Response to Reviewer #1:

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.

> *We thank Reviewer #1 for these useful comments. We agree that the model results motivate a direct comparison with natural examples in terms of wave climates along the spits. Such analysis, which we are working on, is beyond the scope of this publication. Motivated also by direct comments from Reviewer #2, we have expanded the text in places to highlight similarities between modeled spits and the examples shown in Figure 1, also now including wave climate information in Figure 1.*
>
> *(Changes are detailed in specific responses to Reviewer #2)*

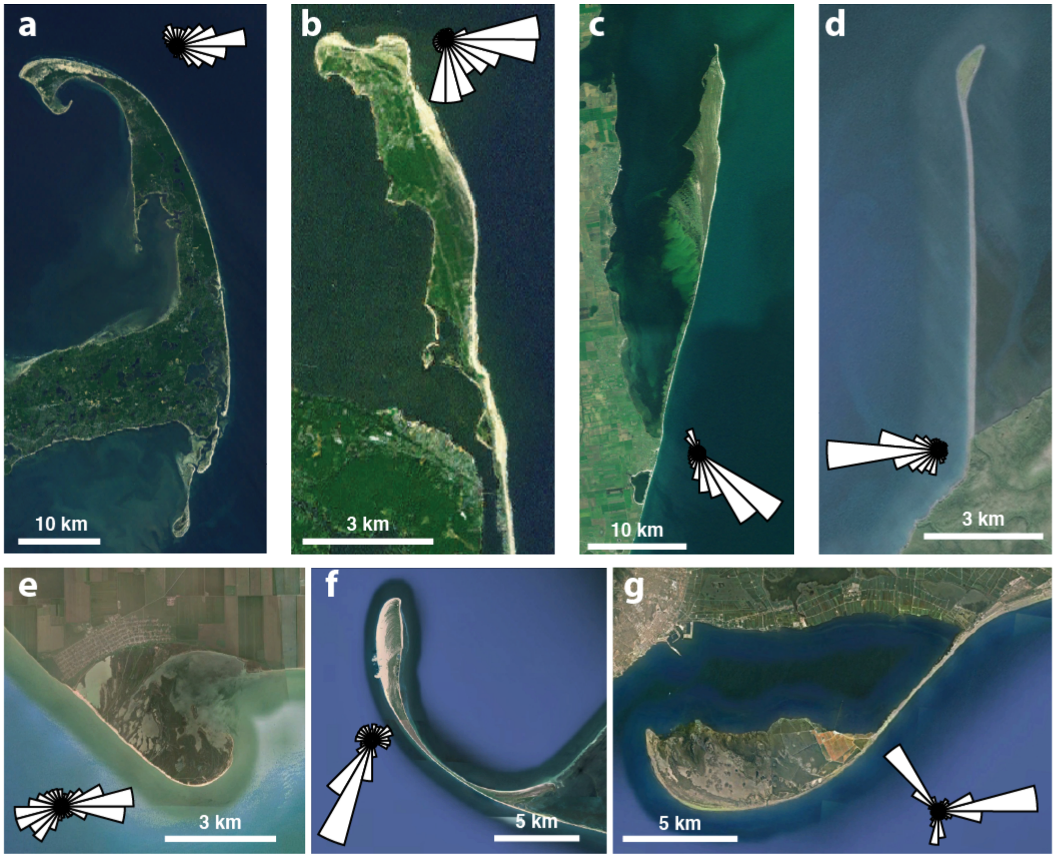


Figure 1. Natural examples of free spits: a) Cape Cod, Massachusetts, USA, b) Sandy Hook, New Jersey, USA, c) Dzharylhach, Ukraine, d) spit on Hagemeister Island, Alaska, USA, e) Kamyshevatskaya Spit, Russia, f), Ostriv Tendrivs'ka Kosa, Ukraine, g) La Banya Spit, Spain. Insets display the angular distribution of deep-water wave energy, (a-f) from WaveWatch III (Chawla et al. 2011), and (g) from the Cap Tortosa wave buoy (Bolanos et al, 2009).

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?

