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DISCUSSION

Discussion of Fenster, M.S.; Dolan, R., and Morton, R.A., 2001. Coastal Storms and Shoreline Change: Signal or Noise? *Journal of Coastal Research*, 17(3), 714–720.

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The subject paper of this reply concludes that (p.718) "... the range of uncertainty for shoreline predictions is *greater* in cases *excluding* storm-influenced data ... compared to cases including such data ..." and goes on to draw other conclusions concerning shoreline behavior based on this claimed result. We disagree with these conclusions, and will show below that the reported findings arise from a flawed analysis of the series of shoreline positions used in the investigation.

The results in the paper by Fenster, Dolan, and Morton (2001) (hereinafter referred to as FDM) are based on an analysis of a shoreline position data series at Hatteras Island, NC, called by them transect 21-13, and shown in their Figures 3A and 3B. The first of these figures purports to show only nonstorm-influenced data used in a linear regression and error analysis, and the second uses all storm- and nonstorm-influenced data for a similar analysis of the transect. Comparison of predicted errors (95% CI) in 2010 from the Figures indicates that subsetting the data to eliminate storm-influenced shoreline positions for determining the underlying long-term trend of shoreline position leads to poorer results than if all of the data are used. In contrast, our analysis of their data shows that the contrary is true (*i.e.,* eliminating storm-influenced shorelines leads to better results) and that FDM reached their conclusion because of their restrictive and incorrect criterion for categorizing shoreline positions as storm- or nonstorm-influenced.

We begin by noting that the data set of 12 shoreline positions used by the authors is only a subset of the 17 dates shown in Table 1. In addition, the table is mislabeled. Its caption begins "Storm-influenced shoreline positions and dates ...", but no position information is given, nor when contacted were the authors able to supply the data in a timely manner for our reanalysis. Fortunately, it was an easy matter to scan Figure 3B, and use a digital image-processing program to calculate the shoreline position values. The accuracy of this process was assured by our attainment of the same results for the trend and R^2 shown in Figure 3B. Having the data in hand, we could proceed with our own analysis of it.

If an analysis is made of storm- and nonstorm-influenced shoreline position data, the results will depend critically on the definition of such data. The authors chose to define storm-influenced shorelines "... as those in which a storm with deep water wave heights $\geq$ 1.8 m had occurred less than two weeks[1] prior to a photogrammetric flight." This is of course equivalent to assuming that any recovery (*i.e.,* accretion) of the shoreline position occurs within two weeks following a storm. It is true that considerable recovery can occur even within a matter of days following a small winter storm. However, a much longer time is required for great storms such as the March 1962 Ash Wednesday nor'easter which devastated many of the U.S. east coast barrier islands (Galgano *et al.,* 1998), or hurricane Alicia, which severely impacted Galveston Island (Morton *et al.,* 1994). In both of these cases, accretion and dune rebuilding was seen for many years subsequent to the storms.

There is another problem with the meteorological definition of a storm-influenced shoreline used by the authors. It is that the erosion impact of a storm also depends on its duration, and the time in the spring-neap tidal cycle the storm occurs. A Storm Erosion Potential Index (SEPI) for nor'easters has been developed by Zhang *et al.* (2001) which clearly illustrates the importance of duration, and water level (*i.e.,* storm tide) at the time of storm occurrence. The Ash Wednesday nor'easter of March 1962 was so damaging in large part because it occurred at a perigean spring tide and lasted through five high tides. Deep water wave height is not

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[1] It is unclear from the paper the criterion actually used by FDM to define a "post-storm" shoreline in terms of time elapsed between the date of the storm and the date of the imagery. The body of the text states "two weeks," the text in Table 1 states "about a week," and a column heading in the same table indicates one month.

Table 1. *Summary statistics for the entire FDM dataset, the non-storm influenced dataset as selected by FDM, and the same non-storm dataset with the December 1962 post-storm shoreline removed.*

	Prediction Uncertainty	R^2	Erosion Rate (m/yr)
Entire dataset of FDM	40m	0.68	0.52
Non-storm influenced dataset as selected by FDM (includes Dec 1962 post-storm shoreline)	45m	0.72	0.48
Non-storm influenced dataset with Dec 1962 post-storm shoreline removed.	30m	0.87	0.41

an adequate means for characterizing the erosion impact of a storm.

