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Earth Surface Dynamics Discussions

# *Interactive comment on* "Complex coastlines responding to climate change: do shoreline shapes reflect present forcing or "remember" the distant past?" *by* Christopher W. Thomas et al.

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REVIEW – Complex coastline response: Thomas et al. (esurf-2016-35)

In this paper, the authors use the established Coastline Evolution Model (CEM) to explore morphological legacies of two different kinds of wave-dominated, "complex" coastal landforms – cuspate capes and flying spits – under a shift in forcing conditions from constructive to destructive. In the CEM, capes and spits are emergent, self-organized features (hence "complex") that develop in response to a high-angle wave climate (a predominance of deep-water waves with an angle of incidence $>\sim 45^\circ$ and $<90^\circ$ relative to the shoreline). Where a high-angle wave climate is thus constructive,



a low-angle wave climate has the opposite effect, driving diffusive smoothing of the coastline.

The authors present a numerical modelling experiment in which two complex coastlines, one characterized by capes and the other by flying spits, that form under high-angle wave climates are then confronted with low-angle wave climates. How long does it take for the existing (antecedent) morphology to diffuse away (if it ever does)? That response time is converted into a "characteristic time scale" of coastal morphological adjustment (for a given morphology). If the characteristic time scale for morphological adjustment is shorter than the time scale over which the forcing conditions change, the coastline maintains a state of quasi-equilibrium. (Here, that state is defined in terms of observed feature aspect ratios relative to those expected from the forcing applied.) Oppositely, if the characteristic time scale for morphological adjustment is longer than the time scale over which the forcing conditions change, then antecedent conditions impart a legacy effect on how the coastline evolves, resulting in observed features with shapes and spatial scales significantly different from those expected from the forcing.

This paper represents an important examination and extension of CEM behaviour, and I fully support its publication in ESurf. However, the summary above was challenging to assemble – and that is the primary aspect of this manuscript I encourage the authors to address. I didn't find this draft particularly easy to read. I think the "why" of the paper gets crowded out by the technical "how", obscuring the through-line and rationale underpinning much of the "what".

Part of this problem may originate in the Introduction. The second paragraph mentions "different wave climates", "shifts wave climate", and "slight differences in wave climate" without being more specific about what those shifts (directionality? angularity?) are or might be relative to present conditions. The reason the specificity of this framing matters is that the modelling experiment itself is very specific. The authors begin with complex coastlines created by high-angle wave climates and then only dial that forcing back (from high-angle to lower/low-angle wave climates) – they never turn the forcing

up, or oscillate between high- and low-angle wave climates. The carefully defined focus of the experiment is good. But the Introduction suggests, with its broad scope, that these two different coastline shapes perhaps will be battered with a variety of wave-climate shifts, rather than an effectively single shift (slow or fast) from high-angle to low. The end of the Introduction states that the "wave angle distribution was changed. . ." [P2, L25], but the authors don't specify the character of that change (anti-diffusive to diffusive) until [P4, L5–10]. Furthermore, after this initial mention of "the transition from anti-diffusive to diffusive wave climates" at [P4, L9], explanation of the forcing "trajectory" from anti-diffusive to diffusive [P6, L17] doesn't come for another two pages, deep in the experimental design (Section 3.1).

A clear paragraph at the end of the Introduction that frames the experiment more specifically (not just that the wave climate gets adjusted, but gets adjusted in this way. . .) would propagating through the sections that follow. Such a paragraph would also give the authors an opportunity to provide some up-front rationale for their experimental design before they explain the design in all its nuts and bolts. (In the paper's present sequence, the time frames described at [P5, L20–30] come across as arbitrary choices rather than deliberate.) Why only change the wave climate from anti-diffusive to diffusive? That reasoning may be straightforward to someone familiar with the model (and therefore skipped over), but I think playing out that thought experiment here is both worthwhile and necessary, given the paper's orientation with regard to management projections and shoreline-change forecasting.

I think it might also help for the authors to make clear early in the manuscript what "dynamic equilibrium" (or quasi-equilibrium) means in this system – that for a given combination of parameters U and A, the model eventually delivers a coastline configuration characterized by a certain pattern and morphometrics, with behaviour quantifiable at every point alongshore via gamma. "Dynamic equilibrium" appears often in the first half of the manuscript (as the initial coastline configurations get spun-up), but readers may not have a physical sense of what that equilibrium is or why it arises. Indeed, a section



on dynamic equilibrium – perhaps after Section 2.2, having introduced gamma as a metric – may offer a home for the "mechanism" sections 5.1.1 & 5.1.2, which I found out of place in the Discussion. The first two-thirds of both 5.1.1 & 5.1.2 are dedicated to what the mechanisms are in an anti-diffusive regime, anyway. Consolidating these various sections would allow the remaining "after the shift" elements of 5.1.1 & 5.1.2 to move into the Results (because that's really what they are), somewhere around 4.1.1 & 4.1.2.

One way to paraphrase all of my suggestions above is to look for ways to group like with like; many related pieces are compartmentalized into disparate subsections. With focused revision in the first two thirds of the paper, the closing sections will fit more comfortably with the Results. Finally, I encourage the authors to look for ways to explain things more simply wherever possible. The paper is at its best in those moments (the Conclusion, for example).

Minor comments:

[P2, L10] I think this paragraph would benefit from a different topic sentence, and the "analogous study" remarks should move down in the paragraph. (Indeed, these two paragraphs could merge, with rewriting.)

[P2, L24] Coming after a reference to Fig. 1, the line "these coastline morphologies using the appropriate wave-angle distribution" suggests (or leaves open the possibility) that the wave climates are tuned to the examples in Fig. 1. The modelled coastlines are generic – deleting this mention is probably fine, given the space in subsequent sections dedicated to explaining the modelling conditions. Note that the discussion of "mapping gamma" at P4, L5 is likewise ambiguous with regard to Fig. 1 – the gamma explanation needs clarifying.

[P5, L10] I did not understand Section 2.3.2 – neither the explanation offered, nor its relationship to the Results.

[Fig. 3] I don't understand what the authors mean by the "static wave climate" condition. The only mentions I found appeared in the figure captions, and given the various scenarios being compared I wasn't sure what was actually "static" – an unchanging PDF? a single angularity and direction? (In either case, what is the static condition?)

[P7] Section 4.1.3 looks to me like it should lead this subsection (become 4.1.1).

[general] Look for opportunities for shorter sentences, especially given the paper's technicality? (Swap semi-colons for periods?)

[general] The authors may disagree, but I would also urge them to specify "characteristic time scales of morphological change" (or similar) rather than simply "characteristic time scales" because, here, wave climate also changes and could conceivably have some characteristic time scale of its own. I know the only characteristic time scale of interest in this work is for morphology, but phrases like "characteristic time scales for change" [P12, L23] still read as ambiguous, supported as they are by the context.

[Fig. 3] Fig. 3b.iii, second panel of the triptych – is that a "capes" run or a "spits" run? Those look to me like the result of a symmetrical wave climate. . .

[Figs. 4 & 5] Is there a way to visually incorporate the "characteristic time scales of morphological adjustment" (both the first and second e-folding times) into these panels to lend the reader that background frame of reference?

[Fig. 6] Sub-panel at bottom right looks like it might be out of justification with other elements? Unfinished relative to the others?

I look forward to seeing the final version of this manuscript in print.

Best of luck –

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