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Interactive comment on "Quantifying Thresholds of Barrier Geomorphic Change in a Cross-Shore Sediment Partitioning Model" *by* Daniel J. Ciarletta et al.

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[[The article is well written. I appreciate the richness of results resulting from very simple equations. I think this is a good model to think about real settings. I don't have major issues with it. Instead, I have some minor comments / discussion.]]

+ Thanks. We were inspired to construct and explore this framework by the work of many other researchers, and we hope the community finds utility in this model or its results.

[[1-I was confused about the parameter Dt throughout the article. If I understand

correctly, this is both 1) the accommodation depth of the shoreface, 2) sandy substrate thickness, and 3) the inner profile closure depth (line 510). Can you better describe this parameter and all its interpretations early in the paper? More importantly, how does it relate to the classic depth of closure (which for century time scales should be much larger than 5 m, and much larger than 2 m).]]

+ We modify the last paragraph of the Background to better explain this, as Dt does have multiple controls depending on geologic context. In some cases, Dt is directly controlled by the presence of a consolidated sediment or bedrock interface, as it is in the Outer Hebrides and the Gulf Coast of Florida (allowing very small Dt). In other cases, where sediments are unconsolidated to depth, the accommodation available at the shoreface is based on the depth of the wave ravinement surface. We know from field observations that this depth is generally shallower than what would be expected of a classic depth of closure. For example, if we look at places like Fire Island (NY) and Parramore Island (VA) that have experienced progradation over the last centuries, we can see that there is 4 to 6 meters of sediment overlying what was geologically recently (centuries ago) seabed. As such, our best guess is that this vertical accommodation is more related to the inner depth of closure, which Hallermeier (1978) suggests as the seaward limit of shoreface that is significantly shaped by alongshore sediment transport processes (that we posit are responsible for most Qs fluxes in non-headland beach systems). Since changes in alongshore sediment fluxes occur significantly at sub-centennial timescales, translations of the uppermost subaqueous shoreface would likely occur over depths less than or equal to the inner depth of closure itself, and mostly independent of the outer depth of closure. Even where a barrier progrades significantly beyond the initial cross-shore location of the inner depth of closure over longer timescales, we believe the vertical accommodation to be filled does not change significantly over the spatial scales consistent with progradation (kilometers or less). This is because the slope of the shoreface in real-world systems becomes flatter with increasing offshore distance.

Finally, we consider that even the classic depth of closure could be very small in fetch-limited environments like bays and large lakes. In these cases, the inner depth of closure would be correspondingly small, and could help explain how barriers in places like the Great Lakes appear very dynamic despite limited energy availability.

Modifications to last paragraph of Background: "Moreover, sandy-substructure accommodation (the vertical space needed to be filled or eroded to invoke shoreline migration over multi-decadal scales) differs across the globe due to both local geology and available wave energy. In some cases, vertical accommodation is solely a function of antecedent geology, where consolidated sediment and bedrock interfaces define the seaward transgressive surface of the shoreface. In other systems with unconsolidated sediments, the depth of the shoreface available to be filled is a more a function of wave climate and uppermost shoreface lithology. Combinations of these influences are possible, which suggests the baseline sensitivity of barriers to sediment input/loss magnitudes varies considerably."

[[2-Would you be able to make a comparison between your model and the model of LTA14? Is there anything that your model can do while the LTA14 can't? Can the two models be easily merged, or do they use incompatible schematizations?]]

+ The biggest differences between the SBSP model and LTA14 are that the latter has a parameterized shoreface and consideration for backbarrier lagoon depth, while the former has relatively detailed subaerial morphology and rudimentary stratigraphic capability. The schematizations are only partly incompatible, and it may be possible to merge these two by using LTA14's shoreface to drive the direction and magnitude of Qs. Additionally, the merged model could incorporate some aspect of LTA14's overwash component to fill the backbarrier and control fluxes to the lagoon. It is not perfectly clear how this would work, since there are considerations for how overwash impacts any existing topography in a cross-shore profile. Furthermore, recent field data seems to suggest that storm-driven overwash events are more complicated than depicted in LTA14, with sediment movement both onshore and offshore from the subaerial system.

