Author Response to Reviewer #1.

The comments in by Reviewer #1 are in font color black. The authors' responses are in green. The changes to the revised manuscript are in blue.

Geoscientific Model Development gmd-2021-28
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Summary

Thank you for the opportunity to conduct a review of this manuscript. Here, the authors couple an existing storm surge and erosion model to estimate annual rates of coastal erosion for two study areas in the Arctic (Drew Point, AK and Manmontovy Khayata, Siberia). The authors conclude that they can predict multi-annual cumulative erosion on the same order of magnitude of what has been observed at these sites and that their methodology is an important first-step toward an approach for estimating erosion for pan-Arctic scales.

Recommendation

I commend the authors on their writing styles, as evidenced by the small number typographical errors throughout the manuscript. However, this work hosts a multitude of technical issues, most notably the study's methodology and conclusion based therefrom. My feeling is that it does not warrant publication. For this reason, I have limited my review to two major comments, as opposed to more detailed in-line comments.

We thank the reviewer for their perspective, and provide detailed explanations below addressing the statements by the reviewer.

Major Comments

The authors highlight that "the most important root causes of Arctic shoreline change can only be gained through careful evaluation of the physical processes involved" and yet make no such effort for their own study. For example, one of the two sites where the authors apply their model is Drew Point, AK. Here, it is well known that permafrost blocks bound by ice wedges topple onto the beach due to an undercutting process that is facilitated by storm surge (i.e., "thermo-abrasion"). This reality is in stark contrast with the incremental style of bluff retreat associated with the model of coastal erosion employed by the authors (Figure 1). I understand that the authors ultimately wish to exercise their modeling framework elsewhere, but what is the scientific value of applying such a model to a place like Drew Point? My feeling is that Drew Point is not an appropriate location to apply or test the erosion model the authors use in this study.

We appreciate this comment by the reviewer and acknowledge that the purpose of choosing Drew Point as one of the validations sites for our study was not described clearly enough in the manuscript. The reason we have chosen Drew Point as a validation site for our model is exactly *because of* the special block erosion processes there, and the fact that ArcticBeach v1.0 purposefully does not include a specific representation of block erosion or any other complex three-dimensional process. Instead, our model aims to approximate coastal erosion as dimensional diffusive processes including a tunable bulk (offset) parameter accounting for unknown or unrepresented processes. To further elucidate the reasoning behind this decision, we provide the following main points:

- Block erosion is not the typical erosive process considering the arctic shoreline as a whole (Lantuit et. al. 2011, also stated in line 34 of the manuscript). Our approach is to include those physical processes that are most important in driving coastline retreat that can be applied across the whole arctic coastline, and not just one short segment like Drew Point, Alaska.

- We do not aim, at this point, to add physical processes that are only specific to certain stretches of coastline. We find this contrary to our goal of providing an order-of-magnitude estimate of erosion rates for coastlines that erode with different dominating processes. Through this approach, we take a first step for a physical parameterization that is well-suited to be incorporated into a larger earth system or coupled model, without having to resolve differences in specialized processes for certain coastline segments. This approach is justified at lines 30-38 in the manuscript.

- So, in order to validate if our model would be useful for simulating erosion on a coastline with a special process such as block erosion, when our model does not include block erosion itself (indicated in Section 2, line 66 of the manuscript), we carefully selected Drew Point, where block erosion is a special, dominant feature.

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- While we did address coastline-specific processes not included in the model in Section 4.2.1, lines 370-379, and addressed the reason we chose to leave some (such as notch erosion) out (Section 1, lines 32-34 and lines 36-37) and instead indirectly calculated (see response to Reviewer #2), we can see that it should have been more clearly explained in the manuscript, and have added what has been changed in the revised manuscript below in italics (now a new Section in the revised manuscript, Section 2.1.3, 'Validation Sites').

- We also understand that in the field of arctic erosion, many scientists have a research focus on the extensively-documented and special-case processes occurring at the rapidly eroding Drew Point (several of these models are referenced in the manuscript in Section 1, lines 27-30), and therefore might be concerned with our conscious choice to not include block erosion in our model. Therefore, we have added detailed statements in a new subsection 2.1.3 more clearly outlining the reasons we chose Drew Point as a validation site.

- We would also like to point out that while our model does not include block erosion, it does provide acceptable estimates of cumulative retreat at Drew Point, within 3.3 m from 2007-2016 (Figure 4). At the same time, the same model can also realistically approximate the cumulative retreat at a site far across the Arctic Ocean, characterized by very different coastal conditions and erosional processes and where block erosion is not a main mechanism (but instead primarily thermodenudation) is what controls coastline retreat (Mamontovy Khayata, Siberia, Figure 4).

