The comments by Reviewer #3, Anatoly Sinitsyn, are in font color black. The authors' responses are in green. The changes to the revised manuscript are in blue. The line numbers refer to the section and line numbers in the tracked changes version of the revised manuscript after addressing the comments from Reviewer 3.

Geosci. Model Dev. Discuss., referee comment RC3
https://doi.org/10.5194/gmd-2021-28-RC3, 2021

Comment on gmd-2021-28
Anatoly Sinitsyn (Referee)

Referee comment on "ArcticBeach v1.0: A physics-based parameterization of pan-Arctic coastline erosion" by Rebecca Rolph et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-28-RC3, 2021

Anatoly Sinitsyn,
June 27 2021, Trondheim

Review of the article "ArcticBeach v1.0: A physics-based parametrization of pan-Arctic coastal erosion" by Rolph et al. (2021)

The article presents a model for estimation of coastal dynamics at permafrost, ice-rich coastlines. More specifically, erosion rates at coastal bluff and beach are handled by the model. The model utilizes 1-D coastline erosion model of Kobayashi et al. (1999), bathystrophic storm surge model of Freeman et al. (1957), and empirical equations of Kriebel and Dean (1985) for estimating cross-shore sediment transport. The model is forced by historic hydrometeorological data (wind speed and sea ice concentration), and initialized by existing bathymetry of the case study locations. The model is validated by observed water level data. Sensitivity of the modelled retreat rates is accessed with the Monte Carlo approach. Modelled retreat rates are compared with observed rates for evaluation of the model performance. It was found that the water level plays critical role in defining retreat rates. The results demonstrate that the model is capable to reproduce retreat rates withing the same order of magnitude as the observed retreat rates. This is promising result justifying the model performance, and possibilities of application for crude assessments of coastal dynamics in relevant coastal settings.

The model developed by the authors looks definitely useful for the field of arctic coastal dynamics, and shall be considered as a very good step forward.

I have several, largely suggestive comments, which are presented in the attached file. Intension of these comments is to clarify some points in the text of the article and make it more suitable for engineering community, who is not necessary dealing with permafrost coastlines on a daily basis.

We sincerely thank Anatoly Sinitsyn for taking the time to review our manuscript and greatly appreciate his useful comments. Our responses are below.

Main point of my comments are the following:

- Despite the title, the model is aiming to handle some, but not all, of the morphologies comprising pan-Arctic coastlines, i.e. ice-rich coastal bluffs/coasts. This limitation could be mentioned in the text otherwise the article might provide to a reader a hope on a generic model applicable to all Arctic coastlines, or a vision that the Arctic coasts are all ice-rich.

We understand that the text with its respective title could leave the reader with the impression that all Arctic coasts are ice-rich, and we thank the reviewer for pointing this out. However, we do argue that our model could be applied on ice-poor lithified coastlines with very little modification (e.g. setting ice content to 0% and assigning the appropriate water level calibration). Despite this, we did choose to focus on rapidly retreating coastlines, and since these tend to be ice-rich, unlithified coastlines, we have not yet tested our model on ice-poor or lithified coastal segments. In the revised manuscript, we make it clearer to the reader that this model has not been tested on the other types of coastlines by adding the following statement (and other references to sandy coastlines, please see our responses below).

Abstract: Lines 17-19: "This proof-of-concept model is tested on ice-rich, unlithified coastlines. Through flexibility of input parameter choice (e.g. ice content, cliff height), the framework permits application to ice-poor, lithified or sandy coasts.'

- As continuation of the previous comment, it looks natural, if such modelling attempt would aim to model or refer to a well-described coastal process such as thermal abrasion or thermal denudation, and to model a core component of such processes. If fact model do model components of such processes. This would help to compare the model results with direct field observations. sOne may object that it is just a sense of usage of a certain terminology, as the article is efficiently deals with the processes called thermal abrasion and thermal denudation. Still, due to the aforementioned points, the article looks somewhat detached from the body of literature describing the processes on the Artic coastlines.

