

Interactive comment on “Morphological and sedimentological response of a mixed-energy barrier island tidal inlet to storm and fair-weather conditions” by G. Herrling and C. Winter

G. Coco (Editor)

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The authors have addressed all comments by the reviewers and so I look forward to seeing a revised version of the manuscript. The manuscript is certainly of great interest to the audience of ESurf and the problem of sediment mixtures and wave forcing in tidal inlets is relevant and certainly requires more research efforts. It is extremely complicated to develop a methodology to study the effect of sediment mixtures especially when spatial and temporal data are missing. Still, the authors discuss the shortcomings of the work and show a plausible approach to the problem (let's keep in mind that in most circumstances data covering spatial and temporal variability is non-existent). Similarly,

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the problem of wave-current interaction(s) is so complicated that we do not have a clear idea on how to describe these processes. In this context, I agree with the authors that the case studied is quite different from the 'Portuguese' cases but I would like the authors to point out (adding a sentence or two) at these works and explain in detail why such works can not be directly applied to this study (in this context, please notice that a new article by Fortunato et al. has just appeared in the literature: Fortunato, André B., Alphonse Nahon, Guillaume Dodet, Ana Rita Pires, Maria Conceição Freitas, Nicolas Bruneau, Alberto Azevedo et al. "Morphological evolution of an ephemeral tidal inlet from opening to closure: The Albufeira inlet, Portugal." *Continental Shelf Research* 73 (2014): 49-63.). I understand there is almost one order of magnitude difference in the depth of the inlet but I remain convinced that some good and useful lessons can be drawn from the work on shallower inlets.

I have appreciated the effort of the authors who have prepared new figures and have detailed their reasoning in great detail. I have only a few additional comments: - the first two comments by reviewer 1 are interesting and I think the authors have replied appropriately. Have these comments resulted in any change in the text (I hope so) - comment by rev. 1, p74, l9. I think the reviewer refers to the paper der Wegen, M. van, A. Dastgheib, and J. A. Roelvink. "Morphodynamic modeling of tidal channel evolution in comparison to empirical PA relationship." *Coastal Engineering* 57, no. 9 (2010): 827-837. - swash bars. In my modest opinion this is a terrible definition that has unfortunately permeated through the nearshore community. I am not going to force the authors to change the term (if they wanted, they are certainly welcome to do so) but I hope they will add a couple of words indicating that even if they use the term 'swash bars', the term is confusing and totally unrelated to the physical processes. - rev 2: comment about sediment balance where figure 11N is introduced). This is a case of an interesting question and the authors have given an insightful answer. I only want to make sure that at least part of the discussion will be included in the new version of the manuscript. - please make sure that section 3.1 (where the model is described) is clear and contains all the details requested by the reviewers.

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I wish to thank the authors for their work in addressing in detail the comments of the reviewers. Even though the manuscript will go for re-review, I am confident the new version of the manuscript will be very much improved and I look forward to receiving a revised version of the manuscript.

Sincerely, giovanni coco

Interactive comment on Earth Surf. Dynam. Discuss., 1, 745, 2013.