- The new subsection (in the <u>tracked changes version</u> of the manuscript: <u>Section 2.1.3, lines 145-162):</u>

"The validation sites for ArcticBeach v1.0 are Mamontovy Khayata (MK), Bykovsky Peninsula, Siberia and Drew Point (DP), Alaska, USA (Figure 3). These sites were chosen because they: 1) involve specialized processes that are, at this time, purposely excluded in ArcticBeach v1.0, and 2) are coastline segments that are very different from each other. We chose not to include the specialized processes of either DP and MK in our simple model because our goal is to establish one general numerical model that represents a first step at simulating diverse types of Arctic coastline, efficient enough to be incorporated into a greater

earth system model. So, to establish this initial model v1.0, we chose these specialized places of MK and DP in order to test whether or not our model could simulate observed retreat, while, at the same time, not including all of the associated special site-specific processes.

The differences between the validation sites are highlighted by two main aspects. Firstly, the validation sites differ from each other in terms of their primary erosional processes. At MK, the primary mechanism for erosion is sub-aerial erosion, thermodenudation, and thaw slumping (Overduin et al., 2016; Günther et al., 2015). Coastline retreat at DP, on the other hand, is strongly driven by block erosion (Jones et al., 2018; Ravens et al., 2012). The block erosion occurring at DP is a specialized process that only occurs on very short stretches of Arctic coastline compared to the Arctic coastline as a whole (Lantuit et al., 2012). A second reason these validation sites are so different is that they are physically located far away from each other, such that the environmental forcing (sea ice cover, winds, sea surface temperature) are pointedly different. This allows for the model framework of ArcticBeach v1.0 to be tested because it does incorporate all of these forcing variables (which are also readily available from CMIP model output (Meehl et al., 2000) and reanalysis datasets). In this case, these variables were taken from reanalysis data mentioned in Section 2.3."

We have also added new statements to Section 4.2.1, Lines 432-437 of the tracked changes version of the manuscript, (please see our response to Reviewer 2):

"Further, our goal is not to explicitly represent some site-specific processes such as notch erosion, but rather indirectly calculate the effects of seawater on retreat by using Equation 1. This approach leaves the opportunity to utilize ArcticBeach v1.0 on a range of coastlines that have different erosional processes which do not include notch erosion as a primary mechanism for retreat (see Section 2.1.3). Notch erosion is thus indirectly calculated in Equation 1 with the terms d_c (water depth at the cliff toe, which must be positive for the erosion module to be activated, see also Figure 1) and l_c which refers to the length of cliff exposed to the seawater."

My biggest concern regarding the validity of this study is the lack of an error analysis of the model outputs (i.e., annual rates of erosion). The model predictions are higher and lower than the observations and in a somewhat chaotic fashion (Figure 4a-b). In many cases, the model predictions are several factors (approaching an order of magnitude) off. Given that the calibration of the model includes an input of historical retreat rates, is this level of error acceptable? What explains the seemingly non-systematic trends in model error?

The reviewer raises the question of what causes the error and 'seemingly non-systematic' trends in modelled retreat rates compared to the observed retreat rates (Figure 4a-b). We felt that addressing this behavior was so important that it warranted its own subsection in the manuscript (Discussion section 4.1), but since the reviewer has still raised this question, we see that this section must be addressed more clearly. Essentially, what we had tried to explain in Section 4.1 is that due to the fact that the tuning parameter used in the model is the mean of a timeseries of annually-calibrated values, and the difference between this mean value and a given annual value will directly determine if the model will over- or under-estimate erosion for that year. For example, years when the annual tuning parameter values are above the timeseries median (in other words, the years where the red stars in Fig. 8 are above the red dashed line) are the same years when the model underestimates annual retreat rates (the years where the blue bar is lower than the orange bar in Figures 4a and b).

For someone just quickly skimming through the figures, we have also added statements to the captions of both Figures 5 (previously Figure 4) and Figure 9 (previously Figure 8), so that this *purely systematic* variability is explained *directly in the figures*, instead of the reader having to dive in to the relevant Discussion Subsections 4.1 and 4.1.1. This way, we hope we have now made it clearer that the 'seemingly non-systematic trends in model error' are, in fact, very systematic, and can be directly explained by the way we have performed the model calibration.

Please see also our very relevant changes to the manuscript in response to Reviewer 2 comments Section [c] and Reviewer 2 Section: 'Results'.

New caption to Figure 5 in the revised manuscript (previously Figure 4) that refers to Figure 9 (previously Figure 8):

"*The years when the observed retreat rates are under(over)-estimated are the same years when the annual values of the so-called 'water level offset', a proxy for the physical processes at this point unresolved by the model, are above(below) the median values. These years are indicated where the red star is above(below) the red dashed line in Figure 9."*

New caption to Figure 9 in the revised manuscript (previously Figure 8) that refers to Figure 5 (previously Figure 4):

When the annual water level offsets (red stars) exceed the median water level offset (red dashed line), the model predictably underestimates observed retreat rates (see corresponding years in Figure 5) and vice versa.

