraph for greater clarity.

Page 4 Line 1 - sea-level

Author response: Correction implemented.

Page 5 Line 14 - Processing parameters are reported...

Author response: Correction implemented.

Page 6 Line 20 " Río and Gracia, 2013), and survey parameters" -replace by " Río and

Gracia, 2013); survey parameters" "and survey parameters and shoreline errors are given in Table 1" - this reference goes to section 3.1 (regarding the survey parameters); the reference to Table 1 in the context of shoreline position errors is repetitive with Lines 15-17.

Author response: We have revised the manuscript to include reference to Table 1 in section 3.1, and remove unnecessary repetition.

Line 25 - delete "calculated"

Author response: Correction implemented.

Page 7 Line 5 – Figure 5 should be mentioned after the reference in the text to Figures 2, 3 and 4.

Author response: Correction implemented.

Line 12: "by a net total of 143.7 _ 28.4 m" - is it an average value for the whole segment?

Author response: Yes, sentence reworded for clarity.

Line 16 "shoreline retreat was 14.5 _ 3.2 m, an average rate of 36 cm per day." - replace by "the shoreline retreated by 14.5 _ 3.2, with an average rate of"

Author response: Correction implemented.

Line 17 - the shoreline positionS

Author response: Correction implemented.

Line 18 - meant that THE shorelines

Author response: Correction implemented.

Lines 19-20 "Coastline retreat was highly episodic in time and space, occurring primarily over two periods" - repetitive, replace by "Coastline retreat primarily occurred over

two periods"

Author response: Correction implemented.

Line 21: " There was minimal change in coastline position DURING SIX DAYS between August 5th and August 11th

Author response: Correction implemented.

Line 25: " a 13-month period"

Author response: Omitting the conventional hyphen here was deliberate to comply with The Cryosphere's house style ("is our house standard not to hyphenate modifiers containing abbreviated units"). We will continue to follow this house style, but are happy to defer to editorial preference on this.

Line 26: " in Figure 4, sampled across the A-B-transect indicated on Figure 3." - replace " in Figure 4; they were sampled across the A-B-transect indicated in Figure 3".

Author response: Correction implemented.

Page 8 Line 3 - from three to ten days

Author response: Correction implemented.

Line 4 -and their speed reached up to...

Author response: Correction implemented.

Line 5 "For zero to three days prior to the 1st 5 2017 survey (on 2017-07-06)" replace by " For zero to three days prior to the same survey"

Author response: Correction implemented.

Line 11 - of very strong winds

Author response: Correction implemented.

Line 16 - and facilitate further undercutting.

Author response: Correction implemented.

Line 18 – and the wind speed was low.

Author response: Correction implemented.

Lines 20-21 These meteorological conditions resulted in large waves and undercutting - sounds more logically.

Author response: Correction implemented.

Line 29 - where retreat rates typically range.

Author response: Correction implemented.

Line 30 - between 0 and 2 m

Author response: Correction implemented.

Page 9 Lines 13-14 - "In this case, however, for the total 17.4 m of shoreline retreat between 2016 and 2017 reported here to remain consistent with the long-term average of 2.2 m a -1 , no further erosion of this reach would need to occur for more than seven years" - rephrase: In this case, however, to remain consistent with the long-term average of 2.2 m a -1 , no further erosion of this reach would need to occur for more than seven years after the retreat of 17.4 m in 2016-2017.

Author response: Correction implemented.

Page 11 Lines 2-3 " Further factors facilitating rapid erosion at this coastal reach ARE the high ice content (ca. 40% Obu et al., 2016) and the low relief"

Author response: Correction implemented.

Line 10 -Although this region is microtidal - "although the studied region is microtidal"

Author response: Correction implemented.

Page 11, Lines 13-14 - "Winds exert substantial control over local sea levels, with north-westerly winds driving a positive storm surge and easterly winds driving a negative storm surge (Héquette et al., 1995; Héquette and Barnes, 1990)." - repetitive; already appeared in the Introduction

Author response: we have rewritten this paragraph to reduce repetition.

Page 12 Line 11 - on Bykovsky Peninsula?