[[3-The authors found very rapid behavioral changes triggered by small changes in parameters (e.g., SLR>5 mm/yr). Even though this is plausible, I encourage the authors to consider a limitation of their model. Their model arbitrarily and independently fixes the fluxes Qs and Qd. As a result, the model does not have many degrees of freedom. The analogy is trying to simulate hydrodynamics by imposing boundary conditions very close to the area of interest: there is not much room for smoothing them and the system has a very stiff response. In reality, the fluxes Qs and Qd should not be fixed. For example, the foredune flux should decrease when dunes are larger. Also, Qs and Qd might not be completely independent. For example, larger waves might increase both Qs and Qd. Could you comment on these feedbacks?]]

+ The motivation behind this model is to test the relationship of these fluxes to morphology at the most basic level, and based on this comment, we consider that it has provoked precisely the type of thought that it was intended to encourage. We acknowledge here that we are mostly testing the magnitude difference between the subaerial and subaqueous fluxes, and so behavioral boundaries are understandably rigid. That being said, we can speculate on what is actually happening in natural systems. As we point out in the discussion, one of the major forces potentially driving real-world systems is deflation (Qw), which itself is probably modified by time-variable controls such as climate and vegetation. Even if Qd was somehow fairly static over decadal timescales, the inclusion of Qw competing with it in the subaerial domain would almost certainly reshape our regime plots to some extent, and could result in true equilibria for dune volumes (e.g. where dune Qd and Qw are balanced with respect to volume losses to sea-level rise). Additionally, while our model is somewhat rigid as currently parameterized, it is worth mentioning that our framework does pick up on the slowing of dune growth caused by Qd being distributed over a larger dune profile with increasing time (see section 3.3, paragraph 2)âĂŤa concept recently discussed by Davidson-Arnott et al. (2018).

Qs and Qd are also certainly related to each other, as wave energy shapes the sedimentology of the system itself, and waves are dependent on wind. Accordingly, Jackson et al. (2019) points out that coastal erosion and the development of large/transgressive dunes likely occurred synchronously in response to increasing windiness during the Little Ice Age. As discussed in section 5.2, what we find intriguing is how the magnitudes of Qs and Qd change with respect to each other under different energy regimes. If the magnitude of Qd increases faster than Qs with increasing system energy (windiness), this might explain how barriers undergo transitions to/from dune-dominated morphologies.

[[4-The color scheme is confusing. It goes to dark to white to black. In Fig. 8 bottom-left it seems that there are sharp discontinuities in the behavior (i.e., the horizontal streaks for Qs>30). But I think this is an artifact of the color scheme. (instead, I think that there are parts of the plot where discontinuities are real, e.g., between blue and yellow). You can check out scientific appropriate color schemes here https://www.nature.com/articles/s41467-020-19160-7]]

+ This is not an artifact, but it certainly could appear that way. Once the barrier begins to undergo sustained progradation (Qs>30), it is not just the height of ridges that changes at the end of each 500-year simulation, but also the number of ridges, which is in some cases affected by amalgamation. Compare the bottom left and right plots of Figure 8. The discontinuities in active ridge height generally line up with the number of ridge crests produced, but not always. Where these plots do not align, this is because amalgamation can reduce the number of ridges while maintaining the height of the active ridge.

The overall scheme for these figures was constructed specifically to be readable to persons with color deficiencies (test on Coblis Colorblind Simulator https://www.color-blindness.com/coblis-color-blindness-simulator/), as well as highlight important trends in the output. However, we acknowledge the data is genuinely hard to interpret because of the reasons stated above. We add this discussion to the caption of Figure 8 to help dispel any confusion:

“Note that the active ridge height and number of ridge crests do not change synchronously at the end of each 500-year simulation due to the presence of amalgamation. Discontinuities in the plot of active ridge height generally align with the plot of ridge crests produced, but where they differ it is because amalgamation can reduce the number of ridges while maintaining the height of the active ridge.”

[[5- Fig 3,4,5,. What is the slope of the backbarrier? Is it a parameter that affects the model result? Or is it just a graphical add-on? Please specify.]]

+ The slope of the underlying sandy platform is a graphical feature. We will add a line to the captions to specify this: “The backbarrier slope of the sandy platform is shown for illustrative purposes and is not currently parameterized in the model.”

[[Line 404. Not a good form to start a paragraph with however]]

+ Agreed. “In contrast to natural systems,” would be more appropriate. Edit made.

[[Line 451. Suggests that our model]]

+ Thank you. Insertion made.

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