> *Response: We agree that the assumptions underlying the one-contour-line modeling approach meet a limit when the disparity of temporal scale between surf zone and shoreface changes is appreciable. We now more explicitly discuss the timescales over which such approximations are applicable, which, for natural cases, is larger than annual*

scales and spans the decadal to centennial scales of spit formation and growth. We do not believe that short-term coupling should have a distinctive qualitative effect at large scales. If so, we would expect it to be observable in the form and change patterns of natural spits.

Changes: We have included additional text addressing the timescales of spit growth and shoreface change in section 3.1:

> *"As with all one-line models, the CEM approach assumes that sediment transport gradients in the surf zone are spread across the profile at a rate commensurate with that of long-term shoreline shaping. As closure depths are typically considered at annual to decadal scales (Bruun, 1962; Hallermeier, 1981; Swift et al., 1985) the CEM is not necessarily appropriate for simulating shorter-term changes (Falqués et al., 2011), but should be appropriate for simulating accumulated shoreline change over the decadal to centennial scales of spit formation and growth. Similarly, the influence of storms on alongshore sediment transport is spread across time, integrated over the long-term wave climate."*

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.

Response: This is a useful perspective and we look forward to comparisons with our model results from other models that more explicitly account for shoreline curvature. We agree with the reviewer that the curvature will affect the hook shape most of all. This should not affect the qualitative results, particularly in terms of the dynamics of main spit components we identify (Figure 5) as well as the trends in sensitivity to environmental conditions (Figure 6-15).

However, even as we are confident in the qualitative importance of Qs,max on spit evolution, more detailed treatment of shoreline curvature should affect both 1) the mass balances (which as we demonstrate controls the rate of spit extension and recurving and 2) the shoreline angle at which Qs,max occurs (due to wave ray divergence over the curved hook's shoreface). We now more explicitly address these matters, particularly in terms of Qs,max, in section 6.5 of the Discussion.

Changes: We have added text to the discussion Section 6.5:

> *"As demonstrated by Kaergaard and Fredsoe (REF), shoreline curvature can also affect the shoreline orientation at which Qs,max occurs by up to ~10°; although this would not have a qualitative effect on the interactions between spit components, this would quantitatively affect the orientation of spit growth and therefore could affect the fluxes onto the spit itself."*

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?

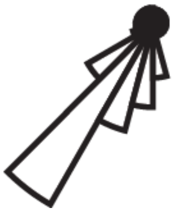
> *Response: There are no technical/model issues with modeling high-angle features (particularly as CEM was designed to model such conditions). One modeling example can be seen in Ashton et al., 2007 where a spit is formed. We here chose to model spits growing landward from a headland as this is the classic example in coastal sedimentology, the headland-spit system defines a bound littoral cell from nodal point to spit end, and this landward extension only happens for the common case of U<0.5.*
>
> *We also have chosen to present a series of experiments where the updrift condition (either the headland width or updrift erosion rate) can be controlled. Shorelines subjected to high-angle-dominated wave climates (U>0.5) are unstable, such that shoreline undulations should grow on a sandy shoreline up drift of a coastal perturbation. This could result in a constantly evolving updrift condition as self-organized undulations would migrate into the domain (as can be seen in Ashton et al., 2007). For this case, the cross-shore amplitude of an initial bump on the coast would be as important as the width. In short, for high-angle waves (not necessarily a common case), the dynamics are more complex and it is less straightforward to isolate the updrift boundary conditions.*
>
> *We know of no previously published study that investigates the controls on spit form across as many environmental conditions as we explore. A study of individual high-angle spits could also be interesting, but such a study would likely require different boundary conditions, and in the case of just one spit, probably different updrift conditions to resemble nature, such as a rocky coast. Even as explorations of high-angle wave instability have proliferated, low-angle coasts are most common and their dynamics have been perhaps overlooked.*
>
> *Changes: We have changed the text in Section 3.1 to more explicitly motivate our model approach:*
>
>> *"Our goal is to model the classic case of spits growing landward by extending off of a headland, for our first experiments spanning a littoral cell from headland source (nodal point) to spit sink (Inman, 1989). Therefore, for all experiments we use U < 0.5, such that headlands experience a net diffusive wave climate, in contrast to the case of spit growth in a high-angle-wave environment as explored in other studies (Ashton et al., 2007; Kaergaard and Fredsoe, 2013b, 2013c) (Peterson et al. 2008). Even with a predominance of low-angle waves, larger values for U result in more waves approaching from the sides of growing spits, which affects the recurving graded spit shape. Because we categorize the wave field into four 45° bins, the wave distributions are relatively broad, a condition common for some, but not all coasts (Figure 1)."*

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.

