

## ***Interactive comment on “Short and long-term thermo-erosion of ice-rich permafrost coasts in the Laptev Sea region” by F. Günther et al.***

**F. Günther et al.**

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### **Authors' response to Anonymous Referee № 1**

#### **Anonymous Referee № 1 General comments:**

*Referee: Authors of the paper study very serious and urgent problem – a retreat of arctic ice rich coast permafrost under both a sea and atmospheric energy influence and its vary depending on geomorphology conditions. Undoubtedly authors show the representative material of a long time and wide region investigation as well as good enough argued conclusions concerning a development of thermodenudation and thermoerosion of icy frozen ground on shore of East Siberia. So the paper examine a significant question of arctic nature, base on extensive data and come to ascertained*

C1892

*results.*

**Response:** We thank the Referee for the expression of recognition of our work. We are very pleased that the Referee shares the author's view of the importance to study arctic ice-rich permafrost coasts.

### **Authors' response to Anonymous Referee № 2**

#### **Anonymous Referee № 2 General comments:**

*Referee: The manuscript summarizes a study examining the relative contributions of erosion near the foot and upper portions of permafrost-laden cliffs in the Laptev Sea region. Erosion rates at the lower sections of the cliffs are presumed to be related to mechanical abrasion by wave action and thawing (TA), while erosion rates at the upper sections are presumed to be related to solar insolation and heat advection causing thermal denudation (TD). The study is thorough, well credits prior work, and is well presented. With respect to the subject matter and scientific objective, I am excited to see this type of study as I see a strong need for this type of information and find it particularly relevant to assessing future rates of change. Based on limited research, am not aware of much literature addressing this topic.*

*The methodology is well described, including quantification of uncertainty in the recession rates, and appears to be repeatable. Equations are clearly presented and easy to follow. In summary, it is a very good paper and I fully recommend it be published, with some edits as outlined below.*

**Response:** We thank the Referee for the detailed report of the manuscript's aspects. We are very pleased that the manuscript has attracted great interest.

#### **Anonymous Referee № 2 Specific comments:**

*The manuscript text is a bit long; the results and discussion sections cover too many topics.*

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The results and discussion section have been shortened substantially as requested.

*It is stated in the beginning of the manuscript that the overall objective is to assess the dependence of cliff recession (I prefer the use of the term recession for linear rates and erosion for overall volumetric changes) on geomorphology and relative contributions of water levels and atmospheric drivers.*

We now use the term erosion for cliff retreat uniformly throughout the manuscript, as the term primarily used in the literature on permafrost coastal erosion and as comprehensive term for thermo-denudation and abrasion, according to the definition (see below).

*This is a great and very important question that needs to be answered and although providing estimates of carbon flux based on the estimated erosion volumes is also of great consequence, it distracts from the main aim of the paper and could be presented in a separate follow-up article.*

We thank the Referee for underlining the importance of estimating total organic carbon (TOC) fluxes in the arctic coastal zone and highlighting it as an applied research component of the manuscript. We also believe that this small section should remain in the manuscript for two major reasons. Primarily, we believe that the quantification of TOC fluxes from permafrost coastal erosion are of special interest to the readership of the journal Biogeosciences. Further, we separately assess upper and lower coastline recession over extensive coastline segments. This approach allows calculation of erosion volumes in more detail, which has been lacking from Arctic permafrost coastal erosion assessments so far to our best knowledge and for which we strongly advocate in this regard.

*Substantial conclusions are reached and the scientific methods and assumptions are valid and clearly outlined for the most part. It is mentioned that the biggest uncertainty with the NDTI approach is lack of accounting for the thermal niche at the bottom of the cliffs. Additionally, how was the base of the bluff deciphered from failed material*

C1894

*residing at the base?*

This is an important point, because this is a source of additional subjective uncertainty. In fact, shadow effects are sometimes more challenging, but generally we see the material is removed away without long residence on the cliff bottom. We included two sentences into the coastline digitization subsection, describing how we have handled this.

