

Interactive comment on “Morphological coupling in a double sandbar system” by T. D. Price et al.

T. D. Price et al.

t.d.price@vu.nl

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We thank the associate editor and the two anonymous reviewers for their time and their useful, constructive comments on the manuscript. We hope this report sets all concerns at rest.

Associate editor’s comments

(Associate editor’s comment; *our answer*)

This is a very well-written manuscript that will be of interest to the many research groups working on nearshore sandbars. [...] However, both reviewers sense that a bit

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more should be done so that the manuscript can provide a broader view of the state of the art for this interesting problem. After my own reading of the manuscript I can only concur with the judgment of the reviewers: some more work is needed to have a more complete contribution. However, most of the literature is not quite discussed in the introduction or discussion (for example many works are discussed only quickly mentioned at the beginning of section 3.2), and the discussion could certainly be a lot more detailed and insightful. [...]

We would like to thank the associate editor for his generally positive feedback. We agree with the general comment of the AE as well as the reviewers that the results in the paper need to be discussed further in the light of other literature to make it a true review paper. We have now rewritten the manuscript such that the literature is better discussed throughout the paper, providing a broader view on the topic. We will follow the reviewers' comments to elaborate on our adaptations below.

Anonymous Referee #1

(Reviewer's comment; *our answer*)

The manuscript covers some excellent work that has been done by the authors and is published elsewhere. As a stand-alone manuscript it does not present anything new, but gathers work from a few papers (4-10 depending on how you count them). In this context it should be considered a literature review. As a literature review, however, I am not sure that it covers things quite broadly enough. There are brief mentions of older foundational literature and more recent studies (that are not their own), but only brief mentions of those. In the abstract, indeed it does say that this is a presentation of "our most recent findings". However, I am not sure that a review of your own work constitutes a publishable "literature review" paper.

We thank the reviewer for his/her kind remarks and constructive comments. We have now rewritten the manuscript such that the existing literature is better discussed throughout the paper. As such, our own results are not presented as "new", but instead they are used to illustrate the latest knowledge on morphological coupling between sandbars. In the abstract, and at the end of the introduction, we now emphasise that we include our own findings in the context of a review of the state of the art of finite-amplitude behaviour, with a focus on morphological coupling in double sandbar systems.

I think that the word review should be in the title. The reader should know they are picking up a review paper from the start, rather than some new research by the authors.

We have adapted the title and it now explicitly includes the word review: "Morphological coupling in multiple sandbar systems - a review".

The organization is a little strange to me. Differentiating the "variability" and the "coupling" seemed like an unnatural separation. I guess I see now that I have read it a couple times that this is really a function of different studies that have been done or the steps that the authors took to examine double bar morphodynamics. Perhaps a reorganization that treats the whole subject more like a short history of bars, double bars and then morphological coupling (which indeed is the most complex idea and the most recent work). That is just a suggestion for the organization, but I think a more complete review is in order in general.

In our revised manuscript, we give a concise introduction on sandbar research in Section 1, with an emphasis on multiple sandbar systems. This provides the framework for the following sections. Subsequently, we discuss observations of morphological cou-

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pling and the processes related to these observations, based on numerical modelling.

The authors argue for presenting observations from only one site. I think that this would make sense if this were an independent study, with a specific analysis (like the barline coupling analysis in Price and Ruessink 2013). However, for a more complete review I would argue that inclusion of more sites would be valuable. Perhaps the problem is the most recent analyses that they have done (coupling), haven't yet been completed (published) for all sites (Duck? Nordwijk?). Assuming you are looking at coupling on other beaches :-), maybe wait on this review until that is out? Maybe other beaches are not as strongly coupled? Why (see below)?

Although we have observed single instances of similar coupled morphology and coupling behaviour on the Aquitanian coast (France), Duck beach (USA) and Egmond (The Netherlands), we use our extensive analyses of the Gold Coast (Australia) to characterise the types of coupled sandbar morphodynamics. We agree that similar analyses at those other beaches would be a useful and significant contribution, by shedding light on the coupling statistics at other sites, as mentioned in Section 4. Although we now have the tools and analysis techniques available, this work has not (yet) been done. In the revised manuscript, we now explicitly mention this towards the end of Section 3.1. There, we also include an example from Duck beach (NC, USA), showing an observation of an ldt coupling type and the corresponding wave height and angle of incidence preceding its development (Figure 9).

Price and Ruessink (2013) found that two bars were coupled about 40% of the time (at the Gold Coast). I wonder what happens the other 60% of the time. Is the correlation lower, but still coupled? Are the bars completely uncoupled? I think that in a strictly scientific paper it is reasonable to carve out certain circumstances or conditions, but I feel that a review paper needs to address all conditions. Have you guys looked at this yet? Why sometimes coupled and other times not? I would think that incoming

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wave energy would always have to come over that outer bar so it would always have an effect, no? Maybe this (40% versus 60%) is wave energy or angle dependent.