Response: We agree that the terminology here was unclear and we have reworded our statement to specify that the case is limited for simple one-line modeling.

However, we disagree with the reviewer's suggestion that the most common case in nature is one with waves approaching from one angle. We now include wave roses for the natural examples in Figure 1, showing a variety of conditions, including several cases with large wave spread, limited waves from one directional bin, and several constrained directional bins. We also note that even within one "bin" of 22.5 or 15 degrees there are significant potential differences in both Qs and mu (up to 50% and 75%, respectively, see Fig. 1), which suggests that modeling waves from multiple directions is likely more warranted than modeling waves from only one direction. For example here is the pdf of hindcast peak wave energy (H^12/5) for WaveWatch for (lat: -25, lon: 14), just offshore of Namibia:

namibia waverose



Even though there is a prominent wave direction, at this 15 degree binning, almost 50% of the wave energy is not coming from this main directional bin. (This simplification is further limited as simplified wave field descriptions tend to be of peak direction, and do not include wave spreading (which is often included in models) or secondary wave fields that are likely to affect the shore on the shadowed portions of the hook.)

Changes: We now include wave roses for the natural examples in Figure 1. We have adjusted the text in Section 3.1 as annotated in the previous response.

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.

Response: It was not our intention to omit different types of wave climates, here we use an approach that we have applied many times in the past, by categorizing waves using two simple parameters. We could redesign our experiments to include perhaps a wave climate as a Gaussian distribution with different amount of spreading; however, this would be an altogether new enterprise and it remains unclear whether such experiments would add significantly to the conceptual findings we present here.

Within the broader context, for all of the model experiments we show, after initial spin-up

> *from the arbitrary initial conditions, spits do extend offshore of the headland at a more or less straight direction, with a smooth graded shoreline extending from the headland to the spit itself. This gentle shape is maintained by shoreline smoothing by alongshore sediment transport gradients acting both on the spit shape and the headland shoreline. We do not see a distinction between this graded shape and the scenario of a single modeled (high-angle) spit presented in Figure 5 of Kaergaard and Fredsoe, 2013a (and it is unclear from this figure what the initial conditions of this model experiment are). As such, we are somewhat unclear by the reviewer's assertions that our results differ from others that have been presented. The simulated spits we show all grow parallel to the headland—this occurs because the headland coast itself rotates.*
>
> *We agree that wave angle distributions have a profound effect on the results, which we discuss throughout. For this reason we include a diagram of the wave climate used for all model result figures. Overall we are somewhat puzzled by this suggestion that we need to strongly defend our model experiment choices. Why isn't a similar criteria applied to models that use a single wave approach angle when this case is demonstrably an oversimplification for most natural wave climates?*
>
> *Changes: We now have adjusted the text in Section 3.1.*

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.

> *Response: The reviewer here provides an interesting thought experiment, one that often is brought up in terms of not only symmetrical spit growth, but to question why spits would be expected to grow off of a headland even against the direction of net alongshore sediment transport (e.g. Fig. 4) In contrast to the assumption in this thought experiment, modeled spits do not extend in a straight direction from an arbitrarily aligned initial coastline. This is in part because the spit grows from sediment sourced from the headland and the headland rotates.*
>
> *The reviewer does not provide other examples of spit growth other than Kaergaard and Fredsoe (2013a), and as we discuss above we do not believe that there is a clear distinction between our model results and previous example as, in both cases, the shoreline eventually becomes smooth across the headland/spit transition. One just needs to rotate their perspective (be it their head or model domain).*
>
> *Changes: As we are unclear of how our model results contrast with previous studies we cannot directly address that point. However, we point out that spits do grow linearly from the coast in Section 4.2:*
>
> > *"As a result of rotation of the headland coast, modeled spits eventually extend*

> > *more or less straight off of the headlands coast."*

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?

