Overview

This paper presents a new morphodynamic model of Arctic delta dynamics, based on DeltaRCM-Arctic, which has been described and analyzed in several previously published studies. The new model, ArcDelRCM.jl, adopts the basic water and sediment routing schemes from DeltaRCM-Arctic, adds several new behaviors (e.g., time dependent thaw depth, a time series of river discharge), and modifies the treatment of permafrost and ice cover from DeltaRCM-Arctic. The paper shows results comparing ArcDelRCM.jl to DeltaRCM-Arctic, using a reconstruction of DeltaRCM-Arctic based on the published studies, as the code is unfortunately not publicly available. They also present results from simulations conducted with ArcDelRCM.jl using parameters meant to mimic the setting of the Lena Delta. The authors find that results from the various simulations presented suggest that bedfast ice, and the protection of nearshore deposits by bedfast ice, is largely responsible for the creation of an extensive sub-ice platform known as the "2 m ramp." While I enjoyed reading this paper and appreciate the extensive effort by the authors to model these complex processes in Arctic deltas, I have several concerns about both the technical aspects of the modeling and the presentation of the results. I am confident this paper can be published and will be a significant contribution to the community following a major revision. I provide several suggestions below that I believe will improve the manuscript.

Specific comments

There is a lot of technical focus on the model but comparatively little in terms of scientific questions, hypotheses, results, and discussion. I do not suggest removing any of the methods, as I believe these are necessary to understand how the model works and specifically how it differs from DeltaRCM-Arctic. I do suggest, however, that the authors frame the paper with a science question or a series of science questions and conduct more quantitative analyses that allow them to understand the impact of their changes to the model and the range of results they obtain with the new model.

Similarly, it struck me that nearly all of the results in this paper are qualitative. It seems like a missed opportunity to present a more complete picture of the features and behaviors that can be observed from these new simulations. I suggest the authors try to quantify some of their results, such as the extent of the subaqueous deposits, or the distribution of elevations, etc.

The introduction lacks appropriate referencing for cellular automata and reduced complexity models in the geomorphology community, as previously pointed out by a community comment. Examples include Murray and Paola (1994, 2003) and Murray (2007).

The background on the 2 m ramp is quite short and vague, given the heavy focus on it in the results. Please provide a more thorough description about what we do and do not know about the formation of the 2 m ramp. L47 seems to suggest that the ramps have permafrost, but there is no reference for this and I'm not sure that this is universally true. The 'sub-ice platform' on the Yukon described by Dupre (1980), for example, is reported to not have permafrost.

The methods section is noticeably missing any description of the boundary conditions used and what types of experiments were performed. The authors have stated there will be some comparisons to DeltaRCM and DeltaRCM-Arctic, and also attempts to specifically model the Lena delta. Much of this information is actually in the results section, but I suggest it be moved to the methods section. A table listing all experiments performed and the appropriate parameters or processes that were changed would be helpful.

It would be useful to know which model source code you started with, specifically, as you've listed two (Liang and Perignon). Neither of these is the most recent, though: pyDeltaRCM

L105: this requires more discussion. Was the model tuned to reproduce some specific features using this parameter? Did you try tuning other parameters? Did you perform some sort of error analysis or are you just looking for features by eye? Please provide a physical justification for picking a new value for this parameter that differs from that in established literature.

Section 2.2 begins by stating that the authors refactored the DeltaRCM algorithm 'as we saw fit.' This is a little concerning, as it is hard to know what they mean by this. Did you test a non-refactored version against a refactored version to ensure you're still getting the same results? I also suggest comparing to the most recent version of the model, pyDeltaRCM, as suggested in the community comment.

It seems to me that if the paper is about simulating arctic deltas and specifically about modifications to existing models, then all changes made to the model should be included in the main text, not an appendix.

Section 2.2.2: maybe I'm missing something, but these units don't work out for your time dependent thaw depth. How do you get rid of the Kelvin? Also, is thaw depth not dependent on there being standing water? Just dependent on positive degree days? We know that there are taliks under water bodies, which is what the rules in DeltaRCM-Arctic try to simulate by enforcing some minimum thickness for permafrost cells. Can cells under channels be permafrost in your model?

Since the authors changed both the erosive rules for permafrost and the rules for how permafrost 'forms' in the model, it is not clear how both of these changes in combination affect the results. In order to assess the new permafrost rules, please also provide figures and analysis in the paper of permafrost extent and evolution in the model as compared to DeltaRCM-Arctic. Is there permafrost under the channels? On channel banks? On the ramp?

How sensitive is the model to the choice of I=10? This should be justified further.