At the Gold Coast, the sandbars were significantly coupled 40% of the time at the 98% confidence level, based on cross-correlation results between their barlines (derived from the time-exposure images). The other 60% of the time the coupling was thus statistically insignificant. We also used different significance levels and applied continuous cross-wavelet transforms of the barlines (which take the variability in the alongshore patterns into account; see Ruessink et al., 2007), but these gave similar results. In many cases, uncoupled morphologies contained a more-or-less straight outer bar (LBT or weakly developed RBB). The depth variations along the outer bar are presumably too small to affect the inner-bar flow field and morphology. Although the forcing may be identical for events leading to coupled and uncoupled sandbars, the initial morphology of either sandbar may differ. As the morphology influences the flow field, and therefore the sediment transport, an identical forcing over a different initial bathymetry may lead to completely different morphological responses of the bar system. Moreover, the morphology needs time to adapt to a change in offshore forcing, and this adaptation time may differ for the inner and outer bars. As such, a 1:1 prediction when and how bars coupled based on wave conditions alone is not possible. This is comparable to many unsuccessful attempts to predict the bar states in the Wright and Short (1984) model from wave conditions only.

Maybe a flow chart would help clarify things, for example outer bar straightening (33% of the time) → decoupling (60% of the time) versus outer bar variability (66% of the time) → coupled (40% of the time). (That seems a little weird to me.) Put in some wave heights, wave approach angle, up-state/down-state transitions, etc. Try to organize all the factors. Again this is just a suggestion for organizing things for a more complete literature review. Your Fig 7 is a nice conceptual model, but it is only for one branch of that big bar morphodynamics tree.

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Such a chart, summarising the factors that influence alongshore variability and coupled morphodynamics as well as the statistics involved, could provide a useful summary of general sandbar morphodynamics (if done well). For this paper, however, we focus on morphological coupling. We believe that Figure 7 (Figure 8 in the revised manuscript) provides a clear conceptual model of the factors and processes involved in the development of the different coupling types. In Price and Ruessink (2011) we provide an overview of state transitions in relation to the wave conditions and in Price and Ruessink (2013) we provide an overview of the statistics of sandbar coupling at the Gold Coast, as well as the conceptual figure of the morphodynamics related to sandbar coupling presented in the current manuscript.

Toward the end of section 3, the discussion of coupling, you are sometimes comparing single bar or outer bar observations with inner bar model results. In general, I think that your results are better than you say. For example, page 14 line 11, you are comparing Fig 3 (outer bar observations) with Fig 9 (inner bar model results). I expect that refraction over the outer bar (among all the other changes to the wave field) reduces the wave angle, so inner bar and outer bar can't really be compared (without considering that).

In this part, the growth and decay of rip channels at both bars in relation to the angle of wave incidence is compared to similar behaviour in single bar systems. It is true that the angle of wave incidence reduces over the outer bar and that smaller angles of wave incidence can be expected at the inner bar. On page 14, line 11, we intended to compare the outer bar observations from Figure 3 with the increase/decrease in alongshore variability in the outer bar in Figure 9c. We now emphasise this by explicitly stating "at the outer bar" in the concerning sentence.

Details (really it is quite well written):

Pg 2 line 2: nowadays – This word is a colloquialism that I don't think is correct for a scientific manuscript. Try just getting rid of the word, the sentence is OK without it.

We have removed "nowadays" from the revised manuscript.

Pg 16 line 25: should be "obliquity of incident waves plays" (there are 3 errors in that one sentence, and none elsewhere! Weird.)

To our knowledge, this sentence does not contain any errors. Although the reviewer's suggestion "obliquity of incident waves plays" could be used instead of "obliquely incident waves play", we opt to use the latter. We generally use this throughout the manuscript.

In conclusion, the work is well written and the science is good. But the science has been presented elsewhere and as a scientific review it is incomplete. I recommend major revision only because I think that rewrite will take some effort, perhaps it should really be somewhere between "minor" and "major" revision. I look forward to reading the more complete literature review in their revised manuscript.

We thank the reviewer for his/her comments. We hope that the revised paper now presents an adequate review on morphological coupling in multiple sandbar systems, and that our answers have set all remaining concerns at rest.

Anonymous Referee #2

(Reviewer's comment; *our answer*)

This manuscript presents a number of findings that have been published previously by the authors and I therefore believe that it has to be evaluated as a review paper instead

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of a research paper. However, as a review I have a mix of feelings. On one side, the authors are the ideal team to make a review on this topic. On the other, the paper limits itself to presenting the results of the authors themselves. The introduction clearly states: "This paper aims to present our recent findings on...". After reading the paper I can confirm that indeed this is so.