Author response: Correction implemented.

Figure 2. What is the image at the background? Figures 2, 3 and 4. Would be better readable if you used different colours for coastlines of different time periods instead of shades of grey and black

Author response: As noted in the figure caption, the background image in Figure 2 is the 2917-07-06 image. We have experimented with a number of approaches the symbology of these shoreline positions, included various combinations of colours and patterns. Unfortunately, the proximity of the lines means that there is substantial over-potting when viewed at most scales. While we found that using colours did not materially improve the legibility of the shoreline positions, we do believe that the greyscale symbology provides sufficient information to readers in this context.

Referee #2 Cunliffe et al. present a case study for an eroding permafrost coastline along the Canadian Beaufort Sea Coast using historic photos, satellite images, and airborne images acquired from a UAV. The imagery ranged in spatial resolution from 3.5 m to 0.02 m and consisted of images acquired between 1952 and 2017 and focused on a 500 m segment of coastline. It appears that the historic imagery was already published previously (could be better clarified in the paper) and the novelty of this paper was the high temporal image acquisition using UAVs in 2017. Seven UAV surveys were used to create high spatial resolution orthophotos and digital surface models to assess coastal change rates on the order of days to weeks during July and August of 2017. The

paper is well written and organized. The study design and presentation of results are clear but need to be improved. In particular, the mismatch in image spatial resolution and temporal observations require further consideration in the paper. A number of suggested edits and revisions are provided below to help refine the paper to make it suitable for publication in The Cryosphere.

Author response: We thank Referee 2 for their positive appraisal of our manuscript and for their constructive suggestions. We have revised our manuscript in light of this feedback (responses to specific points of feedback provided below), and believe that our improvements address the points raised.

General Comments The comparison between decadal-scale erosion rates from images with a spatial resolution that ranged from 0.5 m to 3.5 m with coastal change positions determined from images with a spatial resolution of 0.02 to 0.04 m requires further validation . This is particularly important given the assertion that erosion rates in 2017 was 14.5 m/yr compared to a long-term average rate of 2.2 m/yr, or as stated in the abstract more than six times faster. The authors need to include a suitable image from 2017 or 2018 at a resolution that is more in line with image resolutions available historically to demonstrate that the increased resolution of the UAV imagery is not responsible for the measured increase in erosion, simply due to being able to better detect the feature of interest. Doing a quick survey of images available from DigitalGlobe shows that there are some potential options available for the study site in 2017 and 2018 that could provide this necessary check.

Author response: We agree that error estimation is an important consideration when working with different resolution data. However, we do believe that our comparison between images with different spatial resolutions is appropriate, as these differences in resolution are explicitly described by the 'pixel error' term (Table 1) and propagated through to both the uncertainty in shoreline position (Equation 1), and shoreline retreat rates (Equation 2; Table 2). This treatment of uncertainty in assimilating data sources of different quality is well established (e.g. Irrgang et al., 2018; Radosavljevic et al.,

2016; Río and Gracia, 2013). We found that change in shoreline position between the coarse and fine resolution images (between 2011-08-31 – 2016-07-27) was 0.7 $\pm$ 0.3 m a-1; so our comparison between these two periods indicates a slower than average rate of change. The very rapid changes in shoreline position we found were in comparisons between fine-grain drone-derived products, and were corroborated by our own field observations. We have added a sentence to our discussion clarifying this: "Our own qualitative observations on the ground over the summer of 2017 (Video S1) confirmed the extremely rapid shoreline changes described above." We do not think that purchasing and analysing additional more coarse-resolution recent imagery would be additionally informative in this instance.

On line 30 of the abstract the authors report that in 2017 mean coastal retreat was 14.5 m/yr. However, in table 2 it appears that there were only 40 days of erosion analyzed during this period. It appears that the 14.5 m of erosion refers to the magnitude of shoreline change and not an annual rate. This critical point needs to be better clarified and the mismatch in temporal periods among observation periods given more careful consideration . One consideration could be that the image acquired on 2016-07-27 be compared with the image acquired on 2017-08-15 to determine the most recent annual erosion rate instead of using the 2017-07-06 for this. Reporting it in this manner and then using the UAV image acquisitions within this latter annual-scale period to assess event driven erosion patterns and controls might make for a cleaner analysis and presentation of results.

