

Interactive comment on “Intertidal finger bars at El Puntal, Bay of Santander, Spain: observation and forcing analysis” by E. Pellón et al.

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Revision of the manuscript “Intertidal finger bars at El Puntal, Bay of Santander, Spain: observation and forcing analysis”

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REFEREE COMMENTS

GENERAL COMMENTS

The manuscript describes the characteristics and dynamics of a system of small-scale finger bars at the swell-protected side of El Puntal (Bay of Santander, Spain). They use

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2 years of video-images to quantify their wavelength (mean value, 26 m), orientation (mean angle, 26 deg, down-current oriented with respect to the wind-driven currents), amplitude (10-20 cm) and migration rate (mean value 6 cm/day, in the eastward direction). Subsequently, they analyse the different forcings that can influence the finger bar migration. The main candidate to explain the eastward migration is the wind (through both the wind-driven current and the current created by the wind-generated waves).

This new set of finger bar data is very interesting because there are very few finger bar data sets with such detailed measurements, especially in low-energy intertidal environments. Thereby, the contribution of this paper to the experimental nearshore research is significant and the scientific contents are well within the scope of the journal Earth Surface Dynamics. The approach and methodology are in general valid (with the exceptions written in the next paragraph and in the Specific comments) and the results are discussed in an appropriate way.

My only concern is related with the forcing analysis (section 4). On the one hand, to my opinion, this part of the paper has a limited relevance in the sense that it only contains a statistical correlation of different forcing indicators with the migration rates. Before reading the paper, and given the background of the authors, I was expecting to find a more relevant forcing analysis, including an application of a morphodynamic model to this site. Applying such a model with the different forcings included would allow them to determine in a more quantitative way the importance of each forcing. On the other hand, section 4 is quite long compared with section 3, which contains the description of the finger bar characteristics, more interesting and relevant to my opinion. I can understand that the authors consider that applying a model is beyond the scope of this paper, but then I recommend that the less relevant forcing analysis they perform is not explained in such a detailed way (see the Specific comment SC8). On the other hand, the authors could describe in some more detail the methodology they use to do the measurements and the characteristics of the bars they measure (sections 2 and 3). I have not written any specific comment on this latter issue but my impression is that

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some parts of section 2 and 3 are explained in a very synthetic way.

With my English knowledge (somehow limited because it is not my native language), I consider that the use of English language in the paper could be improved. I have written some specific recommendations in the Specific comments (e.g., Specific comment SC6) and some corrections in the Technical corrections part, but the text should be throughout revised to correct other potential mistakes.

More details and other recommendations have been included in the Specific comments and Technical corrections below. I recommend publication in the journal *Earth Surface Dynamics* after the Specific comments below have been considered.

SPECIFIC COMMENTS

SC1: Page 1, lines 15-17. Page 5, lines 18-19. In these two places of the manuscript, the authors describe the finger bars as being up-current oriented with respect to the ebb-flow. I know this is true but I find very confusing to describe the orientation with respect to the ebb flow because later on in the paper they show that the main candidate to explain the eastward migration is the wind (with the tide playing a secondary role). Thereby, the finger-bar orientation could also be described as down-current oriented with respect to the dominant sand transport (eastward directed). In fact, in lines 14-15 of page 3, the authors describe finger bar orientation of previously reported small scale finger bars as down-current oriented with respect to the dominant sand transport (and the authors show that this also occurs in their case). Given the importance of finger bar orientation, I find very confusing that the authors mention twice that El Puntal bars are up-current oriented (instead of down-current oriented) because they compare this orientation with the weak ebb flow. Summing up, in the Abstract I recommend to write that finger bars are down-current oriented with respect to the dominant sand transport. And in page 5 I recommend just to write that finger bars are oriented towards South-East.

SC2: Page 6, lines 15-21. (Page 9, line 3.) How the apparent periodic movement has

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been corrected? The authors should mention here the methodology they have used to correct this. Is the piecewise regression, mentioned in Page 9 line 3, used to correct this oscillation problem?