We have also added these statements to <u>Section 2, lines 70-74 of the tracked changes version</u> of the manuscript:

"Small scale processes such as niche formation are accounted for in a bulk tuning parameter (Section 2.5) in this coarse-scale approach. We would like to point out that the model is not aiming for reproducing individual years and erosional events at a specific point, but to deliver large spatial scale and long term (decadal) approximations of coastal erosion related to the physical environmental conditions. This is also the reason why we restricted model tuning to only a single offset parameter."

I calculated a negative value for the Nash-Sutcliffe Model Efficiency (EF) statistic using the measured vs. modeled erosion rates reported for Drew Point in this study, which indicates that the mean of the Drew Point observations is a better predictor of annual erosion than the author's model. This back of the envelope calculation with a widely used error analysis metric underscores a potentially major issue regarding the predictive power of the author's model.

The EF is given by:

$$EF = \left[\sum_{i=1}^{n} \left(O_i - \overline{O} \right)^2 - \sum_{i=1}^{n} (P_i - O_i)^2 \right] \Big/ \sum_{i=1}^{n} \left(O_i - \overline{O} \right)^2,$$

where P_i are the predicted values, O_i are the observed values, n is the number of samples, and is the mean of the observed data. The EF statistic ranges from 1.0 to $-\infty$, with 1.0 indicating a perfect match between P_i and O_i and EF less than zero indicating that is a better model than P_i for simulating O_i.

Without a formal error analysis or comparison to another erosion model, it difficult to argue that this study has advanced our understanding of Arctic coastal erosion processes or produced meaningful insights for the communities that are vulnerable to this environmental problem.

We thank the reviewer for conducting this analysis. We understand that while the mean of the observed erosion rates may be a good predictor of past erosion rates, we would like to highlight that the Arctic (thus the controlling environmental variables such as open water duration and temperature) is changing in a non-linear fashion, thus the need for physics-based numerical models becomes urgent to understand what sort of changes we will expect to see in the future. It is not possible to use the EF analysis above on future erosion rates because we do not have observations of the future. We argue that while ArcticBeach v1.0 indeed does not perfectly reproduce past erosion rates at specific points and individual years, it does simulate realistic orders of magnitude (Figure 4), and, just as importantly, provides a framework for projecting retreat rates accounting for transient environmental conditions. Projected wind forcing is available, for example, from CMIP models, which have been built from well-known geophysical principles, and from wind speed and direction, the coupled storm surge model in ArcticBeach v1.0 can calculate what sort of relative water levels we will have in the future. Coastal retreat is only one part of ArcticBeach v1.0, and the other part is calculating relative water levels at the coastline where bathymetry is only roughly known (Section 2.2). Water levels at the coastline are an essential driving process of coastline erosion, and it is therefore useful to have such a water level model incorporated into the erosion model. The water levels calculated even take into account varying periods of sea ice cover, as described by reanalysis data. Relative water levels were able to be reproduced well (RMSE values given in the newly added Table 2), as described in Section 3.2 and shown in Figure 5 (now Figure 6 of the revised manuscript). We have also provided RMSE values in a newly added Table 2 (see response to Reviewer 2) for retreat rates.

In addition, processes impacted by projected increases in temperature, such as accelerated erosion due to thermodenudation and sea level rise, can also be factored into ArcticBeach in future work. This is discussed in Section 4.2.1, lines 371-379, and Section 4.3 (<u>Please see the additions to these subsections in the revised manuscript as mentioned in response to Reviewer 2</u>).

In summary, we should expect these coupled and non-linear processes will cause future erosion rates to deviate from the mean of past observations also in a nonlinear fashion. <u>Therefore, using the mean erosion rates today will likely not be the best way to predict erosion rates into the future</u>, and forcing variables such as water level must be taken into account in addition to other physical processes also present in our model (Sections 2.1 through 2.1.2, and <u>newly added Figure 1 (please see response to Reviewer 2</u>). While we have stated in the Introduction that ‘statistical methods might therefore show promising results to simulate erosion rates’ at line 25-26, but in response to the reviewer’s comment, we have also added statements in the Introduction section to highlight and further explain the need for such a numerical model and why we cannot rely solely on the mean of past retreat rates to predict future rates.

Added to Section 1, Lines 26-30 of the tracked-changes revised manuscript:

"Further, current statistical relationships of coastal erosion to other climate variables will change in the future because changes in the Arctic are happening in a non-linear fashion. In addition, how tightly certain environmental processes are coupled to erosion is also changing. For example, wave action in the Arctic is increasing nonlinearly, leading to variability of how vulnerable Arctic coastlines are to erosion in the future (Casas-Prat and Wang, 2020)."