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# ***Interactive comment on “Observing Muostakh Island disappear: erosion of a ground-ice-rich coast in response to summer warming and sea ice reduction on the East Siberian shelf” by F. Günther et al.***

**F. Günther et al.**

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## **Authors’ reponse to reviews**

We thank Referee #2 for the thorough revision of our article and the constructive comments for further improvement.

## **Response to Anonymous Referee #2**

**Referee #2:** *This manuscript by Günther et al. presents observations of coastal retreat using historic and contemporary high resolution remotely sensed imagery for Muostakh*

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*Island, Laptev Sea from 1951 to 2012. Recent mean retreat rates of the coast at this small island are 1.7 times higher between 2010 and 2012 relative to mean retreat rates measured from 1951 to 2012. The authors compare current (2010 to 2012) patterns of annual and seasonal erosion to various environmental variables thought to impact erosion of arctic coasts and show that the increase in erosion at Muostakh is likely in response to increases in air temperature and increases in the open water duration period. This is an important manuscript that provides information on recent erosion rates for a site in the south central Laptev Sea. Currently few studies have reported on heightened erosion rates in the Arctic in response to increases in air temperature and open water duration.*

**Response:** Thank you.

*While the manuscript is generally well written, it could be better organized. The real strength of this paper lies in the image processing methods that the authors used to orthorectify and analyze the remote sensing data. However in the methods, results, and discussion sections the presentation of this data always comes after the presentation of the environmental and local parameter data. I suggest that the authors reorganize each of these sections to highlight first the methods and findings associated with the image processing and coastal retreat rates and secondarily the environmental and local parameter data.*

We reorganized the manuscript as suggested. In addition, we propose to include Geo-Eye data from July 2013, in order to complete three annual cycles from 2010 to 2013 for volumetric erosion analyses also for short-term.

*The connection to the variability in environmental and local parameters is a nice touch to this paper but it is definitely weaker than the detailed remote sensing change detection analysis.*

Indeed the focus on the reconstruction of coastlines and the relief situation at various times and we thank referee 2 for the words of appreciation. Nevertheless, it was nec-

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essary to include local and environmental parameters on a first order level, because quantification of sediment mass and even more substantial the calculation of seasonal erosion rates, would otherwise not have been possible.

*Further, it would be interesting if the authors provided a more focused discussion on the comparison of the historic and current DEMs for measuring subsidence. These methods and findings are not only very interesting at this site but have wide ranging applicability for permafrost terrain change in the circum-Arctic.*

We agree with referee #2 that this a current research question, which requires more detailed comparison of DEMs. In the meantime we have processed new GeoEye stereo images of Muostakh that have been acquired in 2013. We propose this dataset to be included in the manuscript, mainly because it fills a gap in the structure of the manuscript, which is the comparison of eroded volumes during the last three years, compared to the long-term volume losses. Parallel to this, the 2013 GeoEye along-track stereo imagery offers the possibility to use the high quality DEM extracted from this dataset for subsidence measurements, and our field survey data as independent reference for quality control of DEM differencing.

### **General comments and questions**

*The introduction could be shortened and better organized. This would help the flow and structure of the paper.*

We considerably shortened the introduction as suggested.

*Be consistent with the terminology used to describe the permafrost at Muostakh Island. In some cases it is referred to as yedoma, in other cases as Ice Complex or Late Pleistocene ice-rich permafrost.*

After introducing the term, we now consistently use Ice Complex

*The authors should explain in detail why they chose to use a different approach for determining erosion rate measurements in this study relative to their other 2013 study.*

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*Are the results from this study comparable to rates determined elsewhere using the transect method?*

Both approaches offer specific advantages over the other one. The main reason not to choose the transect method, as we did in our other study, is because of the very short time slices analyzed here. For example erosion observations over a couple of weeks showed quite unequally distributed erosion, which probably would not have been captured entirely using transects. Also, in places short-term erosion causes variations of a non-uniform cliff outline, across which a rectangular transect is difficult to apply. However, a control analysis reported in the other study revealed that both methods deliver comparable results. Even in this more local and detailed study we relate our measurements to a coastline increment of 50 m, which finally results in one single, but averaged, distance measurement every 50 m and ensures comparability to erosion rates determined elsewhere. Summarizing these main points, we now explain more clearly why the area based approach was preferred to transects.

*It is a bit unclear from the text and figures whether the erosion rates represent the average for the entire island area or if they just represent the average rate for those sections considered to be erosional. Please add text in the methods to clarify this.*

Although we also provided island-wide estimates of erosion, the reported rates represent the average rate for those sections observed to be erosional. This differentiation now comes earlier in the methods, as suggested.

*Analysis of the wind data is fairly cursory. The authors should strengthen this section or remove it.*

We further developed this section as suggested.

*Is there a particular wind direction that tends to elevate water levels so niches can form?*

The prevailing wind direction during the open water season is north, which is consistent

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with the direction where largest fetch is available. However, wind patterns have been different during the last three years and switched to prevailing southwestern winds. See also response to referee #1. The wind related discussion is now more condensed in one place.

