

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

Anonymous Referee #1

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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.

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.

The organization is a little strange to me. Differentiating the “variability” and the “cou-

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pling” 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.

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)?

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 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.

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.

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).

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

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.

Interactive comment on Earth Surf. Dynam. Discuss., 1, 1209, 2013.

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