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Interactive comment on “Assessing the natural morphological sensitivity of a pinned, soft-cliff, sandy coast to a changing wave climate” by A. Barkwith et al.

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In this paper, the authors apply a large-scale coastal evolution model (CEM) to the Holderness Coast, on the eastern seaboard of the UK, to explore how changes to the North Sea wave climate may affect the shoreline planform in one of the fastest eroding coastal regions in Europe.

This paper represents an important advancement in the progression of the underlying CEM, which has a well established legacy in the literature (originally presented in Ashton et al. 2001, and detailed in Ashton & Murray 2006a, 2006b – see manuscript for citations). Previous numerical experiments using this model have tended to investigate

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representational landscapes, morphology analogous to real settings – for example, a cusped coastline that resembles but does not reproduce the Outer Banks of the Mid-Atlantic USA. With the exception of a few conference proceedings, work based on this CEM has stopped short of calibrated simulations for spatially explicit sections of coastline. Here, by contrast, application of the CEM to a real coastline is fully in the spotlight, and the model's theoretical basis and dynamical underpinnings are means to an end.

I look forward to seeing this work published in ESurf once the authors address a few important changes in a revised manuscript.

– Major Comment

The opening pages of the manuscript (PP856–864) are well written and present a clear description of the research question, context, and rationale. However, after P865, that clarity of explanation and structural organisation begins to unravel. I urge the authors to look again at their argument and find its through-line. Make the methodological steps concise and straightforward, with each motivating the next (for example, see my first "specific comment" listed below). Likewise, the entire Results section needs more attention – not to the quantitative content of the results themselves, but to the paragraphs that should be lending those results greater transparency. I offer a list of suggestions that I hope will help the authors reassemble this paper such that the fundamental findings from this work are more accessible. Many of my specific comments are minor, but I expect their cumulative effect is significant.

– Specific Comments

P858, L18–19 – "Previous work has shown that changing the distribution of wave-approach angles can change the shape of a sandy coastline..." Are these the most appropriate / specific references for this statement? The Slott et al. (2006) reference makes sense, but the analysis in McNamara et al. (2011) involves a hazard-mitigation element that is irrelevant to the purposes of this paper. Use Ashton et al. 2006a instead?

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P861, L16 – "Due to uncertainty..." If there is so much uncertainty in predictions of what the North Sea wave climate will look like in the future, then why not use a longer (more inclusive) record of wave data? Similarly, at P865, L4–10, I'm not sure that two years of wave data is "a truer representation" of the wave climate just because the data are relatively recent. I suggest the authors clarify their justification for forcing the CEM with only two years of wave data. I suspect the answer has something to do with the fact that if these two years are generally representative of the North Sea wave climate – and are they? – then the data are sufficient for baseline probability distributions of wave height and incident direction, which the authors then manipulate anyway to create an ensemble of hypothetical future North Sea scenarios.

P865, L11–18 – The final paragraph of the Methods section seems divorced from the discussion of wave climate in Sections 2.2 & 2.3, and is less clear by comparison. Merge or integrate these considerations of wave climate? Changes here may follow from the comment above.

PP866–870 – This section, from the paragraph beginning with "Lithological and shore-line properties...", and the entire Results section that follows is especially unclear. The paragraph breaks do not seem natural, which usually means the weight-bearing topic sentences are buried elsewhere. Making the passive voice active might also help simplify the steps and logic throughout. (For example, consider the sentences framing P867, L13–14, "...changes to the coastal morphology and the sensitivity of coastline to change are appraised.")

The explanation for wave-climate permutations need to be cleaner for the results to make sense. I found the term "rotation" confusing. I understand that the authors are rotating their wave roses, but some mentions of rotation read like changes in wave-angle incidence related to refraction. This ambiguity should be moot, given that there is no explicit refraction in this model. On the other hand, refraction is implicit in the arguments behind shadowing and the relative angle between shoreline orientation and incident waves. If the authors can be even more clear that it's the orientation of the

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entire wave climate that they're adjusting, I think that would help.

Likewise, "relative erosion" – and what it illuminates that absolute erosion does not – needs to be clarified: "relative" with respect to what? The 2010 shoreline planform? I found this part of the calibration and results difficult to understand.

PP871–874 – Much of the Discussion (Section 6) reads like Results (Section 5), perhaps with the exception of Section 6.2. Reorganise Section 6 in concert with the overhaul of Section 5?

P874, L3–9 – The effects of "heterogeneous" versus "homogeneous" in this paragraph are muddled. I think the authors mean that different (heterogeneous) rock types have different properties (e.g., erodibility), but units of a given rock type are internally homogeneous?

P875 – Conclusions have inherited all the confusing elements of the sections they draw upon. In revision, as the Results and Discussion change, amend the Conclusions in kind? I think it is especially important to clarify L21–23, which seems to me a punchline that deserves emphasis.

P875, L24 – These implications-despite-caveats are unclear. Simplify?

(various) – In general, the revision needs a careful proof. See "propergated" (P865, L12); tense agreement (P868, L5); "erosional" (P869, L15); "lead" (P876, L10); and other examples.

Verb tense. the work happened, but the experiment results still stand. This version of the manuscript includes a mix of present and past. Employing past tense in the conclusions to state that something "has been explained" in the preceding discussion is confusing. Might the authors reiterate an important point a different way?

(other) – Lastly, I wonder if the authors would consider including a brief discussion of log-spiral bays, of which the coastline south of Flamborough Head is a neat example, and for which this CEM provides a neat explanation. (I'm aware of a Coastal

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Sediments '07 proceeding by Littlewood, Murray & Ashton: "An alternative explanation for the shape of 'log-spiral' bays". These proceedings are available online via Google Books.) I think highlighting this connection to a classic morphology might help extend the relevance of these results to a wider range of coastal settings, and embed the work, despite its spatially explicit focus, in the context of fundamental research.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 855, 2013.

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1, C397–C401, 2013

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