SC3: Page 6, line 24. The authors should explain how they measure the wavelength and migration rate.

SC4: Page 7, lines 3-14. This paragraph describes a methodology, not results. I recommend moving it to the end of section 2.2. Moreover, I wonder why the authors only reconstructed one bathymetry, given that they have 2 years of data. Results of amplitude should be more robust if they measure amplitudes at least twice during the data set. Another option is to do a topographic measurement with a GPS to verify the results of the video-derived bathymetry. I recommend at least to add a comment on this. Finally, a minor comment. The sentence starting with "After this, the shoreline..." could be in a new paragraph because from there to the end of the paragraph the specific methodological steps to reconstruct the bathymetry from the images are explained. Also, I would change "After this, the shoreline..." by "The shoreline...".

SC5: Page 7, line 24. What are the oscillations visible in Fig. 6? Are these the oscillations mentioned in SC2? Or they are physical? (e.g., due to tidal forcing...) A discussion on this should be added.

SC6: Page 8, line 4. Page 9, line 2-12. The text in these lines should be improved, I found it difficult to understand. I write here some recommendations to make the text more understandable (check also the Technical corrections below for Page 9).
a) Firstly, the titles of sections 3.2.1 and 3.2.2 could describe in a more precise way the contents of the sections. My recommendation: 3.2.1 Mean motion → 3.2.1 Time-averaged characteristics 3.2.2 Time evolution → 3.2.2 Time-dependent migration rates
b) Moreover, the first sentence of section 3.2.2 (line 3) is also misleading to my opinion. I do not understand the meaning of "detect the main motion". Does it mean "find the time-dependent migration rates"? Also, please mention it here in case the piecewise

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regression is used in order to correct the oscillation problem discussed in Page 6, lines 15-21 (see SC2). c) You could explain in more detail how you set up the segment length, the explanation is very short. Please, define T_k and V_k in more detail. Also, it took me a while to understand equation (1). The sentence in lines 10-11 could be improved in the following way to facilitate such understanding: "Considering that, at a time t , N segments are obtained (for all the bars of the system), the migration velocity of the bar system V_m (Fig. 8) is computed as" → "Considering that, at a time t , N segments are obtained (for the N bars of the system), the time-dependent migration velocity of the bar system V_m (which is the average of the velocities of the N bars) is computed as" d) The reference to Fig. 8 should be moved to the end of the sentence in line 13. "...constant along the time (Fig. 8)" e) Finally, lines 3-12 describe a methodology, not results. I recommend moving them to the end of section 2.2.

SC7: Page 9, lines 20-23. The discussion of why they obtain negative V_m in summer 2008 could be more detailed. For instance, there are variables that measure the accuracy of the piecewise regression (e.g., R^2). Could you calculate them in your case? Are they worse in summer 2008? Also, this western migration is not observed for all the bars and for all the positions (as the authors say). So, why all the V_m are negative in summer 2008? Please, clarify more this issue because it creates doubts on the V_m calculations.

SC8: Section 4. As discussed in the General comments, to my opinion this part of the paper has a limited relevance in the sense that it only contains a statistical correlation of different forcing indicators with the migration rates. Before reading the paper, and given the background of the authors, I was expecting to find a more relevant forcing analysis, including an application of a morphodynamic model to this site (for instance, the model by Garnier, R., Calvete, D., Falqués, A., and Caballeria, M., Generation and nonlinear evolution of shore-oblique/transverse sand bars. *J. Fluid Mech.*, 567, 327-360, 2006). Applying such a model with the different forcings included would allow to determine in a more quantitative way the importance of each forcing.

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I can understand that the authors consider this to be beyond the scope of this paper, but then I recommend that the less relevant forcing analysis they perform is not explained in such a detailed way. In particular, it is quite long compared with section 3, which contains the description of the finger bar characteristics, more interesting and relevant to my opinion.