Author response: Thank you for this feedback, we have revised our manuscript to further clarify that the very rapid erosion we report was measured over a period of just 40 days. In this study, we wanted to test the capabilities of UAV-derived observations to describe intra-seasonal change in shoreline positions, and relate these observations to longer term changes. To achieve this, it is necessary to compare time periods with different lengths, and we believe that we are explicit about this comparison (especially in Table 2). We do report the recent (near) annual rate for 2016-07-27 – 2017-07-06

(21 days less than a year) in Table 2; this omits some of the open water season and is therefore conservative if considered as an annual rate. The period 2016-07-27 – 2017-08-15 would be 19 days more than year, but the additional days are biased to the open water season, thus likely overestimating an annual rate. We have expanded our discussion to state: "Over a 384-day period from 27th July 2016 to 15th August 2017, we observed a large retreat in the shoreline position, with an average of 17.4 m, although note that this period is 19 days longer than a year and includes a disproportionate number of days from the open water season." This 17.4 m value is computed from summing 2.9 m + 14.5 m (the net shoreline change between 2016-07-27 – 2017-07-06, and between 2017-07-06 – 2017-08-15).

Considering that the historic remote sensing data was apparently previously published (is this what previously analysed refers to on line 23 page 5) the authors need to enhance their methodology and presentation of the imagery acquired with the UAV surveys. The authors should provide information on the altitude of the UAV during image acquisition, the orientation of the flight paths relative to the coastline, why they recommend using front lap and side of 10 and 20 respectively while only using 5 and 10 respectively, the number of ground control points established in the field, and why the authors did not constrain their orthophotos and digital surface models using ground check points when this method is recommended in the literature. All of this should be correctable and is not seen as a major sticking point. The authors are also encouraged to maximize the use of their UAV data by analyzing the digital surface models constructed in Agisoft. Currently this assessment consists of four sentences in the results section. The authors mention that erosion occurring after the fourth UAV survey prevented proper construction of digital surface models in the latter efforts. However, the digital surface model data acquired during the first surveys combined with the shoreline positions digitized from the latter time period orthophoto mosaics should provide sufficient information to add this element to the paper.

Author response: Thank you for this suggestion and constructive thoughts about how

to approach data collection and analyses of landscape change using drones. In our other work, we are strong advocates for the inclusion of ground control markers, sufficient overlap and appropriate methods to facilitate the best-possible 3D model construction (Cunliffe et al. 2016; Cunliffe and Anderson, 2019; Assmann et al. 2018, https://arcticdrones.org/). However, this particular data collection was opportunistic and not a part of our planned data collection. Thus, we were not able to collect data using our prefered method, though we still believe our results are robust given our data collection constraints. We have added additional information describing the UAV surveys, including the range of altitudes and the number of ground control markers uses to constrain each photogrammetric build in Table 1. The orientation of the flight paths varied between surveys, often due to weather (wind) constraints, but we do not believe that flight line orientation relative to the coast make a material difference to this photogrammetric approach. We recommended higher levels of overlap than we used because we wanted to help future users of this approach avoid making the mistake of insufficient overlap, which can have negative implications of image alignment and the geometric stability of photogrammetric reconstructions. Unfortunately, we did not have sufficient ground control markers to allow independent evaluation of the photogrammetric reconstructions. Our qualitative evaluations of the orthomosaic co-registrations indicated that the RMSE errors (Table 1) appeared to be conservative assessments of the registration error. We wanted to highlight best practice in this area, so that future studies would be able to improve upon our data collection. We have also extended our analysis of surface elevation change, measuring removal rates of ca. 0.79 m3 m2 d-1 of material (ca. 13,800 m3 over 500 m over 35 days), and have extended the methodology, results and discussion sections of the manuscript accordingly.

Specific Edits and Questions - Replace the use of grain with resolution throughout the paper

Author response: As requested, 'grain' has been replaced with 'resolution'.

