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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.

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Authors' reponse to reviews

We thank Referee #1 for the willingness to carefully read our article and for providing specific information and to give advice on how we can further improve our manuscript.

Response to Anonymous Referee #1

Referee: *This paper analyzes seasonal thermo-erosion dynamics for a permafrost coast in the central Laptev Sea, Russian Federation. It documents the evolution of*

the small Muostakh Island, which is characterized by ice-rich permafrost and has spectacular erosional features; icy cones, called baydzharakh fields. Analysis uses high-resolution satellite imagery (2010-2012), geodetic field surveys and historical aerial photographs (1951) to quantify longterm coastal change. Thorough comparison of aerial photos of the 1950's and satellite imagery in the period 2010-2012 shows longterm land loss of 0.9 km^2 (24% of the island disappeared in the 60 years). In addition, analysis of stereo-reconstructions show the island subsided 1.2 m due to melt of excess ice and subsequent drainage of meltwater over this period. The reconstructed rates of thermo-abrasion and thermo-denudation for the warm recent period of 2010-2012 were much faster: average coastal erosion from 1951 to 2012 was 1.8 m/a; recent rates were 1.7 times more rapid at 3.1 m/a. Using time series of local sea ice concentration and air temperatures at nearby Tiksi, the authors apply normalization to coastal retreat observations over seasonal and inter-annual periods to identify their seasonal intensity and to discuss environmental controls on processes involved in coastal thermo-erosion development.

Response: Thank you for summarizing the main aspects of our study, which gives us feedback about the reader's impression and expectations.

Arctic coastal erosion is one of the effects of a rapidly warming Arctic climate. This study provides a well-documented example of Arctic coastal erosion on a small island in Laptev Sea, Russian Federation. The study use particularly thorough reconstruction techniques for assessment of erosion rates and, even more novel, subsidence. Erosion rates were much faster over the last 3 years, as compared to the 60-year background rates. These important results corroborate other studies in the Arctic region, particularly the Beaufort Sea, along similarly ice-rich coasts. It is important to assess and quantify the geomorphic changes in different regions and this paper is a great contribution towards our mapping of the changing Arctic coast.

Thank you for emphasizing our observation of recently accelerated erosion and for commenting on the significance of arctic coastal erosion studies. In this overall evaluation we agree with the referee #1 that regional or even local coastal erosion studies contribute to our understanding of this arctic-wide phenomenon by taking into consideration specific permafrost conditions.

To then relate the erosion rates to controlling factors is a logical next step; but this paper lacks sophistication in quantifying the controls on erosional processes. The environmental parameters that control erosion are much simplified and the analysis weakens the strong reconstructions of observed erosion rates.

Indeed we aimed not only to reconstruct coastal erosion rates for several time periods, but also to explain the temporal evolution of the observed variations in erosion intensity. Particularly for this reason we selected Muostakh Island as study site, because only there (with the exception of the adjacent Bykovsky Peninsula) eroding sea coasts of the Laptev Sea are located not far from a functioning hydrometeorological observatory (i.e. Tiksi). We agree that the controls are simplified, but there are many reasons for that. We tried to make it as simple as possible but not simpler. Firstly, we think that the calculation of thermo-denudation and thermo-abrasion rates for periods shorter than one year or not equal to complete annual cycles (and for subsequent comparison with other rates), is only possible if based on air temperature and sea ice data. Of course this approach might deviate from the realistically occurring thermo-erosion behavior but is considered sufficiently close as an initial approximation. Secondly, this approach can be easily applied by other researchers, and this is very important. To our knowledge, the literature lacks any proposed strategy on how to compare erosion periods of unequal length, since with the increased availability of very high resolution satellite imagery and under consideration of the region's remoteness, such time series will remain the rule rather than the exception. Therefore, reliable reconstruction of rates goes hand in hand with plausibility of environmental parameters, which determine season length.

I also think that not much new insights are provided by the analysis of the controls.

We agree with referee #1 insofar as we did not discover new controls which have not

been proposed as major influences before. However, to our best knowledge, for ice-rich permafrost along the vast East Siberian coastline this has not been consistently demonstrated in the literature. We do feel that the quantitative correlation of positive air temperature and erosion rate is a new contribution that should be tested elsewhere.

I suggest that this section needs either major strengthening, or needs to be shortened considerably, with a focus on the reconstructions and erosion rate data. A few suggestions to strengthen the analysis of the controls: 1) To use 'positive degree days' as the control on thermo-denudation is a (likely justified) oversimplification. However, it neglects the variability of temperature, T-air, over the season, and furthermore neglects the influence of clouds on the actual incoming heat.

