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Earth Surface Dynamics Discussions

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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REVIEW – esurf-2020-88 (Ciarletta et al.)

This work by Ciarletta & colleagues is an engaging and well-written exploration of cross-shore barrier dynamics in a deliberately simplified numerical model. The authors are clear about what the model does and does not explicitly address, and that it serves a tool for first-order quantitative insight into transitions between barrier states and behaviors otherwise framed in conceptual terms. The differentiated regime space indicates the rich dynamics that the model is capable of simulating, and the authors are careful to keep one foot in the real world by constraining the fundamental parameters

of the model with empirical rates.

My comments are minor, but I hope offer some helpful suggestions:

Abstract, Introduction – To me, snippets of the Abstract (and related snippets of the Introduction) are written in a way that suggests a decade is approximately the upper end of the model-world time scale here. (Other parts of the manuscript are clearer in this regard.) Just to reflect: the model runs on an annual increment and can comfortably tick over for a couple of centuries, which means the interesting changes occur on decadal time scales. If the authors describe it as a model of multi-decadal barrier dynamics – on the order of 101–102 years – I think that might help readers have a better sense from the outset of what the model does and doesn't do. (And it creates useful space for the interesting discussion, late in the manuscript, regarding what explicitly including event-driven changes and fast-acting processes might add.)

P3L66 and elsewhere – Recommend cutting "we believe," since it's implicit in the conditional statement that follows. Would also change instances of "believe" on P14L440/442 – prefer "thought to be. . ." or similar.

P3L76 – "Whole-barrier dynamics at the mesoscale (10s to 100s yrs) are poorly represented by models, partly because the complexity of geomorphic processes at this scale cannot be easily represented by simple linear relationships. . ." – I think Werner (2003) on complex hierarchy offers a helpful explanation here (because what manifests at the mesoscale is a mix of linear and nonlinear), and I'd offer that McNamara & Werner (2008a, 2008b) are one example of a barrier model that does quite well in this regard (and ultimately manages to account for both natural and human-dominated conditions). The operational blocks of this model (i.e., triangles, partitioned distribution) in many ways appear related, at least conceptually, to that earlier work. And even if the similarity ends there, it's a sound foundation. Furthermore, the spatially extended version of the LTA14 model exhibits some interesting mesoscale behaviour – which the authors cite (Ashton & Lorenzo-Trueba, 2018). All that is to say – I suggest the authors

cut back the "poorly understood" spin throughout the Introduction and take a lighter, more open tone like the one at P3L80 – a framing of the problem that makes plenty of room for previous work. Another reason for that subtle adjustment is that this work, too, is, as the authors rightly state, a "first-order" model, steered but hardly constrained by empirical parameters.

Sticking with this same introductory sentence – I'm not sure that geomorphic processes operate at the whole-barrier scale (and I think the authors would concur). Returning to Werner (2003) – whole-barrier dynamics are an emergent property of geomorphic processes of sediment transport at smaller-than-whole-barrier scales. So perhaps the authors could revisit their warrant here. My sense is that models struggle to represent whole-barrier dynamics because they aren't built to capture emergent properties – which I think is more or less what this sentence is trying to convey.

P3 (Section 2) – I think the first half of the Background could be folded into the Introduction, with some adjustments for repetition. There's a shift at P4L95 to the "motivation" for the model – the conceptual model by Psuty (2008) – that could set up the Methods as the start of Section 3. Then there is another shift at p4L115 that reaches toward implications, which as a reader I wasn't ready for until I'd seen the model do its work. I think those last two paragraphs of Section 2 would sit well in the Discussion, and establish a far more interesting beachhead than the current paragraphs about the Bruun rule (see related comment below).

P8–9 – I like the narrative of Section 3.3 – but I think it belongs in the Results as 4.1, where it would nicely set up the subsequent sections that discuss trade-offs between variables.

P14, P15 – Glad to see the nod to alongshore sediment transport and vegetation feedbacks. Plenty of scope for further work – and the right acknowledgement here of what's important but just out of scope.

P12L370 – Does there need to be so much text chasing the Vousdoukas et al. (2020)

result? (I understand the rhetorical move to make the underlying point about nonlinear versus linear response to exogenous forcing.) Perhaps the second paragraph of that point – P13L387 – is what pushes it slightly out of balance. The idea of asking what it take for this model to return those rates of retreat is interesting, but it reads as a kind of excursion – all the more because it begins the Discussion. Rather than cut it completely, it could just be condensed.

I enjoyed reading this contribution, and I look forward to seeing it in its final form.

References

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