- Consider changing the use of drone to UAV throughout the paper

Author response: Thank you for this suggestion. In line with the large and growing body of literature on drones in environmental science, we would prefer to continue using the term 'drone' in this manuscript as we feel this term is becoming more dominant in the literature as other terms such as UAV, UAS, RPAS are becoming less frequently used. We believe that our meaning of this term is clear from the text on page two; however, we are happy to defer to Editorial preference regarding this nomenclature in The Cryosphere.

- Equation 1 seems to be incomplete according to variables presented in Table 1 to determine shoreline change uncertainty. Check this.

Author response: The input parameters for Equation 1 (now Eq. 2) are present in Table 1, and we have revised the wording to increase clarity.

- Was the CTD data acquired in 2015 or during the study period in 2017. Check line 7 on page 7. If from 2015 how is it relevant to this study?

Author response: The CTD data reported was collected during 2017, and this typo has been corrected in the manuscript.

- Specify whether the time-lapse camera in operation for 4 days imaged the study coastline during the observation period.

Author response: The time-lapse camera was observing the study coastline, and we have amended the manuscript to make this more explicit.

- Change cm per day on line 16, page 7 to m per day

Author response: Unit change implemented.

- Please explain the significance of the linear regression method being more conservative than the end point method as reported on lines 23-25, page 12

Author response: We have revised this text, which now reads: "Erosion rates from linear regression tend to underestimate rates calculated from end point rates (Dolan

et al., 1991; Radosavljevic et al., 2016), which is consistent with our findings of 1.9 m a 1 versus 2.2 m a-1, but linear regression and end point rates alone do not account for uncertainty in shoreline positions (Himmelstoss et al., 2018). Changes in the rate of mean shoreline position for all time points are shown on Figure S4." Figure S4 is a new addition to the Supplementary Information, depicting the differences in average shoreline position over time and the linear regression rate.

- Adding field photos of the study coast would add useful information to the paper and provide a context for understanding the permafrost characteristics at the site.

Author response: We agree that photographs (and videos) can be extremely helpful in conveying useful information regarding research subjects. We included such additional information in the Supplementary Information (Figure S3 and Video S1). As these resources would be available with this manuscript, and photographs of this coastline have previously been published in Radosavljevic et al. (2016), we did not think that it would be necessary to include them in the body of the paper. Again, we are very happy to defer to editorial guidance on whether including additional photographs of this site in the manuscript itself would be helpful.

References cited: Assmann, J.J., Kerby, J.T., Cunliffe, A.M., Myers-Smith, I.H., 2018. Vegetation monitoring using multispectral sensors - best practices and lessons learned from high latitudes. Journal of Unmanned Vehicle Systems 334730. https://doi.org/10.1101/334730

Carrivick, J. L., Smith, M. W. and Quincey, D. J.: Structure from Motion in the Geosciences, John Wiley & Sons, Ltd, Chichester, UK., 2016.

Cunliffe, A., Anderson, K., 2019. Measuring Above-ground Biomass with Drone Photogrammetry: Data Collection Protocol. Protocol Exchange. https://doi.org/10.1038/protex.2018.134

Cunliffe, A.M., Brazier, R.E., Anderson, K., 2016. Ultra-fine grain landscape-

scale quantification of dryland vegetation structure with drone-acquired structure-from-motion photogrammetry. Remote Sensing of Environment 183, 129–143. https://doi.org/10.1016/j.rse.2016.05.019

Irrgang, A. M., Lantuit, H., Manson, G. K., Günther, F., Grosse, G. and Overduin, P. P.: Variability in Rates of Coastal Change Along the Yukon Coast, 1951 to 2015, Journal of Geophysical Research: Earth Surface, 123(4), 779–800, doi:10.1002/2017JF004326, 2018.