Because "'thawing degree days"' are a first-order index for the seasonal variability of temperature, we identified T-air as the control on thermo-denudation. In fact, the influence of clouds on the incoming heat is significant. Langer et al. (2011) found that reduced solar radiation during cloudy conditions is partly compensated by increased long-wave radiation. With respect to the energy balance of polygonal tundra, Langer et al. (2011) conclude that, surface temperature is reduced during cloudy days in summer months, whereas cloud cover delays the refreezing in fall, as cloudy skies reduce long wave losses. However, we would not go so far as to say that measured air temperature neglects cloud cover. With respect to the heat balance of the Earth, one is closely related to the other through back radiation.

Since the daily T data is available, I suggest that an improved metric can be formulated that uses all sea-ice free season daily temperatures more specifically.

Thank you for this suggestion. As it turned out, we cannot use only daily temperatures for the sea ice free season, because this would exclude important phases of thermo-erosional activity. For example, already in May the coastal hinterland is free of snow and we observe thermo-denudation rather due to strong solar radiation than air temperatures on coastal cliffs to start in spring when the sea is still covered with ice, as evident from small alluvial fans on top of snow at the cliff bottom. Referee #1 is correct in that, the coastal thermo-erosion activity is most intense when the sea is free of ice and T-air is well above the freezing point. This is also one of the main results of our study, which we highlight now more explicitly in the last paragraph of the conclusions section.

Even a simple integration under the daily temperature curve would already be a more powerful simple metric.

This suggestion has already been implemented. We calculated thawing degree days as the integral of mean daily positive temperature over certain observation periods as a cumulative measure in order to perform normalization of erosion rates. These normalization efforts showed a harmonization of originally very diverse rates (Fig. 18, purple graph). We tested several methods to calculate thawing degree days, e.g. based only on min max temperatures using the sine method, but finally used mean positive daily temperature.

A more sophisticated direct melt model for TD with the actual observed T-air for the period 2010–2012 could be formulated potentially.

We thank referee #1 for seeing the potential of our data to contribute to a model for thermo-denudation. However, the development and test of a model is beyond the scope of the paper.

Make sure to show how far is Tiksi from Muostakh Island? Can you justify that this meteorological station is representative for Muostakh Island?

This an obvious and legitimate question, which we have now answered in a clearer manner. A weather station operated on Muostakh only until 1991, which is why we had to rely on data of the climate station "'Polyarka"' near Tiksi. Continuous weather records for Muostakh and Tiksi exist parallel for 15 years from 1970 to 1990. Average annual air temperatures during this period in Tiksi were -13.1C, whereas on Muostakh

-12.9C were observed. Generally, summer temperatures on Muostakh strictly followed those observed in Tiksi and a correlation of R^2=0.97 was found for the overlap of both records. A more "'oceanic"' climate on Muostakh seems to damp temperature amplitudes slightly. Boike et al. (2008) show monthly average air temperature from 1998 to 2005 for Samoylov Island in the Lena Delta and compared these records with Tiksi. Although the distance between both stations is about 110 km, they found an almost identical annual temperature cycle. However, the distance between Muostakh and the climate station near Tiksi is 39 km only and we therefore expect the representativeness of Tiksi for Muostakh is given.

Probably the major missing process is that thermo-abrasion and cliff-niching only happens when the ocean water is set up against the shoreface by wind-driven waves.

Regarding thermo-niche formation, we referred to the high temporal resolution observation of Wobus et al. (2011), who find that thermo-niche or "'notch"' formation and progression of erosion is observed even when sea water is not in contact with the bluff face. However, we agree with referee #1 insofar that the number of observation times for thermo-abrasion is limited, which is caused by the restrictions in image interpretation, such as snow accumulation at the cliff bottom, as described in the manuscript. Additionally the long-term thermo-abrasion rate (1951–2012) could not be related to observations of sea-ice concentration and the length of the open water period, due to a lack of such data. Currently we see few possibilities to implement a monitoring of thermo-niche formation, because the high coastal cliff of 20 m and the straight coastline does not allow on-site installation of a measurement setup.

Thermo-abrasion will not be a direct function of just the wind-speed, but it needs to include the wind-direction, and is further controlled by fetch and local bathymetry. At least a directional analysis needs to be added that shows what wind directions are important and then the Implicitly the authors assume that wind climate has not changed over the last 5 decades. Is this a reasonable assumption? Are there any trends in the wind data from 1999 – 2012.

Following the suggestion made by referee #1, we extended the time period of wind data analysis to the entire 1951-2012 observation period and added a directional analysis. Wind speed and prevailing wind direction are now shown as wind charts separately for a generalized sea ice free period of three months, lasting from 15 July to 15 October. Indeed considerable differences were observed in the relative frequency of wind directions between the two major observation periods 1951-2012 and 2010-2012. Northern wind directions prevailed during the open water season over the past 61 years. Quite unexpectedly, during the last three years winds from west and southwest were dominating. Particularly the increasing frequency of southwesterly winds is of notice, because this wind direction brings the warmest air masses from the inner continent, which we also show as rose diagram. Changes in wind regime might therefore be also responsible for increasing thawing degree days.

