

Interactive comment on “Statistical modeling of the long-range dependent structure of barrier island framework geology and surface geomorphology” by Bradley A. Weymer et al.

Anonymous Referee #1

Received and published: 20 March 2018

Thank-you for the opportunity to review the manuscript “Statistical modeling of the long-range dependence structure of barrier island framework geology and surface geomorphology” by Weymer, et al. The writing style of this contribution is excellent, the authors should be commended. The manuscript for the most part is clear, coherent and well organized. The research utilizes Electromagnetic Induction (EMI) and GPR data, and topography, to examine the long-range dependence of the framework geology and the geomorphology of Padre Island in the Gulf of Mexico, and interpret the results of ARIMA statistics run on the datasets to investigate the control of framework geology on the island geomorphology. The research continues to build on recent studies that have explored the use of EMI for mapping geology in coastal systems and control of

Printer-friendly version

Discussion paper



framework geology on barrier island geomorphology at Padre Island. This is important research that contributes to the growing body of science on the influence that framework geology exerts on multiple time and space scales of barrier island response and evolution.

The research uses fairly complex statistics not commonly applied in coastal analyses, and the paper would benefit from including examples from other studies in the earth sciences that have used ARIMA approaches for similar applications. Rather than providing a 5-page statistics lesson (that would be more suited for a dissertation), I recommend reducing as much detail as possible and instead provide some real-world examples. This would also help provide justification for adopting these statistics. Why is this approach the best to test the hypothesis?

Although the authors provide a research objectives section, the paper appears to be more exploratory than hypothesis-testing, presents previously established knowledge as new, and there are statements in the early sections that are conclusion statements, giving the appearance of pre-conceived conclusions that drive the interpretation of statistics. For example, Pg 3, lines 69-72; Pg 10, lines 279-281; others as noted in comments in track changes. In addition, the work uses the same EMI data and beach metrics previously used by Wernette et al., 2018, but also includes higher resolution EMI and GPR data. Previous work by Weymer et al, 2016 and Wernette et al (2018) made the argument that EMI can be used to identify framework geology, so the present manuscript doesn't need to make that case and it should not be presented as a new conclusion, rather it can be stated that the findings corroborate the previous work.

The manuscript is a bit long and because the details of the EMI data & collection, and the development of morphologic metrics have already been published (Weymer et al 2015; 2016; Wernette et al, 2018). Much of the detail in those sections can be condensed. This is indicated in the comments on pages 11-14. Condensing the statistics section (suggested above) will also help reduce the length of the paper.

[Printer-friendly version](#)

[Discussion paper](#)



In the Discussion, it gets confusing at times what the paper is about. Is it about the EMI dataset and using it to map framework geology? Is it about the interpretation of the statistical data? Or is it using the combination of the latter to argue how framework geology controls island geomorphology?

There are several statements in the Discussion that this is the first time that EMI data can be interpreted to map framework geology, which has already been established in several recent papers (Weymer et al 2015; 2016; Wernette et al, 2018). The results of the FARIMA analysis are then used to support the findings that framework geology and island geomorphology both exhibit LRD at a regional scale, but less so on smaller scales. How is this finding useful and what might it tell us about the processes shaping barrier evolution. Smaller scales are similarly discussed and it is found that local scale (<10 km) geomorphology is influenced by geologic framework. Does this corroborate with findings at other barrier settings?

The above are some of the major comments on the paper. I have provided an abundance of comments and suggestions in track changes on the e-manuscript. Note that I converted it to a Word document for the purposes of commenting and the formatting is impacted in some parts of the manuscript.

Figure 1: The photo for the southern zone seems more representative of a storm impacted beach and not an example of the typical beach morphology. Figure 2: Please show where the photo & plot in b. are located in a. Figure 3: Highlight the interpretation of the bottom channel in the GPR data. Figure 7: Would be helpful to add what each plot is on the plots themselves (e.g. beach width (bw); beach volume (bv) and so on).

Please also note the supplement to this comment:

<https://www.earth-surf-dynam-discuss.net/esurf-2018-5/esurf-2018-5-RC1-supplement.pdf>

Printer-friendly version

Discussion paper



Interactive comment on Earth Surf. Dynam. Discuss., <https://doi.org/10.5194/esurf-2018-5>, 2018.

ESurfD

Interactive
comment

Printer-friendly version

Discussion paper

