Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-5-AC1, 2018
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Earth Surface Dynamics Discussions
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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.

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Received and published: 22 April 2018

Please find below our point-by-point responses and explanations for how we addressed the comments by the Reviewers on the earlier version of this manuscript. In addition, we uploaded the revised figures (1,2,3,7) and a pdf copy of all tracked changes in the manuscript as well as our responses to each comment/suggestion by Reviewer #1.

REVIEWER #1:

Thank-you for the opportunity to review the manuscript "Statistical modeling of the long-range dependence structure of barrier island framework geology and surface ge-

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Discussion paper

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omorphology" 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 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?

Response:

We agree with the Reviewer that a discussion providing examples of how ARIMA models have been used in the earth sciences is missing from the paper. We removed some of the text and Equation 1 regarding the R/S analysis, as this is described in many places and is not the central statistical approach in the current study. Although we see the Reviewer's point that the statistical methods section is long, for completeness, we choose to leave the detailed explanation of the ARIMA statistics and equations in the paper, so the reader can see the mathematics described here without having to search the literature. However, we removed most of the discussion towards the end of this

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section to reduce the overall length. We also explain later in the Discussion section (via track-changes) that the reason why we chose to use ARIMA is because it is designed to handle both short and long-range correlations that other statistical models do not account for. Prior to the analysis, we did not know whether the series would contain any short-range correlations, thus, this is why we propose that the approach we chose is best to test the hypothesis. We added the following paragraph, which gives specific examples of how ARIMA has been successfully used in the earth sciences (new lines 420-433 in the revised paper).

"ARIMA models are used across a wide range of disciplines in geoscience and have broad applicability for understanding the statistical structure of a given data series as it is related to some physical phenomenon (see Beran, 1992, 1994; Box and Jenkins, 1970; Cimino et al., 1999; Granger and Joyeux, 1980; Hosking, 1981; Taqqu et al., 1995). For example, Cimino et al. (1999) apply R/S analysis, ARIMA, and Neural Network analysis to different geological data sets including; tree ring data, Sr isotope data of Phanerozoic seawater samples, and El Niño phenomenon. The authors show that their statistical approach enables 1) recognition of qualitative changes within a given dataset, 2) evaluation of the scale (in)dependency of increments, 3) characterization of random processes that describe the evolution of the data, and 4) recognition of cycles embedded within the data series. In the soil sciences, Alemi et al. (1988) use ARIMA and Kriging to model the spatial variation of clay-cover thickness of a 78 km2 area in northeast Iran and demonstrate that ARIMA modeling can adequately describe the nature of the spatial variations. ARIMA models have also been used to model periodicity of major extinction events in the geologic past (Kitchell and Pena, 1984)." . . .

Additionally, we added a discussion (new lines 818-826 in the paper) following the Reviewer's comment that the paper would benefit from a discussion of other methods to resolve geologic controls and why FARIMA was best, was chosen.

. . . "To our knowledge, few framework geology studies have specifically used statistical testing to analyze correlations between subsurface geologic features and surface mor-

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phology. Two notable exceptions include Browder and McNinch (2006), and Schupp et al. (2006), both of which used chi-squared testing and cross-correlation analysis to quantify the spatial relationships between offshore bars, gravel beds, and/or paleo-channels at the Outer Banks, NC. Although these techniques are useful for determining spatial correlations between different data sets, they do not provide information about the scale (in)dependencies between the framework geology and surface geomorphology that FARIMA models are better designed to handle."

Although the authors provide a research objectives section, the paper is 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.

Response:

This was a careless mistake and we have made all of the suggested changes by the Reviewer in the paper to reflect that the results in the current study support previous research by this same author group. We also made the changes suggested by the Reviewer regarding the organization of the paper where there were conclusion statements in the Introduction, methods in the Discussion section, etc. Please refer to our specific responses to each comment in the track-changes version of the revised paper.

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

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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.

