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Comment on esurf-2022-25

Anonymous Referee #1

Referee comment on "An Arctic delta reduced-complexity model and its reproduction of key geomorphological structures" by Ngai-Ham Chan et al., Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2022-25-RC1>, 2022

Based on an existing and published reduced complexity model (RCM) that simulates the morphodynamics of Arctic deltas (Rebecca Lauzon et al., 2019; cited in the manuscript) Ngai-Ham Chan and co-authors have developed an extended DeltaRCM that is able to reproduce an important morphological feature: the wide 2-m ramp characteristic for arctic deltas (Erk Reimnitz, 2002; cited in the manuscript). According to the authors, the models published so far cannot reproduce this important characteristic. The changes from the DeltaRCM are that the entire model has been rewritten in the Julia programming language and that significant changes are made to the model in order to improve its ability to account for processes that are climate-sensitive.

The submarine ramp, which can reach an extent of up to 30 km off the Lena Delta (Siberian Arctic, Laptev Sea), has a significant influence, for example, on the wave energy impinging on the coast and thus on coastal erosion and sediment transport. Chan et al. found that "the delayed breakup of bed-fast ice on and around the deltas is ultimately responsible for the development of the ramp feature". Finally, they tested a strong climate-warming scenario on the simulated deltas. They have found that ramp features degrade on a time scale of centuries and disappear in less than a millennium. In this context, the RCM presented here is an important step toward realistic prediction of Arctic coastal evolution in the face of global warming.

I congratulate the authors on this interesting and important study and believe that the publication of the results contributes significantly to the understanding of the dynamics of the Arctic coasts. I have no doubts about the methodology presented in the manuscript. The results are also comprehensible to me. However, in my opinion, the manuscript focuses too much on the description of the model and too little on the scientific question/hypothesis that is to be answered on the basis of the further developed model. I believe, however, that this weakness can be overcome by a structural revision of the manuscript and that the publication will thereby attract the interest of a wider readership. I therefore recommend the publication of the manuscript after a moderate revision.

Below is a list of my comments and questions (the numbers refer to the line numbers of the original manuscript):

Abstract:

1 – The abstract should be revised to focus more on the underlying question. The abstract in Lauzon et al. (2019) is certainly a good example.

2 – What exactly does "Arctic deltas... provide key stratigraphic records of permafrost landscape evolution" mean? Why is this important in the context of this study?

6 – "We have rewritten..." Certainly, this was necessary work. But is it so crucial that it has to be mentioned in the abstract?

10 – I think that only a few readers know what "bed-fast ice" is. A brief explanation would certainly be helpful, wouldn't it?

11/12 Changes made to the modelling is responsible for the development of the ramp feature? You mean for the appearance of the ramp in the model, right?

12/14 What does "differences in channel structure" mean?

14 – "summer month contribute significantly...". Shouldn't it read " the morphodynamic (?) processes occurring during the summer months contribute significantly to the...". Due to these small linguistic inaccuracies, the abstract loses some clarity.

16 - It would be good if you briefly describe which changed environmental conditions you have assumed in the "strong climate-warming scenario".

Introduction:

22 – What do you mean by "key interface"? Please explain.

23 – Perhaps instead of "sediments and nutrients" it would be better to write "particulate and dissolved matter, such as...".

26 – "Arctic deltas will likely be affected..." Please specify.

41 bis 52 – I think this paragraph should be at the beginning of the Introduction because it explains the need for "improved" modeling and outlines the scientific question.

Figure 1 – Perhaps a small inlay, with an overview map informing about the location of the map section within the Arctic, would be helpful.

55/56 and 59/60 – The importance of summer month: In the study, model runs covering 10 days during the high-discharge period are compared with simulation results over the entire 4 summer months (Figure 7). Is it really necessary to emphasize that the summer discharge of one of the ten largest rivers on earth also has a significant influence on the development of the delta?

Methods:

2.1 Description of DeltaRCM(-Arctic) – The 5-page chapter includes a description of the previous RCM model and is thus a mere repetition of the already published model description (see citations in the manuscript). I think this chapter should be shortened considerably and rather describe the development of the DeltaRCM so far. In particular, it should be described why a further development of the DeltaRCM-Arctic seemed to be necessary.

2.2 The authors state that: "Since we do not have access to the source code of DeltaRCM-Arctic, we have no performance comparisons between the Arctic simulations." On the other hand, the results of the two models are compared in chapter 3. Can you explain how this goes together.

Results:

The whole chapter is well written and the results clearly presented. However, it is not clear to me on what basis (data/publication?) values for sand fraction (25%) and ice cover (40%) were chosen.

357 – This should already be mentioned in the abstract.

440 – Tidal currents in this area of the Laptev Sea are weak. Perhaps it is better to speak of waves and ocean currents in general. At the beginning of winter, when the newly forming ice is still mobile, the formation of sediment-laden sea ice in shallow water (e.g., anchor ice) and the subsequent export of the “dirty” sea ice plays an important role in the sediment budget of the delta. This should at least be mentioned (various papers by Reimintz and Are).

Conclusion:

The whole chapter reads like a redundant summary of the discussion. Perhaps the parts from the discussion that describe the possible future development of the model should appear here.

465 – Was this a random result or was the existing DeltaRCM-Artic further developed to reproduce the 2m ramp? Wasn't the scientific goal then to describe which modification of the model is "responsible" for the formation of the ramp? The Conclusions is, in my opinion, too much written from the technical point of view of the model improvement. Wouldn't it be better to start the Conclusions with the scientific question (what leads to the formation of the 2m ramp?), then outline the need why the existing model had to be extended (i.e. DeltaRCM-Artic does not reproduce a ramp) and then explain what was modified in the model and how you proceeded methodically?