Thermal abrasion is directly taken into account when calculating the convective heat transfer coefficient between the wave action of the relatively warmer seawater and the coastline, and it includes wave height, period, and depth (Equations 10-11 in Kobayashi et al. 1999). The convective heat transfer coefficient equation has been added to the revised manuscript in Section 2.1.1, Equation 2. Thermal denudation, on the other hand, is not explicitly taken into account but we have now added an appropriate reference to it in Section 4.2.1, Line 453 when discussing an outlook for the model and coupling of a 1-D surface heat flux model.

Section 2.1.1, Lines 105-118: 'The parameter h is a convective heat transfer coefficient [$\frac{J}{sm^2°C}$] between the thawing cliff (h_c) or beach (h_b, Section 2.1.2) surface and warmer seawater. It estimates transfer of heat for a turbulent boundary layer in a unidirectional flow above a flat plate (Schlichting (1968), Kobayashi and Aktan (1986)) and is given by

$$h_{c,b} = \frac{\alpha f_w C_w U_b}{1 + F\sqrt{0.5 f_w}}$$

where α is an empirical parameter included for wave-induced thawing with α = 0.5 for unidirectional flow, f_w is a wave friction factor at the thawing surface that is dependent on equivalent sand roughness of either the cliff or beach, C_w is the volumetric heat capacity of seawater, and U_b is the representative fluid velocity just outside of the boundary layer and takes into account wave height, wave period, and wave depth. F is a parameter that changes according to thresholds imposed on the Reynolds number, which is directly proportional to the shear velocity accompanying the shear stress on the thawing surface, and changes depending on whether there are hydraulically smooth or fully rough conditions. More detailed information on the convective heat transfer coefficient and relevant parameters including U_b and F are provided by Equations 10 and 11 in Kobayashi et al. (1999).

Reference to above equation has been added in Section 2.1.2 Lines 143-144 : '… where h_b is the convective heat transfer coefficient on the exposed frozen beach sediment [J/(s m^2 °C)] [and is given by Equation 2].'

Section 4.2.1, Lines, 453 : '…to give a more complete overview of thermal denudation erosional processes at play at permafrost coasts.'

- In motivation for the article, the authors refer to the challenges ice-rich coastlines cause to the infrastructure. It is known from the practice, that it is normal to avoid ice-rich coasts when designing new infrastructural projects. Yet, sometimes handling such coastal type cannot be avoided. Hence, in general terms, relevance of models handling ice-rich sediments for the infrastructure developments might be somewhat limited. Yet, applicability of such models can take place in certain cases with relevant coastal conditions.

Yes, we agree with this comment and thank Anatoly Sinitsyn for bringing up this point. We have made the following changes to the revised manuscript:

Added the word 'existing' to the second sentence in the Abstract: '… causing problems for [existing] industrial, …'

Added statement to the Conclusion section, Lines 515-517. 'Such projected retreat rates from ArcticBeach v1.0 should not be used for infrastructure planning. The model is only capable to deliver first order approximations on how far the coastline will retreat, providing a basis for which associated impacts on already existing infrastructure and nearshore biogeochemistry might be better constrained.'

- As continuation of the previous comment, in my opinion, such model and its further development may consider the needs biogeochemistry on equal footing as the needs of infrastructure.

We also agree with this comment, and would like to direct to our answer in the related comment directly preceding this one. In addition, we have also added to the Discussion:

Section 4.3, Lines 475-480: 'Further development of ArcticBeach v1.0 should consider such biogeochemical applications on an equal or rather higher priority than applications concerning threats to existing infrastructure due to the nature of these two very different applications. Assessing threats to either existing or planned infrastructure generally requires a site-specific model and approach, with very detailed site-specific information and processes. We would like to make it clear that the design of ArcticBeach v1.0 lends itself to more pan-Arctic use for regional and first-order estimates of retreat rates and associated volume transport of nutrient-rich sediments into the nearshore zone.'

Sincerely yours,
Anatoly Sinitsyn
Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2021-28/gmd-2021-28-RC3-supplement.pdf
Powered by TCPDF (www.tcpdf.org)

Page numbers refer to the .pdf supplement containing the comments by Anatoly Sinitsyn (Reviewer 3).

Page 1:

Commented [AS1]: Comm4
Yes, coastal erosion is one of the main natural hazards when it comes to infrastructure in the Arctic. Yet, it is rear when we place the infrastructure at ice-rich coasts, as they are known to have highest erosion rates. We normally try to place infrastructure at more stable morphological types, such as barrier islands, river deltas, etc. If so, then motivation for the article could be somewhat shifted towards the biogeochemistry.