In my view, a review article can not be limited to resubmit part of published works, but it should collect all the existing work on the topic and place them in a broader context. Unfortunately, I cannot recommend the publication of the article in the present state since it focus in presenting results of papers published by the authors. However, I want to encourage the authors to resubmit the paper but including a review of the current state of knowledge about systems double bar. For this review paper I have no doubt that this authors are the most suitable and that they can also keep many parts of the current text.

We thank the reviewer for his/her kind remarks. We have now rewritten the manuscript such that the existing literature is better discussed throughout the paper. As such, our own results are not presented as "new", but instead they are used to illustrate the latest knowledge on morphological coupling between sandbars. We believe the manuscript now presents a better review of morphological coupling in multiple sandbar systems.

Title / abstract: It has to be clearly stated that this is a review article. The fact of including "review" in the title may be an editorial matter.

We have adapted the title and it now explicitly includes the word review: "Morphological coupling in multiple sandbar systems - a review".

Section 1: The introduction, witch like the rest of the text is very well written, is too general. I find it better for a paper of crescentic bars that to one focusing on the coupling in double sandbar systems. It is very significant that, without considering the

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last paragraph (summary), in all the introduction only two sentences refers to inner and outer bars, "multiple sysytem bar" appears only once and "coupling" or "double bar" can not be found..

Section 2: Much of the information on this section should appear in an introduction focused on double bar systems.

In the revised version of the manuscript, we have reorganised and rewritten parts of Sections 1 and 2: Section 1 now provides a concise introduction on sandbar research, with an emphasis on multiple sandbar systems. Section 2 now focusses more on the morphology and morphodynamics of double-barred systems.

Section 3.1: This is for me the best part of the whole text. It focuses on double bar systems and makes stress upon the coupling between the two bars. If I find something missing is the little reference that is made to single bar systems. For example, Is there there any relation (from observations or modeling) between single bar systems and double bar systems?

In the last paragraph of Section 3.1, we compare the processes that govern double-barred systems to those of single-barred systems, by discussing the role of self-organisation and a morphological template in both systems, respectively. In the revised manuscript we emphasise this by adding "..., as in single bar systems." after "the combination of both processes leads to stronger variability in the alongshore inner-bar scales, rather than self-organisational processes alone". Furthermore, in the last paragraph of Section 2, we mention that the alongshore variability of the outer bar relates to the offshore wave forcing "(as in a single bar system)".

Section 3.2: Results of numerical modelling are presented in this section. This basically consists of two parts. In the first, the observed coupling patterns are compared to model results. The second focuses on explaining which conditions are more favourable

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for development or destruction of rip channels. I miss an explanation of the role of coupling in these phenomena.

In the last paragraph of Section 3.2 we explain the behaviour of the inner-bar rip channels observed in the model in relation to the morphological evolution of the outer bar; we explain that (p. 1222, lines 1-4, in the original manuscript) "...the inner-bar depth perturbations follow the alongshore migration of the outer-bar horns at first, the straightening of the outer bar reduces the effect of the outer-bar morphological template on the inner-bar flow pattern, inhibiting the further development of inner-bar features...").

On this section I also want to highlight the overuse of "we". It gives the feeling that new results are presented when they are not at any time.

We have checked the entire manuscript for the overuse of "we" and rephrased the sentences concerned to give a more objective point of view, appropriate for a review paper.

I think the authors should devote more effort to link the results with existing literature. For example, to discuss the differences with the single bar systems and give an explanation.

Although no morphological coupling between sandbars is possible in single-barred systems, we now place more emphasis on the comparison between morphodynamics of double-barred systems and single bar systems throughout the revised manuscript. For example, in Section 2, we now highlight that the predominant "...alongshore rhythmicity of the inner terrace contrasts with shore-attached terraces in single-bar systems, which are mostly alongshore uniform." and "As more wave energy reached the inner bar during alongshore uniform outer bar states, the inner bar behaviour more resembled that of a single-barred system, with its frequent separation from the shoreline and the persistent development of rip channels." In Section 3.2, the growth and decay of

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modelled rip channels at the inner bar during coupling is compared to modelling efforts from single bar systems on p.1221, lines 17-20 and p.1222, lines 5-10 (in the original manuscript).

Section 4: In this section the authors provide additional information related to double bar systems which should have been in the introduction.

The introduction has now been rewritten as to provide the general framework for the paper. Although certain sentences have now been adapted or removed from Section 4, we believe that this section provides a relevant discussion of the perspectives for future research.

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