> *Response: We agree with the detail of this point and have added more discussion of this secondary effect. The model experiments show that the initial value of Qs,in is Qs,max (Figure 8), and that Qs,in becomes smaller over the course of the model experiment. However, the difference between Qs,in for narrow and wide headlands only partially compensates for the difference in erodible mass between narrow and wide headlands.*
>
> *Changes: Changed text in Section 4.3:*
>
> > *"Because of this reduction of sediment export from the headland, the rate of erosion of narrower headlands reduces as the headland-spit system develops, but not enough to slow a narrow headland to the same rate as a wider one."*

– 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.

> *Response: Agreed. We have rewritten this section entirely, with particular attention to the thoughtful details brought up here.*
>
> *Changes: We have rewritten Section 6.2 throughout:*
>
> > *"We define the morphodynamic hook as the shoreline that is past the maximum in alongshore sediment transport; as a consequence, this shoreline should be unstable to perturbations because of the high-angle wave instability. The propensity for spit ends to tend towards instability has been previously proposed for oblique wave incidence (Ashton and Murray, 2006b) and shore-normal incidence (Ashton et al., 2007). Shoreline sandwaves and other organized*

> *shoreline undulations can be found on the ends of many spits (Davidson-Arnott and Van Heyningen, 2003; Medellín et al., 2008). All but one of the sample spits shorelines we selected show shoreline undulations, both subtle (Figure 1e,f,g) or prominent (Figure 1b,c) along the hook, occurring across a range of spatial scales (note that some features are too small to be visible at the scale of Fig. 1). The CEM model, with its simplistic wave refraction treatment, tends to form shoreline sandwaves at the model's discretization scale. These modeled sandwaves tend to develop most prominently along longer, straighter hook shorelines, and are absent on shorter hooks with larger curvature. As these short hooks are rapidly prograding, it is likely that large gradients in alongshore sediment transport overwhelm smaller instability-driven gradients and the shoreline remains smooth.*
>
> *Theoretically, shoreline sandwaves are expected to occur throughout the hook; however, their prominence along a natural spit depends on several factors. Foremost, other recent research, using more complex wave transformation approaches has shown that the tendency towards shoreline instability emerging from an initially straight shoreline may not manifest in the growth of perturbations at spatial scales smaller than 100's of m to kilometers (Falqués and Calvete, 2005; Falqués et al., 2011; López-Ruiz et al., 2014). Therefore, in nature, shoreline sandwaves may not be prominent on short spit hooks for the following reasons: 1) such short hooks are often prograding rapidly such that the gross gradients in alongshore sediment transport may dominate shoreline change, 2) these hooks may be of insufficient length for km-scale sandwaves to initially develop, and 3) also because of this limited coastal extent, small-scale sandwaves could migrate to the spit end before becoming large. In contrast, long gently curving hooks (such as those found for slowly eroding headlands) have long stretches of coast exposed to high-angle waves. Emergence of autogenic shoreline features such as sandwaves and, in some cases, flying spits, adds to the variability of the hook length in this case.“*

– 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.

Response: Although of course our example of spits growing off of a perfectly rectangular headland is a simplification of any natural case, spit growth off of a headland is the classic model of formation. Although spit shapes are common on some delta coasts, previous research (Nienhuis et al., 2013) suggests that spits are more likely to form from reworking of abandoned deltas rather than from actively growing deltas.