Response:

We agree with the Reviewer that much of this information can be found in our earlier work. We reduced as much detail as possible from the Methods sections and cited the appropriate studies that explain the methods in more detail (i.e., Weymer et al 2015; 2016; Wernette et al, 2018).

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?

Response:

It is a combination of the latter to argue how the framework geology controls island geomorphology. Please refer to the changes we made in the Discussion, which should clarify these points made by the Reviewer.

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?

Response:

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Following similar comments mentioned above, we removed the statements in the Discussion saying that this is the first time the EMI data can be used to map the framework geology. Although each data series at the regional scale shows similar d-values, the degree of LRD for the EMI spatial series is stronger at local scales within the paleochannel region (refer to Table 3), suggesting that the framework geology controls are more significant at smaller (local) spatial scales < 10 km. These results suggest that the variable framework geology provides a structural control on beach-dune morphology similar to what has been observed on islands with a semi-regular framework geology (e.g. Santa Rosa Island, South Padre Island and Fire Island) (see Wernette et al., 2018).

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.

Please also note the supplement to this comment: https://www.earth-surf-dynam-discuss.net/esurf-2018-5/esurf-2018-5-RC1-supplement.pdf

Response:

Thank you for your detailed comments and suggestions annotated throughout the text. Instead of listing each individual comment/suggestion in the rebuttal, we instead copied the comments from the supplementary pdf and added them to the revised manuscript with track changes. We provide a response to each comment immediately following the copied comment from the Reviewer. Please refer to the track-changes version of the revised manuscript for our responses.

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.

Response:

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The southern zone of PAIS has numerous washover channels, especially within the last $\sim$ 10 km and is largely erosive. We agree with the reviewer's suggestion and modified the figure to show a more representative image of the beach-dune morphology typical of the southern zone of the island. Additionally, we included the approximate locations of where each photo was taken as indicated by the red dots.

Figure 2: Please show where the photo & plot in b. are located in a.

Response:

We highlighted the location of Plot B (white-dotted box) in Figure 2a as shown below in the modified figure.

Figure 3: Highlight the interpretation of the bottom channel in the GPR data.

Response:

We highlighted the interpretation of the bottom channel in yellow (see below).

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).

Response:

We added the description of each dataset directly on the plots (see modified figure below).

REVIEWER #2:

The paper presents a novel tool that utilizes electromagnetic methods to determine the alongshore variability of framework geology in barrier islands. The authors apply this novel approach (EMI geophysical profiling) to Padre Island (Texas), which is mostly in its natural state (except Malaquite beach). The results confirm some previous work by some of the co-authors, which suggests that barrier island change is scale could depend of the underlying geology. In particular the presence of paleo-channels.

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The authors support this result with a statistical analysis that demonstrates scale dependency at the intermediate scales ($\sim$ 30km), which matches the spacing between paleo-channels.

While the results are not surprising (as they confirm previous work by the authors), this manuscript is novel in its ability to integration electromagnetic, statistical, mapping and geomorphological methods. The paper is well written. In my opinion the manuscript is well suited for publication in Earth Surf.

Response:

We thank the Reviewer for their constructive comments. Please refer to our responses to Reviewer #1 that echo similar remarks about our previous work in the study area.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-5/esurf-2018-5-AC1-supplement.pdf