James, M.R., Robson, S., Smith, M.W., 2017. 3-D uncertainty-based topographic change detection with structure-from-motion photogrammetry: precision maps for ground control and directly georeferenced surveys. Earth Surf. Process. Landforms 42, 1769–1788. https://doi.org/10.1002/esp.4125

Kritsuk, L.N.; Dubrovin, V.A.; Yastreba, N.V. Some results of integrated study of the Kara Sea coastal dynamics in the Marre-Sale meteorological station area, with the use of GIS technologies. Earth's Cryosphere, 2014, 4, 59–69. http://www.izdatgeo.ru/pdf/earth_cryo/2014-4/52_eng.pdf

Novikova A., Belova N., Baranskaya A., Aleksyutina D., Maslakov A., Zelenin E., Shabanova N., and Ogorodov S., Dynamics of permafrost coasts of Baydaratskaya bay (Kara sea) based on multi-temporal remote sensing data, Remote Sensing 10 (2018), no. 1481.

Ogorodov, S.A., Baranskaya, A., Belova, N.G., Kamalov, A.M., Kuznetsov, D.E., Overduin, P., Shabanova, N.N., Vergun, A.P., 2016. Coastal Dynamics of the Pechora and Kara Seas Under Changing Climatic Conditions and Human Disturbances. Geography, Environment, Sustainability 9, 53–73. https://doi.org/10.15356/2071-9388_03v09_2016_04

Radosavljevic, B., Lantuit, H., Pollard, W., Overduin, P., Couture, N., Sachs, T., Helm, V. and Fritz, M.: Erosion and Flooding-Threats to Coastal Infrastructure in the Arctic:

A Case Study from Herschel Island, Yukon Territory, Canada, Estuaries Coasts, 39(4), 900–915, doi:10.1007/s12237-015-0046-0, 2016.

Río, L. D. and Gracia, F. J.: Error determination in the photogrammetric assessment of shoreline changes, Nat Hazards, 65(3), 2385–2397, doi:10.1007/s11069-012-0407-y, 2013.

Vasiliev, A.; Kanevskiy, M.; Cherkashov, G.; Vanshtein, B. Coastal dynamics at the Barents and Kara Sea key sites. Geo-Mar. Lett. 2005, 25, 110–120. https://link.springer.com/article/10.1007%2Fs00367-004-0192-z Ogorodov, S., Baranskaya,

---

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2018-234, 2018.

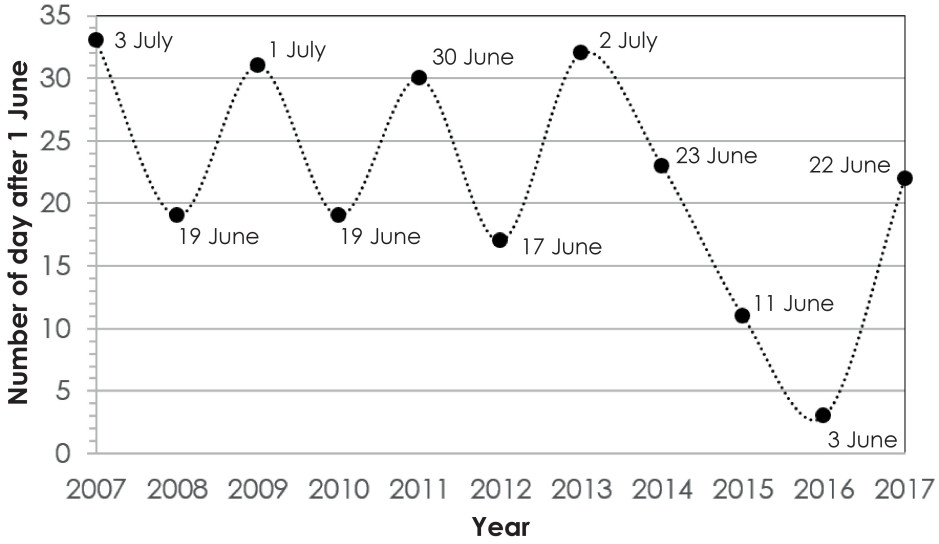
35
30
25
20
15
10
5
0
Number of day after 1 June
3 July
19 June
1 July
19 June
30 June
17 June
2 July
23 June
11 June
3 June
22 June
2007
2008
2009
2010
2011
2012
2013
2014
2015
2016
2017
Year

Fig. 1.