Discussion Commentary For: “Arctic Delta Reduced Complexity Model and its Reproduction of Key Geomorphological Structures” https://esurf.copernicus.org/preprints/esurf-2022-25/

This paper was very thought-provoking and was a pleasure to read. The advances to the DeltaRCM modeling approach (writing the code in Julia, bed-fast ice protection/shielding, time-step stability criteria, etc.) are well-presented. However we do have suggestions and questions about some elements in this manuscript. We itemize and provide numbered comments below for convenience.

1. The introduction to reduced complexity delta models (L. 29-39) lacks references to the origins of this modeling approach for landscape models (e.g., Murray & Paola 1994) and the on-going debate between explanatory and predictive models in geomorphology (Bokulich 2013).

2. As this paper introduces a new implementation of the DeltaRCM modeling framework, we would like to alert the authors to the latest Python version of the model, pyDeltaRCM (Moodie et al., 2021). pyDeltaRCM has computational runtime improvements over the previous DeltaRCM frameworks (Matlab and Python). If possible, we would suggest comparing the new ArcDelRCM.jl code (in original DeltaRCM mode) to pyDeltaRCM in addition to the runtime comparisons presented in the paper (section 2.2).

3. In Section 3.1 comparison experiments between DeltaRCM-Arctic and ArcDelRCM.jl are described and then shown. Given the lack of access to DeltaRCM-Arctic source code, it is unclear how these comparison experiments were conducted. Was the Julia implementation used to mimic DeltaRCM-Arctic? Some clarification here would be appreciated.

4. The DeltaRCM modeling approach does not simulate a delta foreset. The discussion paragraph which touches on this (L. 439-447) could use some further commentary on how this deficiency might impact the results for the modeled ramps. As the ramps extend from the delta shoreline into the ocean, it seems like the model’s inability to accurately model sediment behavior in this region could impact the behavior of the ice ramps, and thus the implications of the results.

5. We would like to alert the authors to the work of Moodie & Passalacqua (2021) in which the same modeling approach is applied to simulate deltas with spatial scales comparable to the Selenga and Mississippi Deltas, this relates to the assertions made on L. 322-323.

6. It was slightly unclear how the sediment is being scaled when changing the input discharges to a time series (Section 2.2.6). We assume a sediment concentration is assumed and therefore the sediment discharge is scaling linearly with the water discharge (per description of Lena Model on L. 324). If this is the case, the comparison in Figure 7 does not seem to be appropriate. As we understand it, the volume of sediment input to the basin between the 10-day and 4 month simulations is not equal,

with a significantly larger sediment volume, both in absolute (m^3 / model year) and relative (m^/3 / model year / m^2 of model domain) terms. The results shown in Figure 7, seem to be more indicative of the total volume of sediment input into the domain rather than the differences due to ice-dynamics. We suggest scaling this comparison such that the total volume of input sediment is the same.

7. It would be helpful to provide an example plot of what the channel graphs look like (L. 316-320). It is clear from Figure C1 that there are no significant differences between the scenarios, but it would be nice to see planform views of the graphs themselves. From the images of the topography shown in Figure 5, there appear to be differences between the scenarios, although the channel structures and number of active channels seem similar between the cases.

8. The graph theoretic approach for channel network characterization that was referenced is designed for the analysis of polygonal trough networks, and it would be helpful to the reader to expound on how it was adopted to the distributary channel networks of deltas. In particular a clear definition for what an abandoned versus what an active channel is should be given for the graph analysis. In addition, we would like to alert the authors to the abundant literature on graph theoretic approaches to delta channel network characterization, in particular Tejedor et al. (2017) and references therein and Nesvold (2019).

9. We appreciated the commentary on how the Lena Delta differs from the analog model simulation (L. 434-438). Looking at the imagery of the Lena Delta as compared to the model simulations, the real channel network appears to be much more complex (greater number of channels, more junctions and tortuous paths) than the model network. How might this influence the results and findings? Is it reasonable to extrapolate results from the model to the real system given these differences (or what are the limits of the extrapolation)?

None of the above points are intended to minimize the contributions made in this work, which we find to be significant. We thank the authors and the Earth Surface Dynamics community for allowing us to comment on this interesting study.

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