Yes, we agree and would like to refer to our responses to the two comments directly before this one.

Commented [AS2]: Comm1
Yes, frozen cliff and beach a partially frozen during summer and may be fully frozen during winter. These are normal variations in the state of a permafrost coast. The model deals with particular conditions of a permafrost coast. Hence, I would suggest first to outline that the model is handling a dynamics of a permafrost coast, and then, if the authors think that that is necessary, to point out that the model focused on partially frozen cliff and beach.

Yes, this is a good point—we have thus changed the following phrases: 'partially frozen cliff and beach' to 'permafrost coast' in Line 7 of the abstract and also 'partially-frozen' to 'permafrost' in Section 4.2.1, Line 453.

Commented [AS3]: Comm2
Arctic coastlines include different morphologies (+ sandy beaches, rocky coasts, river deltas), not only ice-rich coasts. Is the ice/rich coasts the dominant morphology in the Arctic? Perhaps such coasts constitute some 40-50 %, then one could say that the model will support such large-scale models when it comes to unlithified coasts.

We agree this is an important point and we have added a statement to the abstract—please see our response to the first bullet of Anatoly Sinitsyn's review above.

Commented [AS4]: Comm3
Is sea surface temperature masked during the times of ice cover? It think that it does not as it is a boundary condition defining permafrost temperature in the nearshore and the shore/shoreface.

Yes, the statement this comment refers to is correct, and the sea surface temperature is indeed masked during periods of sea ice cover. This is not a problem in terms of boundary conditions, because the model is simply not activated during times of ice cover (see added flow chart in response to Reviewer #2, which is a new Figure 1 in the revised manuscript). We have assumed negligible erosion takes place when the coast is covered in sea ice (although this has the potential to be developed in future work).

Commented [AS5]: Please Comm2 One may suggest to still outline the morphologies where the model could be applicable.

We thank Anatoly for this comment, and also the associated comments throughout the manuscript where this comes up. We have added a statement to the abstract referring to lithified ice-poor coastlines (see our response above to the first bullet point of the 'Main points.'

Commented [AS6]: Please see Coom1

Yes, please see our response to Comm1 and also Comm2. In addition, we have changed the the wording 'partially frozen coastlines' in this sentence also to 'permafrost coasts'.

Commented [AS7]: See Coom2

We have changed a statement in the abstract to mention lithified coastlines, please see our response to Comm2 and also Comm1.

Commented [AS8]: See Comm2. I again would like to mention that there are also other morphologies in the Arctic. More sever wave climate will for sure lead to stronger erosion on sandy beaches. And sandy beaches, at least in some cases do not have frozen cliff in summer due to deep active layer.

Yes, we thank Anatoly for this comment that we should include more statements referring to other types of coastlines. We have reworded the first sentence in the introduction so that it now reads:

Section 1, Lines 26-29: 'Due to warmer temperatures and reduced sea ice protection from bigger waves (Casas-Prat and Wang, 2020; Overeem et al., 2011), especially as freeze-up becomes delayed further into the fall storm season, Arctic coastlines are becoming increasingly vulnerable to the erosion of sandy beaches and destabilization of permafrost cliffs (Sinitsyn et al., 2020; Biskaborn et al., 2019).'

Page 2.

Commented [AS9]: Comm6. What about the roles of coastal types? Why it is not mentioned when it comes to the variability of erosion rates?

Yes, this is a good point and we have added the word 'ice-rich' in this statement to clarify which types of coastlines we are referring to (Section 1, Line 30).

Commented [AS10]: Comm7. It sounds somewhat confusion to mask different geomorphologies by referring to locations. See Comm2

Yes, we have replaced the word 'some locations' with 'some geomorphologies' in this statement (Section 1, Line 42). Please also see our response to the first bullet of the main points/Comm2.

Commented [AS11]: Why the authors do not mention typical coastal types, and corresponding coastal processes, which would be thermal abrasion and thermal denudation when it comes to unlithified ice-rich coasts?