Changes: We have added text to Section 2 providing background further addressing the types of environments where spits tend to be found:

> *“Spits are detrital, non-cohesive (sandy or shingle) depositional features emanating from headland coasts, extending for many kilometers (Figure 1). In plan view, spits are best identified through their curved end, which generally consists of a series of sub-parallel beach ridges indicative of shoreline progradation. Near the headland, spits are usually narrow, backed by an*

> *embayment or perhaps backbarrier marshes, and susceptible to barrier overwash (Schwartz, 1972). Just updrift of the zone of accretion and downdrift of the overwashing region, truncated beach ridges suggest a region of long-term erosion (Figure 1). Spits typically extend off of shoreline protuberances, often from eroded or reworked headlands formed by other geologic processes (Davis, 1896; Roy et al., 1994). Spits are also common on wave-dominated deltaic coasts, such as the Ebro and Rhone River deltas (Raynal et al., 2009; Sanchez-Arcilla et al., 1998) with recent research suggesting that deltaic spit extension occurs most vigorously after delta lobe abandonment (i.e. when local fluvial sediment supply is drastically reduced) (Nienhuis et al., 2013)."*

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

Response: This is a great question and we provide a response in the text.

Changes: We have added text to Section 6.1:

> *"Why does the angle of maximum sediment transport set the fulcrum point trajectory? As the spits tend towards steady state, the gradient in Qs,net becomes constant along the recurved hook (Figure 4,6). In this graded state, erosion and deposition are balanced on either side of the fulcrum point. Regardless of the rate of erosion/deposition (i.e. slope of Qs,net), because the fulcrum point is defined as the shoreline angle where Qs,net is maximized, this point translates downcoast at this flux-maximizing angle. However, outside of steady state (or a condition near it), erosion and deposition may not be exactly graded passing through the fulcrum point, which could results in a different trajectory. This transient behavior can be seen in initial stages of spit development (Figure 9) and would be expected for changes in environmental setting (for example a non-rectangular headland or a spit growing into a shoaling basin)."*

– 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.

Response: Yes, but a deeper basin then reduces these progradation rates. We agree that our text was perhaps misleading.

Changes: We have adjusted the text in Section 5.3 to avoid misinterpretation.

> *"However, in a deeper basin, mass conservation requires a shorter shoreline arc length (with corresponding larger curvature) to cause the same gradient of alongshore sediment transport (the difference between Qs,max and 0 at the spit end) to prograde the hook at the same rate as in a shallower basin (Figure 14d)."*

Minor points:

– Abstract: "fulcrum point whose trajectory is set by the angle of maximum alongshore transport" à 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.

> *Response: Agreed.*
>
> *Changes: We have changed this as suggested in both locations.*

– 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.

> *Response: Yes, Qs,net is for model interpretation. The model itself is forced with short-term (1 day) wave angles from one direction, but the full wave climate is responsible for longer-term shoreline change. To hindcast Qs,net shaping the modeled shorelines, we sum the Qs using the modeled shoreline orientations and all wave angles from the distribution. Because the shoreline is always changing, it is impractical to sum Qs at any shoreline location during the actual growth of the shoreline.*
>
> *Changes: We have altered the text to avoid misunderstanding in Section 3.2. We also have relabeled the Figures as suggested below.*

– Pag.525, line 5. “Each spit eventually is supplied with an approximately equal length of the headland”. Unclear sentence.

> *Response: It was not clear that we were referring to shoreline arc length.*
>
> *Changes: We have altered the text in Section 4.1.*

– Qs in the plots should be Qs,net, I guess.

> *Response: Agreed.*
>
> *Changes: We have changed the figures.*

– 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.

> *Response: Our statement is also not exact, as we suggest “tends” to. Regardless of refraction, diffraction, and local wave generation, sediment transport rapidly decreases around spit tips. Overall, the potential for sediment loss off of the spi end is probably a larger issue with the mass balance approach.*
>
> *Changes: We have adjusted the text to be less strong.*

– 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?

> *Response: The discussion of this phenomenon continues to the end of this section, where we finish by discussing the effect on the hook.*
>
> *Changes: No changes.*

– 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.

> *Response: We agree that it is useful to specify this boundary condition. Actually, we have an open boundary condition at the updrift limit.*
>
> *Changes: We have added text in Section 5.:*
>
> > *"Therefore, for our model experiments the updrift boundary flux condition is open (set at each time step based upon the local shoreline orientation compared to the wave climate)."*

– End sec. 5.2. pag 532, lines 18-19. "The fixed headland case … infinitely long sandy headland". I don't see the point here.

> *Response: We agree that this statement was confusing and did not add much substance.*
>
> *Changes: We have deleted this statement.*