*A minor semantics concern is that the term "TA" is somewhat deceptive. It is defined as erosion from the combined processes of mechanical wave action and thawing due to thermal gradients – maybe TMA (Thermal and Mechanical Abrasion) would represent the physics better?*

Indeed there is a little discrepancy regarding the terminology in the literature. The term thermo-abrasion was introduced by Ermolaev (1932). Are (1978) clarified its meaning by separating the influence of atmospheric heat and solar radiation into the term "thermal erosion", while thermo-abrasion means "disintegration of the shore zone under the influence of the mechanical and thermal energy of moving water". The last and widely accepted (in Russian geocryology) specification was given by Are (1980, 1988), in which the term "thermal erosion" served only as heading for the two processes "thermo-abrasion" and "thermo-denudation", where the latter took over the former definition of thermal erosion. However, unfortunately the Multi-language glossary of permafrost and related ground-ice terms (van Everdingen, 2005) only lists the term "thermal erosion", focusing only on the aspect of hydrologic transport mainly due to fluvial and not coastal thermo-erosion. Therefore, since even the common definition of thermo-abrasion is not included in such a comprehensive glossary, it seems inappropriate to introduce another new term as supposed by the Referee, although that could provide some clarity. We added the important international contribution of Are (1978) as reference.

*There are several points of discussion in the text to the fact that the short term erosion*

C1895

rates are greater than the long-term erosion rates. Somewhere it should be noted that cliff failures are episodic, and that under such a short time period as the 'recent past' that is assessed in the study, it may be the cumulative effects of higher air and water temperatures and/or greater storm energy that caused the cliff failures to occur. Because of the episodic nature of the failures, much erosion might occur in a given year while during prior and following years little erosion might take place. It is recommended that a discussion regarding the cliff failure modes be included in the interpretation of the findings. The fact that TA and TD rates of erosion are very similar for alas coasts but not for the higher yedoma coasts is likely a direct reflection of cliff height (in addition to lithology and forcing mechanisms). Even in lower latitudes, it is quite common for high bluffs to erode via slides or retrogressive failures (Fig. 2 is a classic example of retrogressive failures).

We further developed the discussion on the episodic nature of cliff failures as suggested and concentrated several existing discussion elements of this topic now into one paragraph within the discussion section on "Recent coastal thermo-erosion rates" for better clarity in a contextual relationship.

**Anonymous Referee № 2 Technical Corrections:**

*Pg. 2706, lines 9-12: run-on sentence. Suggest removing the last prepositional phrase 'to coastal thermo-erosion over the past 4 decades : : :'*

The sentence was changed as suggested.

*Pg. 2706, line 10: suggest changing waterline to waterlevel.*

Done.

*Pg. 2706 line 14: suggest changing to "... ranging in length from 73 to 95 km and analyzed: : : :"*

The sentence was changed as suggested.

C1896

*Pg. 2706 line16: change "that" to "to"*

Done.

*Pg 2707 line 17: change "The enhanced " to "Increased"*

Done.

*Pg 2707 line 24: change "an" to "a"*

Done.

*Pg 2711 line 2: add "and" after " : : : tundra, "*

Done.

*Pg 2711 line 15: change to " : : : development directed toward the coast."*

Done.

*Pg 2711 line 25: change to " : : : drainages occur : : :"*

Done.

*Pg 2713 line 17: typo "vincity"*

Done.

*Pg 2714 line 22: " : : : rely: : : "*

Done.

*Pg 2715 line 5: add comma " : : : that is, : : : "*

Done.

*Pg 2718 Section 3.4 header: suggest changing digitilazation to "digitization" (do global change in document)*

Done.

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*Pg 2721 lines 21-23: Suggest rewording to something like: : : "The frequency of erosion rates between -0.5 and 3.5 m/yr are nearly consistent ( 0.03*

Done.

*Pg 2722 line 12: mean is 0.91 for all 3coasts. The sentence suggests that all three coasts are referred to.*

The sentence was split in two shorter sentences for clarity.

**Additional references included in the revised manuscript:**

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Interactive comment on *Biogeosciences Discuss.*, 10, 2705, 2013.

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