Review report on "On a neck, on a spit: controls on the shape of free spits" by Ashton, Nienhuis and Ells, submitted to Earth Surface Dynamics.

This paper investigates the formation of free spits from a sandy headland. A simplified one-line shoreline model based on wave-driven alongshore sediment transport, sediment conservation and wave transformation from deep water is used. Very innovative is that the coupling and feedback headland-spit is considered for the first time and it is found to be crucial. The headland-spit system appears to be strongly self-organized with some elements being forced (e.g. motion of the fulcrum point) but most of them being emergent or autogenic (e.g. spit orientation, sediment input to the spit itself, length and shape of the hook). This has implications for correlating spit shape to paleo-environmental driving conditions, which turns out to be more complex than previously thought. The main external controls over spit shape, dimensions and dynamics is from the wave climate and the width of the headland. The results question previous research where spits were oriented in the direction of alongshore transport or the belief that wave refraction around the spit is the primary cause for recurving.

Overall I think this is an excellent contribution. It is novel and of high quality, very relevant for understanding spit dynamics and of interest for ESURFD readers. I find the manuscript well organized, well written and quite clear. The presented model animations are impressive. Unfortunately, there are no specific comparisons of model results with nature but the modelling work exploring the key physical mechanisms of headland-spit dynamics are worth publishing. I therefore recommend publication of a revised version after addressing some concerns and comments.

Main concerns:

1) The CEM model has two important assumptions that are in fact tied by the way sediment transport is computed: a) the changes in shoreline affect instantaneously the bathymetry up to the wave base and b) shoreline curvature is neglected (see, e.g., van den Berg et al., 2012). Although the authors are fully aware of this and some discussion is presented (e.g., sec. 3.1, 6.5) I find it not sufficient. In nature it takes some time until the surf zone morphological changes driven by alongshore transport reach deeper water. How this time scale compare with the time scale of spit dynamics itself? This has very likely some quantitative influence on the present results. But could it affect the main qualitative findings? Regarding b) it is surprising that a model neglecting shoreline curvature is used to describe hook dynamics. Probably this only affects hook behaviour and doesn't have a dramatic effect on the overall dynamics. But some discussion is necessary. My concern is to which extend those simplifications could affect the main results.

2) I am also concerned with the choice of the wave direction climate. The present study considers only the stable situation, $U<0.5$, and all model runs are for large directional spreading in the wave climate. Why? Are there technical reasons with the modelling framework? If some of the cases excluded have already been considered in previous work this should be commented and some comparison/discussion should be presented. If there are modelling difficulties they should be mentioned. Otherwise it seems a bit of a mystery to me. For example, what would happen for $U>0.5$? Would a spit be formed? The authors claim that the case of waves approaching from only one direction is pathologic and unlikely to occur in nature. I don't agree, there are coasts with a wave climate clearly dominated by waves from one direction with small spread (e.g. Namibia). In contrast, the paper shows experiments only in the other extreme, e.g., large angle dispersion. I think this is not very common in nature either. Could the authors present some experiment with a single angle or at least with small angular spreading? Ideally, the whole range should be explored from very low to high angular spreading. If it is not done the authors should provide a reason and their modelling exercise should be placed within a broader view. For example, computations with a single wave angle lead to a spit growing parallel to the tip of the headland (Kaergaard and Fredsoe, 2013a). Is this due to a fixed headland position or to the

wave climate? Therefore, the choice of wave directions can have a profound impact on the system. Please provide the readers with a broader overview/discussion and justify the particular choices for your model experiments.

Other comments:

– I don't understand the initial development. Assume a symmetrical wave climate and let us assume waves from the left at t=0. Then a small bump would develop to the right in the direction parallel to the initial tip of the headland. When waves reverse, this sand would move back to the headland and there would be no net growth. The spit can grow only if it is not parallel to the initial headland tip orientation. Only in this case the flux back and forth do not balance. But how this inclination is obtained? Other studies find the growth of spits in the same direction of the headland tip. I think this should be clarified. In connection with this, it is written in Sec. 4.3. pag 527, line 9. "As would be expected, narrower headlands erode faster than wider ones". This would be if Qs,in has the same value, but it is seen that it is smaller for narrower headlands (Fig. 8). But what sets the initial value of Qs,in?