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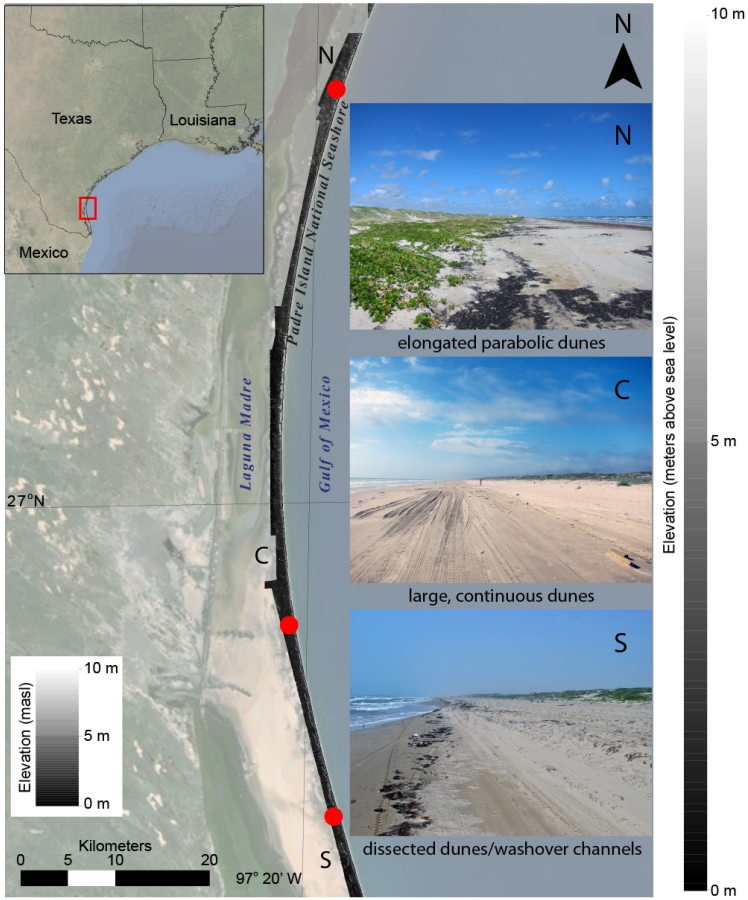

Fig. 1.

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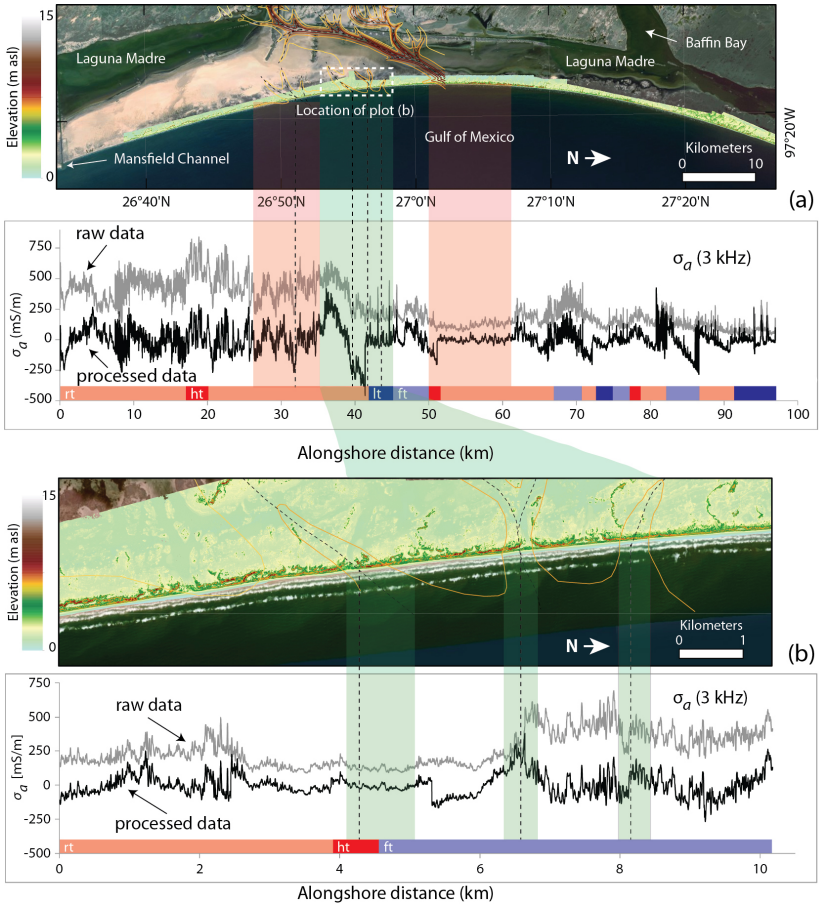

Fig. 2.