The data and error analysis results obtained by us using the data identified by FDM as nonstorm-influenced are shown in Figure 1. Note that this data set includes a shoreline position for December 1962, only nine months after the Ash Wednesday storm. We obtained essentially the same results as FDM from this subset, but point out that 30 meters of *accretion* followed the December 1962 shoreline position. That the shoreline position took an extended time exceeding two weeks to recover from the March 1962 Ash Wednesday nor'easter at the transect location used by FDM in their analysis is obvious from the data and the historical record of this storm. Eliminating the December 1962 shoreline position from the analysis yields the result shown in Figure 2. The prediction uncertainty is substantially (⅓) smaller, and the R^2 is greatly improved. Table 1 summarizes these results. Note that the erosion rate estimate decreases by about 20% when storm shorelines (including the December 1962 post-storm shoreline) are removed, refuting the assertion of FDM that the inclusion of post-storm shorelines results in insignificant differences in the rates of shoreline change. Thus our conclusion (Douglas and Crowell, 2000; Honeycutt *et al.*, 2001) concerning the advantage in using truly nonstorm-influenced shoreline positions to determine the long-term trend of shoreline position is also sustained by this Hatteras Island data set when the data are correctly identified and analyzed.

The ongoing controversy concerning how to forecast shoreline positions and confidence intervals has its origin in the statistical approach used to analyze shoreline position data records. The most common approach is the one used by FDM and others, which is to use regression analysis to find both the nature of the association between variables (shoreline position and time) and the properties of the data. This purely numerical approach, which depends only on the data themselves and has no physics in it, is extensively used in the social sciences in order to uncover relationships. But this approach relies upon several key assumptions which are not always appreciated. These are:

- The "noise" on the data is assumed to be normally distributed, with the variance of Y the same for any X, and the variance is determined as a part of the regression.
- The underlying signal is generated by a process that is linearly dependent on parameters determined as part of the regression process.

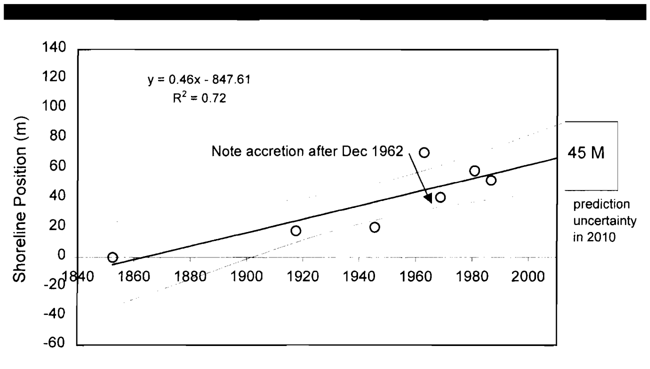

Figure 1. Reanalysis of shoreline position data selected by Fenster *et al.* (2001) as being non-storm influenced. The 30 m of accretion following the December 1962 shoreline position shows that post-storm recovery was not complete at that time.

How closely does a time series of shoreline positions adhere to these assumptions? For U.S. east coast, barrier island, open-ocean shorelines that have not been engineered, or are not influenced by inlets, *etc.*, observational evidence shows that the measurement uncertainty (which includes the variability of the shoreline position indicator) is anything but uniform and normally distributed, and the ordinary linear regression model is at best an approximation to the true physical situation.

Concerning the measurements, there are at least two distinct populations of available shoreline data points: pre-1960s NOS T-sheet shorelines, and post-1940s air photo shorelines (supplemented by more recent GPS ground surveys). Throughout the history of topographic mapping for NOS T-sheets, a primary objective was to avoid mapping storm shorelines (Swainson, 1928) and to "delineate, as near as it was possible to determine, without recourse to leveling, the line of mean high water" (Shalowitz, 1964). Furthermore, most T-sheets were mapped during summer beach conditions. In contrast, available archives of historical coastal aerial pho-