– Sec. 6.2. Discussion on hook instability. I appreciate very much to see this section in the paper but I don't like as it is in the submitted manuscript. I find it a bit rambling and there is a mixture and some confusion between what happens in nature, what happens in the model and what happens in more sophisticated models. The authors start claiming that many spits have undulations at the depositional hook but they then continue by making an attempt to explain why the formation of such undulations can be inhibited. Why? Is that in nature it also happens that many spits do not have such undulations? Please, clarify. First state whether such undulations are the rule, or an exception, or fifty-fifty in nature. A rough qualitative statistics can be easily set, e.g., by looking at satellite images. Then go to the CEM model. Some of the experiments show some subtle undulations (e.g., 6b, 10a, 11c,12b, 13c) but others no. The authors' argument that initial wavelength of emergent sand waves is of several kilometres does not apply to CEM since it does not include wave focusing/defocusing by capes/bays (see main concern 1)). I would expect a similar behaviour as for a straight shoreline in Ashton & Murray 2006a (Fig. 9), small perturbations starting to grow and increasing in wavelength over time. Perhaps the length of the hook is too short? Probably the reason (1) pag. 535, line 18 plays a role, the rapid progradation of the whole hook overwhelms the dynamics of possible small sand waves. Then in nature, the possible conflict of time scales I mentioned in concern 1) may play a role. The time it takes the changes in shoreline to affect the bathymetry must be compared to the time scale of spit evolution itself. In some situations perhaps a potential instability becomes inhibited.

– I miss a bit more of general discussion of how spits form in nature. How realistic the case of an eroding rectangular headland is? Is it common? For example, spits often form at river deltas where there is a sediment supply to the "headland" from the river or just by a change in orientation of the coastline.

– Could the authors provide an explanation to why the fulcrum moves along a straight line?

– Sec 5.3, pag 533, lines 6-9. I don't understand. A shorter distance between Qs,max and 0 leads to larger gradients in alongshore sediment transport and so faster progradation rates.

Minor points:

– Abstract: "fulcrum point whose trajectory is set by the angle of maximum alongshore transport" → whose trajectory is a straight line in a direction set by the angle of maximum alongshore transport. Similarly, Sec. 4.3. pag. 528. Lines 23-25. "… the same onshore trajectory – the angle at which sediment transport is maximized – …". I'm not a native

English speaker but I think a trajectory and an angle cannot be directly compared. Something like "a straight trajectory in the direction of the maximum sediment transport" or something similar would be more clear/correct.

- Sec. 3.2, pag. 524, line 1. Qs,net is defined. Why? It confused me as I thought shoreline change at each time step must be computed with wave conditions at that time step that have been extracted from a time series realising the probability distribution. So, I guess this is just for interpretation of model results, isn't? Readers might be confused too, please clarify a bit more. In contrast, why mu-net is introduced seems clear.
- Pag.525, line 5. "Each spit eventually is supplied with an approximately equal length of the headland". Unclear sentence.
- Qs in the plots should be Qs,net, I guess.
- Sec. 4.2, p 525 20. "All spits must tend to a zero flux at the downdrift end". This is exact only within the model assumptions where there is full wave shadow shoreward of the spit. Because of waves entering the bay (see Kaergaard and Fredsoe, 2013a) flux is not exactly zero at the tip of the spit.
- Sec. 4.3. pag. 528, lines 13-16. I guess this provides an explanation for why narrow headlands lead to smaller hooks. I'm right? But the connection seems a bit loose. Could you make it more explicit?
- Sec. 5, pag 530. Experiments where updrift coast recedes at a set and constant rate. I presume a sediment supply to the headland to balance with Qs,in and the erosion rate is assumed. Perhaps this should be told explicitly.
- End sec. 5.2. pag 532, lines 18-19. "The fixed headland case … infinitely long sandy headland". I don't see the point here.