A list of possible changes to shorten section 4 is: a) Is it section 4.2 necessary given that there is a section 4.3? I mean, is it really necessary to first compare with indicators of wave forcing and wind forcing, if later on they also compare with the sediment transport generated by waves and wind? b) In case they still keep section 4.2: I find unnecessary to relate bar dynamics with four indicators of the wave forcing, which are very similar (P , P_y , S_{xy} and S_y). The authors could choose only one indicator of the wave forcing (the one they find most appropriate) to compare with their measured velocities. This would lead to one panel less in Fig. 10 and two panels less in Fig. 11. At the end of the section they can mention that other indicators give similar results and reason why they have used the one they have used. c) Section 4.2: In case they finally use the quantity $S_y = S_{xy}/X_b$ (which is the one I find most appropriate to compare with T_y), they should reason why they define it in this way (page 12, lines 18-21). For instance, they can reason that it is an approximation for the term $\partial S_{xy} / \partial y$ in the alongshore momentum balance equation, term that is equivalent to T_y in the same equation.

SC9: Page 12, lines 5-7. Page 12, lines 26-28. Page 14, lines 9-11. Page 15, lines 9-10. The authors discuss in detail the behaviour of the migration rates and the quantities chosen to compare with them during the winters. However, they do not mention the autumns, even though the migration rates were higher during autumns... I recommend to add the autumns to the discussion in these lines.

SC10: Page 14, line 14. Page 15, lines 13 and 20. Table 2. In order to know the relevance of the correlations obtained, I recommend to quantify the statistical significance (not significant / significant / highly significant) of the R^2 values obtained, and to write

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it next to the correlation coefficients R^2 in the text and in Table 2.

SC11: Page 14, lines 22-29. Page 15, lines 1-4. Page 15, line 14-22. I recommend to include all the formulas used to evaluate the sediment transport q before starting section 4.3.1. In this way, the methods will be first explained and then sections 4.3.1 and 4.3.2 will only contain results. That means, moving equation (9) (with the whole paragraph) just after equation (6). Also, after introducing equation (9) I find very important to explain in detail what is α_t and how do you compute it (Page 15, lines 14-22) because it ends up being the best way to estimate the sediment transport. Also, they could explain better what is $\min(TR)$ and how do they compute it.

TECHNICAL CORRECTIONS

Abstract

Page 1, line 14 (and many other places throughout the manuscript). There must be a space between a number and the corresponding unit. Page 1, lines 25-28. Please, rephrase the sentence (I do not understand it): "The astronomical tide seems to play an important role in the bar dynamics, as the tidal range conditions the mean (daily) fetch and also the time of exposure of the bars to the marine dynamics." Do you mean the following? "The astronomical tide seems to be important for bar dynamics because the tidal range conditions and the mean (daily) fetch control the time of exposure of the bars to the surf zone waves and currents."

Section 1

Page 2, line 6. as \rightarrow since Page 2, line 10. this is \rightarrow these are Page 2, line 13. magacusp \rightarrow megacusp Page 2, line 22. as \rightarrow because Page 2, line 23. off-shore \rightarrow offshore

Page 3, line 5. I recommend adding a third (very recent) example of finger bars in intermediate morphological beaches states (at the Gold Coast, Australia): ten Doeschate, A., Ribas, F., de Swart, H., Ruessink, G., and Calvete, D., Observations and modeling

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of transverse finger bars, Proc. Coastal Dynamics 2013, paper No. 208, 2013. Page 3, line 9. I recommend changing this reference to "Ribas and Kroon, 2007", because the latter is the experimental data set where the finger bar orientation is measured. Page 3, line 11 (and other places throughout the manuscript). wave length \rightarrow wavelength Page 3, line 15. the sand transport \rightarrow the dominant sand transport Page 3, line 15. that is opposite \rightarrow , which is opposite Page 3, line 17 – Page 4, line 2.