*Why did the authors choose 24.5 m/s to represent effective storm events when several other studies that deal with arctic coastal erosion analyze events that range from 5-10 m/s? Atkinson (2005) considered 10 m/s as effective and Overeem et al (2011) considered 5 m/s as effective.*

We do not neglect these wind speeds to have an effective impact on coastal erosion. Strictly defined the term “storm” means winds measuring 9 on the Beaufort scale, which corresponds to wind speeds of 20.8 m/s. This differentiation is however only terminological. Wind directions and cumulative wind speeds during the open water season were analyzed without further wind speed distinction. Particularly regarding storm climatology in the region we refer the reader to Lantuit et al. 2011.

*Do a spell check throughout the manuscript and fix incorrectly spelled words.*

Done.

### **Detailed questions**

*Page 4102, line 12: does coastline retreat here refer to TA or TD or both?*

Long-term coastline retreat was only measured as TA. This is explained now in the manuscript.

*Page 4109, section 3.1.1: why only analyze the SSM/I data from 1992 to 2012? The dataset extends back to 1979.*

We analyzed analyze the SSM/I data from 1992 to 2012, because higher resolution of 12.5 km is available from 1992 on. We filled the data gap 1997 and rewrote the section.

*Page 4110, line 11: what is meant by synchronous T-air data from 1999 until present?*

Indeed this is unfortunately worded. We had two different datasets, one with four measurements per day, one with eight every three hours. Using this synchronous time period, we evaluated differences in mean daily temperature using four or eight measurements, which turned out to be neglectible. Data from the Tiksi climate station is now described in a sufficiently comprehensive manner. We also included Ivanov et al. 2009 (Long-term variability of climate characteristics in the area of Tiksi hydrometeorological observatory, Problemy Arktiki and Antarktiki, 2009) as reference.

*how were the elevation data adjusted to sea level?*

The tacheometric survey was conducted over one week in August 2011. The height base value in local project coordinates was set to 100 m on top of the island. Subsequent measurements of the waterline were then used to adjust the point cloud to the factual sea level. We included a reference to the respective expedition report, which has been published recently.

*Page 4127, section 4.4.2: please state the dates for the erosion periods in the text in this section*

Done.

*Page 4129, section 5.1: should it be "changes in" instead of "changes of"?*

Done.

*Page 4130, line 9: I don't think that the data presented in Overeem et al (2011) dealt with centuries. . .*

This is correct, we removed this part of the sentence.

*Page 4132, section 5.2.1: please discuss the hourglass shape further. Why are these syngenetic ice wedges configured in this manner?*

Assuming homogenous ground conditions, syngenetic ice wedge width is likely related to the sedimentation rate. Stable surfaces provide favorable conditions for horizon-

tal ice-wedge growth, while quickly aggrading surfaces lead probably to narrower ice wedges. In contrast to the continuous Ice Complex accumulation on Bykovsky Peninsula, Schirrmeister et al. (2011) show a break in radiocarbon ages between 8 to 10 m a.s.l. on Muostakh, where ice-wedge width narrows. Therefore, erosional unconformities could also play a role in this context. We also included a photograph showing the ice-wedge geometry.

*Page 4134, lines 4-6: please expand on this topic. This comes out of nowhere.*

Yes, this was an interesting observation. We included a photograph of these initial baydzharakhs in the island's interior.

*Page 4136, section 5.3: since this is the discussion section the authors should add a short section on the ramifications of the disappearing islands in the Laptev Sea shelf. Will erosion on the mainland be enhanced? Will larger and more hazardous storms develop in the Gulf? Etc. . .*

We thank the referee for this interesting suggestion. We added the assumption: Not unlikely the disappearance of Muostakh would reduce the protection of the Tiksi harbor.

*Figure 2: what is the width of the island in the left frame? Please add this to the figure.*

We added the island width as suggested.

*Figure 4: what is meant by superelevation?*

We changed "superelevation" to "elevation values were converted by a factor of 5".

*Figure 6: please add the outline of the island to this figure*

We removed figure 6 for reasons of manuscript lengths.

*Figure 13: what is meant by, note the seasonal shift of summer air temperatures and open water period? Relative to what?*

Relative to each other. We further explain this observation.

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*Figure 15: this graph is somewhat confusing since there are three different bar colors.*

The semi-transparency of the lighter bars allows to directly compare thermo-denudation vs. thermo-abrasion in one graph. Overlapping value domains cause a mix of a pseudo third color. In the figure caption we now point to this fact. The alternative is to provide two different histograms, which then would be more difficult to compare.

*Figure 20: how does this figure relate to this study? I don't remember seeing it referenced in the text.*

The figure was referenced on page 4137 in line 2. The figure was removed.

All detailed edit and typo comments made by anonymous referee #2 were implemented in the revised manuscript, following the suggestions.

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Interactive comment on The Cryosphere Discuss., 7, 4101, 2013.

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