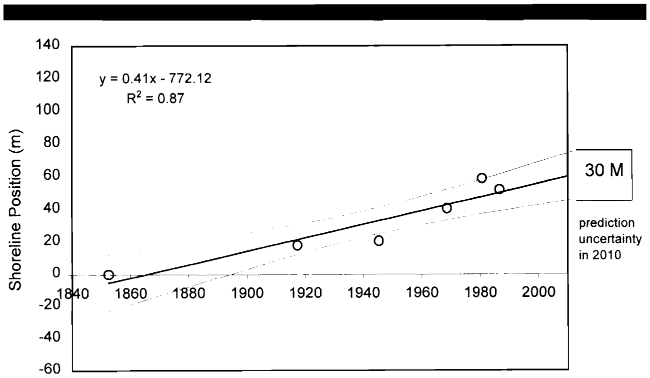

Figure 2. Trend and error analysis with December, 1962 storm shoreline omitted. Note that the R^2 value has improved substantially. The prediction uncertainty (95% CI) has also declined by ⅓ to 30 m.

tography are comprised of photographs taken during any season, and are often biased towards immediate post-storm imagery.

Not only are the observational requirements of ordinary linear regression not met by an uncritically examined set of shoreline positions, but the physical facts about shoreline behavior depart considerably from the simple linear regression model. Consider the following:

- There is an underlying long-term trend of erosion at most sites, but
- There is also a seasonal cycle of shoreline position; the winter beach is much narrower (eroded landward) than the summer beach,
- Winter shoreline positions are much more variable ("noisier") than summer ones because of the effects of storms,
- Severe storms can erode the shoreline in a few days by a greater amount than has occurred in preceding half century, and post-storm accretion after a great storm can go on for more than one year, and
- Episodes of rapid massive accretion followed by gradual erosion (the obverse of the effect of severe storms) are not observed, except in cases of beach nourishment.

Thus a physically realistic model of shoreline change would have to allow for seasonal variations of beach width and observation noise, the occurrence of sudden large erosion events, and post-storm recovery. These effects are not modeled in the linear regression of FDM (and there are not enough data to do so), with the result that large residuals compared to the known accuracy (CROWELL *et al.,* 1991; DANIELS and HUXFORD, 2001) of shoreline position measurements about the trend line derived from the data are obtained. Since the uncertainty of predicted shoreline positions is scaled by the standard deviation of these residuals, large prediction uncertainties result. Using all of the data does have a certain appeal in that defects in the model of shoreline behavior make themselves manifest. Unfortunately, the uncertainties of future shoreline position computed this way are so large that it is not possible to use the predicted positions for policy decisions, nor would it be scientifically prudent to do so. The issue is, can something better be done concerning the long-term trend of shoreline position using our *a priori* knowledge about how barrier island shorelines behave?

A purely numerical low-pass filter algorithm that could accurately extract the lowest-frequency information (*i.e.,* the trend) from such a complex system of shoreline behavior would require a very extensive temporal data set containing many observations per year over the entire record. Lacking such an extensive data set, linear regression as done by FDM only approximates a numerical low-pass filter, but is the best that can be done if no *a priori* information is used in the analysis. However, our knowledge of shoreline behavior is not limited to the time series values alone. If our interest is in the long-term trend of erosion, then the complication of the seasonal cycle of beach width and winter shoreline position variability can be dealt with by using only summertime data. In addition, winter shoreline position data should be avoided because they may be skewed toward brief, eroded positions; it is far more common to take shoreline position data after damaging storms than before. Finally, since there is documented substantial recovery (accretion) after great storms that can take several years or more to conclude, such events can be identified and eliminated from the analysis as in Figure 2, and in the analyses of shoreline position data in DOUGLAS and CROWELL (2000) and HONEYCUTT *et al.* (2001). Note that we are NOT proposing elimination of storm-influenced shorelines in order to (in FDM's words) ". . . increase the linearity of a trend. . .". We conclude that the analysis of FDM, which claims that storm-influenced data points are not physical outliers and that adding such data to a shoreline change analysis reduces uncertainty, is based on an incorrect analysis of their own data and a physically unrealistic definition of what constitutes a storm-influenced shoreline.

We have not suggested in our papers that temporal outliers are valueless and uninteresting. The total ensemble of shoreline positions is an important indicator of what losses can be superimposed on the long-term trend by large and small storms. At the location analyzed by FDM, the long-term erosion loss from 1958–1984 was about 10 meters, but the total shoreline position variation was about four times as much. From an actuarial point of view, a structure near a shoreline has an expected lifetime based on the long-term erosion rate, along with the chance of loss in any given year due to a great storm. Wise coastal construction policy will take both forms of loss into consideration.

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