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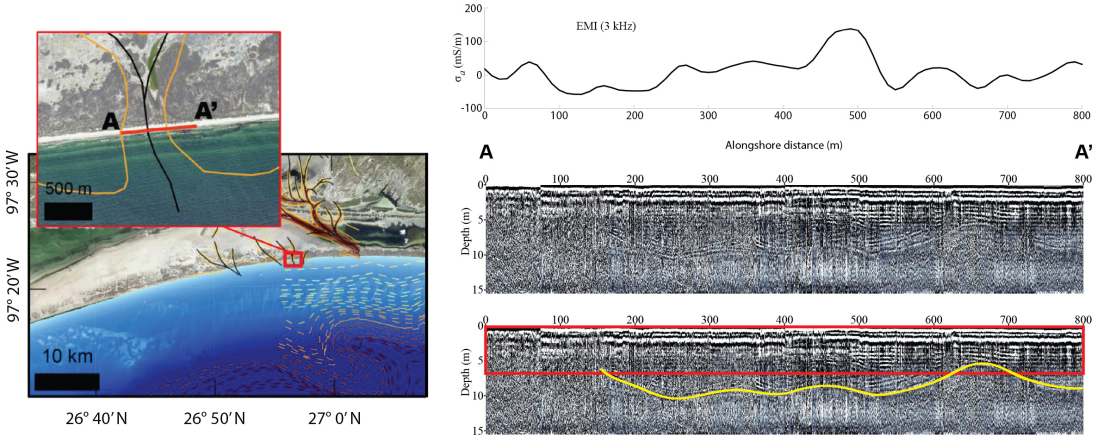

Fig. 3.

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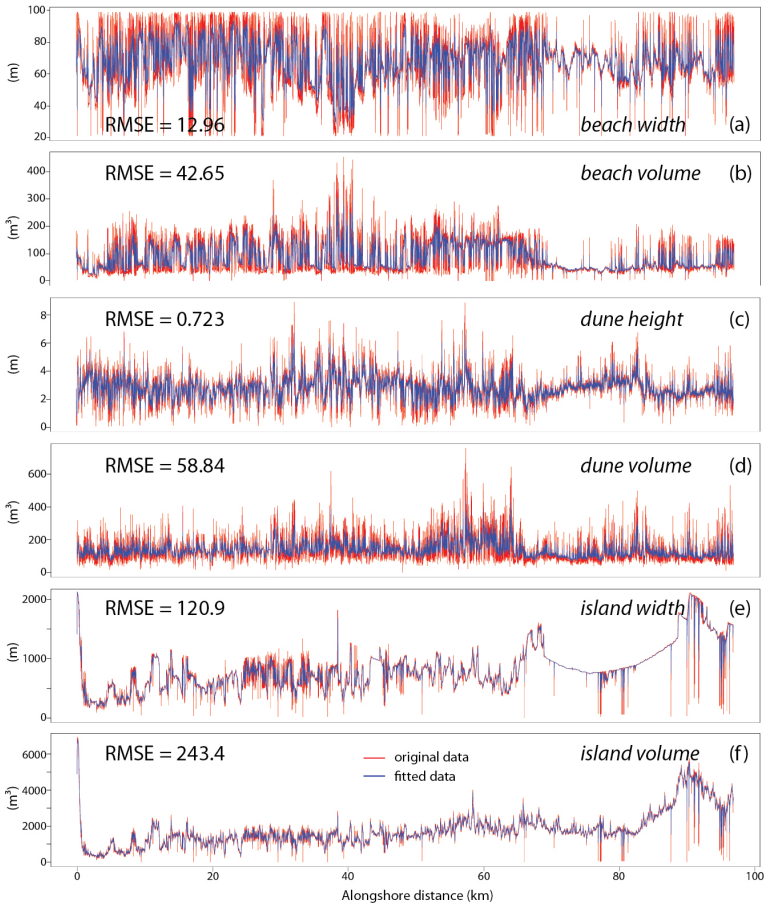

Fig. 4.

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