In this paragraph, I think the authors should distinguish between the studies about finger bar formation (low-amplitude features, i.e., Ribas et al. (2003); Ribas et al. (2012)) and the studies of finite-amplitude behaviour (Garnier et al. 2006; 2012; and the experimental papers).

Page 3, line 24. of intermediate beach \rightarrow of an intermediate beach Page 3, line 26. as \rightarrow since Page 3, line 27. the forcing acting on their dynamics is difficult to determine as, \rightarrow it is difficult to determine the forcing acting on their dynamics because, Page 3, line 29. there is no continuous survey \rightarrow there are no continuous surveys

Page 4, line 29. Section 5 is the conclusion \rightarrow The conclusions are listed in section 5.

Section 2

Page 5, line 20., the western \rightarrow , whilst the western Page 5, line 23 (and other places throughout the manuscript). 1.5% \rightarrow 0.015

Page 6, line 1. set \rightarrow set up

Section 3

Page 7, line 7. one day \rightarrow a day Page 7, line 9. taking into account that it has \rightarrow and it has Page 7, line 31 - Page 8, line 1. Remarkably, for the whole study period, an episode of merging of two bars into one has been detected on 28 March 2009 \rightarrow During the study period, only one episode of merging of two bars into one has been detected (on 28 March 2009, bars 5-6)

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Page 8, line 13. degreasing → decreasing

Page 9, line 5. set → set up Page 9, line 7. signal → bar signal Page 9, line 7. such that → and Page 9, line 7. length → segment length

Section 4

Page 10, line 11. will condition → impose Page 10, line 11. during the bars → during which the bars Page 10, line 12. immersed → submerged Page 10, line 12. neap → spring Page 10, line 13. spring → neap

Page 12, line 2. specify which are “these variables” (use the symbol of the variables)

Page 12, line 9. specify which are “these variables” (use the symbol of the variables)

Page 12, line 10. what do you mean with “same difference”? Page 12, lines 9-13. I do not understand these sentences. Please rephrase them, explaining in more detail.

Page 12, line 14. now → also Page 12, line 27. according to the results of → in accordance with the results of

Page 13, line 11. I would introduce a new paragraph before “Here, we assume...” because now you start describing the specific formulas you will use (see SC11, where I suggest to add more formulas here) Page 13, line 22. The correlation can → In this specific case, the correlation can Page 13, line 23. the alongshore current magnitude → the alongshore current magnitude instead of q (equation 6) Page 13, line 24. The first part of the analysis can be done by analysing V_{wind} and V_{wave} , by separately, → Firstly, we analyse the correlation of bar migration with V_{wind} and V_{wave} , by separate,

Page 14, line 13. shows → show Page 14, line 15-16. Please rephrase the text “and by considering the total transport (Eq. 6)”. I recommend to write this information in a new sentence and explain better what do you mean (it is not total transport but total current), referring again to Table 6 (because this quantity is not in Figure 12). Page 14, line 23. stirring function → stirring function in equation (6)

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Page 15, line 7. What “previous result”? Page 15, line 13. neglected → neglecting Page 15, line 1-2. Write the relation between U_{wind} in eq. (10) and V_{wind} in eq. (6). If they are the same, then use the same symbol

Page 16, line 5. 1.5% → 0.015 Page 16, line 22. Remarkably, an → An

Tables and figures

Table 1, contents. Ribas and Kroon (2012) → Ribas and Kroon (2007) Change the symbols 1, 2, 3 used for the footnotes by symbols like *, o, x (so that you do not get things like 5² in the first row of the table)

Table 2, caption. Results of the correlation → Correlation coefficients (R^2) in this study

Figure 5, caption. reconstruction → reconstruction with videoed shoreline positions during rising tide

Figure 8, caption. Migration speed → Time-varying migration rate

Please also note the supplement to this comment:

<http://www.earth-surf-dynam-discuss.net/1/C543/2014/esurfd-1-C543-2014-supplement.pdf>

Interactive comment on Earth Surf. Dynam. Discuss., 1, 673, 2013.

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