

Interactive comment on “Environmental signal shredding on sandy coastlines” by Eli D. Lazarus et al.

Anonymous Referee #2

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In the manuscript “Environmental signal shredding on sandy coastlines”, the authors investigate if signals of external forcing can be deciphered from the record of shoreline position. They find evidence of morphodynamic turbulence in a sandy shoreline system, which has implications for both interpreting the record of shoreline change and what information may be extracted from repeat surveys at different scales. The authors should be commended for this investigation of signal processing of coastline data. This study is a unique contribution to the scientific community, with important new insights on interpreting coastline change. I recommend that the manuscript be accepted for publication. Below I detail some unanswered questions about data or assumptions, missing pieces of information, and broader questions for the authors.

1. Explain why using the alongshore average of shoreline change is representative of

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the system (Fig. 2). It's unclear that it is the best metric to use for this spectral analysis, especially with rotational modes being the two primary contributors to shoreline change (Fig.4).

2. What is the source of the data that was used to create figure 3? Should be added to "Setting and dataset" section. Is it from one source or multiple?

3. L143-148: Elaborate on the physical interpretation of the wave energy flux being unorganized and stationary in contrast to the transitional shoreline change. It seems there is a lack of intrinsic characteristic timescale, but why is that important to include?

4. Consider using your wave data (heights and periods) to calculate the expected sediment flux using a variation of the CERC formula. How would this Qs signal compare to the signal produced by +/- values of shoreline change? Since the modes are rotational, it may be informative to use if AST is the majority of sediment fluxes. The modes of shoreline change by Ratliff and Murray suggest that there should be a causative link.

5. L 204 – 213: The authors describe the first two modes as rotational and detail how much change each mode accounts for, but do not describe the types of modes the third and fourth are. Though they represent little change, it would be helpful to include the mode. The magnitudes from the PCA analysis would also be helpful to include.

6. In regime 1 of the spectral power plots in Figure 2 (where the spectral power is a power law function of time scale), is the slope of the power law curve meaningful? Is it different between the data sets? There should be more details in the text of what is/can be quantified out of the power law to lead to the interpretation of morphodynamic turbulence.

7. In Figure 2, the arrows are not explained. I think they are showing that the data from one plot goes into making the next, but it appeared at first that they were pointing to something unexplained. Please clarify the purpose of the arrows. Please clarify the type of data used for each plot. It's difficult to discern as is. Could also be clearer

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by labeling the columns. And it is easy to miss the timescales. A separate plot for timescales would make the connection clearer.

8. Line 247 has a typo – extra “and” in the sentence.

Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-72>, 2018.

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