Why start the sea-ice SSMI-analysis in 1992? Daily or two-daily data is available from 1979 onwards from the National Snow and Ice Data Center (NSIDC). This longerterm dataset seems much more appropriate considering the long comparison of the aerial photos and satellite imagery? To start the analysis in 1992 seems completely arbitrary considering the time-span of the imagery.

Indeed sea ice concentration data is available from 1979 on with 25 km resolution at NSIDC. We reprocessed the SSMI data at a higher resolution of 12.5 km, available at Ifremer/CERSAT. This is also the reason why our concentration time series starts in 1992, because the high frequency channels enable to have a 12.5 km grid resolution did not work before 1992. We rewrote the sea ice section accordingly and filled the data gap in 1997.

What is Ice Complex type? Use Pleistoce Ice Rich Deposits to keep it more general.

Ice Complex is an adopted term from the Russian quaternary geology. In conjunction with the often synonymously used term yedoma, these deposits attract strong interest through their vulnerability to disturbance and potential carbon release. We therefore

prefer to be precise in this regard. The references Schirrmeister et al. 2012 and 2013, in particular, provide a review of Ice Complex related literature.

State what length of coastal stretch has been studied in the abstract

Done.

Use 'positive degree days' instead of 'thawing degree days'

In fact the temperature threshold of both indices is the same. However, "'positive degree days"' is a common term in glaciology, whereas "'thawing degree days"' is often used in connection with permafrost. To be consistent with the existing permafrost literature and because permafrost thaws and not melts, as glacier ice does, we prefer "'thawing degree days"'.

Not sure what vertical hourglass distribution means. Maybe use 'non-uniform' or anisotropic to keep the statement more general.

We included a photograph of an outcrop, which shows the ice-wedge geometry with wide ice wedges in the lower and upper part, and more narrow ice-wedges in middle part.

Specify quantitatively what is meant by 'cyclicity'.

Cyclicity means the existence of a thermo-erosional cycle, during which rates of either thermo-denudation or abrasion are overtaken by the respective opposite process. The concept of succession and duration of both processes is now more clearly described.

Nice distinction: Thermo-denudation TD, thermo-abrasion TA.

Thank you.

Introduction Line 7: add Lantuit et al. to the references.

Done.

Interesting, Laptev coast has doubled, just like the Beaufort Coast (Mars & House-

knecht, 2007).

Yes.

Line 26: This is all fairly speculative; the discharge of the Lena has increased, but freshening (and decreasing salinity) has a positive feedback on sea ice stability.

We removed the sentence.

Study area: Line 20: sea level regressed Line 17/ Fig3: caption. Use 'dominating' instead of surpassing.

Done.

Data and Methods *set-up 3.4 Caption; typo in analyzing 3.4.*

Done.

Please list the available imagery with source, date and resolution in a table, and make clear how long the time series is and at what intervals you have data coverage. This information now comes relatively late in the paper and is not very organized.

The table listing the available modern imagery now comes earlier in the manuscript. We decided not to include an additional column for the time intervals. This is obvious from the acquisition date. However, red lines in Fig. 13 show very clearly the interval lengths relative to each other, and even more important, the proportions of temperature and open water period.

4.3.Line 15. Typo in 'distribution'

Done.

Discussion: *The comparison discussion with the reported Sea Ice Free days of Markus et al. is not necessary; you do not compare apples to apples. Markus et al. use the entire Laptev Sea as their reference region, you are 'zoomed in' to the section around Muostakh Island. Omit this section.*

Done.

P4129. Overeem shows: : :. Omit the part of the statement 'over a century'.

Done.

P4131. Please include wind-driven setup into your discussion about storms and wind direction. What wind direction sets up the water highest around Muostakh? That maybe even more relevant than the fetch!

Of course this is a reasonable expectation. The situation is as follows: unlimited fetch is present to the north and northeast. As mentioned above, over the past 61 years, this is also the major wind direction during summer. Considering the Buor Khaya Gulf is enclosed to the south, northern winds should set up the water highest around Muostakh. Therefore, fetch and wind direction complement each other. If currently prevailing winds from southwest will remain persistent during summer, we expect also increased erosion along the east facing coast in the future. However, historical and modern rates recently do not differ greatly there, and are generally very low. These aspects have been described already at least in part, but widely spread in the manuscript. For clarity, we condensed this discussion in one place.

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