I'm particularly concerned about the treatment of permafrost erosion in the new model. Based on my understanding of the text, the model assumes, or rather asserts, that permafrost can never erode, which is not accurate. I assume this because they state that cells can only erode down to the thaw depth. Permafrost riverbanks absolutely do erode. Does this mean that

lateral migration cannot occur if the cells immediately adjacent to a channel are permafrost? If erosion is limited only to the thawed layers, and thawing happens based only on a degree day index, then won't riverbanks always be frozen at depth and therefore completely non-erodible. This is not physically realistic. Please provide justification for this choice and/or a further explanation of the functionality of this choice in the model. L290 also states the authors compare to a DeltaRCM-Arctic run with E_p = 0.65, suggesting that this mimics their restrictive erosion. But this is not true, because the ArcDelRCM implementation does not seem to allow *any* permafrost bank erosion.

L233: Please be more specific about changes to delta t ('increased by a factor of a few.'). How did you discover this? Was it unstable for all other values? Is there a range of ok values?

L260: This addresses temporal changes to the input water discharge, but what about sediments? Are they just scaled with Qw? Is that realistic based on Arctic river sediment flux timeseries? Please provide more information on how sediment input is treated and justify this choice.

I think the authors have missed an opportunity in the results section to just directly compare to the publicly available model output from previous DeltaRCM-Arctic runs, noted in the acknowledgments of Piliouras et al., (2021): "Raw model outputs are available in Piliouras et al. (2020) through DOE's ESS-DIVE repository: 10.15485/1682304. "

Information about the use of graph theory/network-based techniques should be included in methods. Extracting channel networks from model output is not a simple task, and it can be rather subjective. Please include details about how channels were extracted, what methods were applied to those channel networks, and how. Also, why not use graph theory methods designed specifically for river and delta channel networks, such as those discussed in Tejedor et al., (2015)? Also, why was 105 chosen as the number of realizations to include? The authors suggest that based on their analysis of some graph theory metrics, they did not find statistically significant differences between the various runs, including between Arctic and non-Arctic runs. How do you reconcile this with the fact that Lauzon et al., (2019) and Piliouras et al., (2021) did show noticeable differences in some aspects of the channels? Those studies have only 3 replicates and did not do a formal statistical analysis, but do you think those findings were not representative? Or is it a matter of the metrics tested?

Regarding the comparisons to the Lena delta, why was this delta chosen over others? Why not other arctic deltas? Is there something unique to the boundary conditions of this system that might result in unique deltas or features compared to other Arctic deltas? Some discussion on this is warranted.

L340: Given the differences in inputs, I'm not sure this comparison is appropriate. The deltas should be compared when they have the same total volume input.

Figure 8: can you show side by side with a control for those variables that changed?

L366: why would the atmospheric melting period be shortened in the future? Please provide a justification for this choice.

The discussion should put the results re: ramp formation back in the context of what we already know from literature. For example, L376 should include some references. L378-380 has also been suggested by previous papers, including older observational studies and Piliouras et al., (2021).

L396-399: Can you quantitatively compare the amount of in-channel erosion between the two cases for locations where h_ice = 99.99% of flow depth? it would be helpful to understand how important this rule is, and how much its effect varies spatially.

L404-407: If the ice is bedfast, how is deposition occurring beneath it? Wouldn't it have to occur in front of it (i.e., upstream of it)? It looks like you have sub-ice channels in the ramp, though you have not discussed this. These are presumably responsible for the construction of the ramp under the ice, no?

L429-430: There are many satellite images of the Lena delta where ice on part of the ramp is already melted while ice in many channels remains (e.g., Figure 7 in Overeem et al., 2022). This is in contrast to your rule that delays melting on the ramp, which you claim is a major reason for its formation. Is it realistic to force the ice to stay on the ramp for a period of time? Doesn't this somewhat contrive an intended result? Shouldn't the ice be melting uniformly everywhere by incoming radiation? What is the justification for delaying it?

L484: Where is this comparison in the text? I do not remember seeing these metrics

Technical comments/Rephrasing suggestions

L22: 'key interfaces between permafrost landscapes and the Arctic Ocean.' Are Arctic deltas themselves not permafrost landscapes? I would rephrase this.

L55: This sounds contrived, like you are forcing a result. Presumably you are making modifications to the rules to include more physics or more processes, with the hope that you will reproduce a 2m ramp. Please rephrase. The following paragraph should similarly be rephrased. I'd hope that one purpose of the article is to explore the processes that shape Arctic deltas and to better understand those that might contribute to development of the 2 m ramp. As written, it sounds like the purpose is purely to develop a model and present model output.

L107-109: this should be rephrased to focus on what the model steps are, not what sections are in the cited paper.

Section 2.2.4: I suggest renaming this subsection, as this section does not actually describe shore or bank migration. The phrase shore/bank migration implies some redistribution of sediments, whereas you are describing a modeling step that simply smooths the water surface.

L277: What does this mean? That the atmospheric melting can change over time and that it can be nonlinear in its rate change?

Please include a legend on Figure 6.

Figure 9 (and other figures with this color scale): please adjust the color scale. The ramp feature is not particularly visible. Maybe you can change the colorbar to a log scale so we don't see so many numbers that are all black?

Figure C1: Please label the run names on the plots with rotated text instead of the numbers for the various runs. There is no table in the text, and these numbers/IDs are not used elsewhere, so readers cannot readily identify which is which.

References

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