We have now made several references to other types of coastlines in the text. For these, please see our responses to the first and second bullet point of the reviewer's main comments above, as well as comment 'AS8' and Comm2. In these responses, we have added new references to the processes of thermal abrasion and thermal denudation, as well as references to ice-poor, sandy or lithified coastlines.

Commented [AS12]: Comm9. Were those villages placed on an ice-rich coast? Probably some of them were.

Yes, that is indeed correct.

Commented [AS13]: Comm15. When it comes to the motivation behind this article and the biogeochemistry, would be good to refer to Lantuit et al. (2012) and point out that model can be helpful for moving from a static definition of organic carbon (as Lantuit et al. did) to a dynamic.

Yes, we thank Anatoly Sinitsyn for this comment and have now added the following statement where the biogeochemical modelling is mentioned:

Section 4.3, Lines 471-472: 'Such dynamic estimation of nearshore biogeochemistry would be an improvement to using estimates of coastline retreat and static coastal carbon content (Lantuit et al., 2012; Wegner et al., 2015).

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Page 4.

Commented [AS14]: Comm. Would be good to mention that conduction is not taken into account.

Yes, agreed. We have added the following statement also in response to the comments in the Methods section from Reviewer #2:

Section 2.1.2, Lines 159-163: "Consistent with the chosen erosion module in ArcticBeach v1.0, Kobayashi et al. (1999), conductive heat transfer and solar radiation are not directly included. Solar radiation can be partially accounted for in the sea surface temperature input and sea ice cover (see Section 2.3). Conduction effects are much smaller than effects of solar radiation over long time periods and are neglected. However, the opportunity to include effects of solar radiation can be implemented in later versions of the model, to include processes such as thaw slumping and 1-D heat-transfer permafrost models as described in Section 4.2.1."

Commented [AS15]: Comm12. Sediments in the coastal zone in the Arctic are normally saline, hence freezing temperature is lower that 0 C.

Yes, we are aware that salinity does impact the freezing temperature, but we simplified the study such that 0°C was used and did not take into account salinity measurements. However, we realize it was not clear in the manuscript that this was an assumption, and have now added the following to Section 2.1.1, Lines 97-98:
'…(assumed in this study to be 0°C, but can also be adjusted using salinity data near the coastline).'

Commented [AS16]: Comm13. What is Tm?

Yes, thank you, we missed a label for T_m and have now added the following reference:

Section 2.1.1, Lines 105-106: '… and T_m [°C] is the thawing temperature of the frozen sediment.'

Commented [AS17]: Comm14. "heat transfer coefficient" of the sea water?

We have now added an equation (Eq. 2) that describes the heat transfer coefficient. Please see our response to the second bullet point in the main comments above.

Page 5.

Commented [AS18]: Comm16. Long/shore sediment transport also defines erosion at the site when it comes to clastic sediments (sandy beaches). It would be useful just to mention that long-shore sediment transport is not considered by the model

Yes, we have now added the following statement:

Section 2.1.2, Lines 130-131: 'Long-shore transport also defines erosion on sandy beaches but is currently neglected in this 1-D approach.'

Page 6

Commented [AS19]: Comm18. What does this parameter mean? Please defied this parameter.

We have added the following description of 'sand roughness length':

Section 2.1.4, Line 185: '…(assumed to be 2.5 times the median sediment diameter (Nielsen, 1992)) ...'

Page 7.

Commented [AS20]: Comm19. 10 m/s ? What defines the selected value of 10 m/s/?
It is known that lower wind speeds are also capable to generate a storm. It is Ok to use 10 m/s, but one should then outline that this is somewhat a characteristic value. Jus [supplement pdf is cut off here so we cannot see the rest of the comment].

No, the manuscript states: '10m east and west wind speed vectors' and not '10 m/s'. This refers to wind vectors from reanalysis data that has been taken at 10m height. So, this means we include all wind speeds and do not have a threshold.

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Commented [AS21]: Comm20. It would be good to clarify then that, in fact, some of the winter storms might have been taken into account (which brings us closer to the reality).

Yes, this is a good point and we have added the following statement here:

Section 2.3, Lines 224-225: ‘Winter storms can occur over less than 15% sea ice cover, so when this happens, erosion is still simulated during winter.’