Nonlin. Processes Geophys. Discuss., 1, C869–C872, 2015
www.nonlin-processes-geophys-discuss.net/1/C869/2015/

Nonlinear Processes in Geophysics
Discussions

Interactive comment on "Evaluation of empirical mode decomposition for quantifying multi-decadal variations in sea level records" *by* D. P. Chambers

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Received and published: 28 January 2015

Thanks for the comments on the paper. Below, I will highlight your important comments, then follow with my response, noting changes made to the paper to provide more information.

Cheers,

Don Chambers

Comment # 1 ========== The first comment is related to the title. A major part of this manuscript deals with the identification of an acceleration in sea level. This should be included in the title.

Reply ===== That's a good suggestion. I've changed the title to:

Evaluation of Empirical Mode Decomposition for Quantifying Multi-Decadal Variations and Acceleration in Sea Level Records

Comment # 2 ========== You should discuss your results with respect to those from figures 3 and 4 in Franzke (2009, http://journals.ametsoc.org/doi/pdf/10.1175/JCLI-D-11-00293.1). Franzke already showed, in a different simulation study, that EMD and wavelet methods perform worse compared to classical approaches such as OLS when searching for a known trend. Your results clearly underpin this finding. Franzke, however, argued that this is only the case if the real signal is known. If there is an exponential trend and you fit a linear, he suggests that EMD is the better choice, since the error bars of a linear OLS will increase exponentially. My personal opinion is a bit different to that. I prefer to apply different linear and nonlinear approaches to search for the real signal rather than using one individual model. I think that this issue still needs further independent investigations: of course not here, but you should discuss this point.

Reply ===== Thanks for pointing me to that paper. I did comment on another Franzke paper (one from 2009), but that was on using EMD on climate indices. I had not found this second paper in my literature review, probably because it dealt with apply EMD to SST data, not sea level. But after reading it, I see your point. I've added some commentary on that paper in both the Introduction and the Conclusions. It's repeated below:

In the Introduction, after discussing fitting quadratic terms to the highest IMF.

"However, Franzke (2011) conducted an experiment of detecting non-linear trends (i.e., an acceleration) to a small suite of 100 simulated temperature time-series, using different methods including ordinary least squares and EMD. The results showed no statistically significant improvement in EMD. In fact, in most tests, ordinary least squares computed a non-linear trend closer to the input signal."

In the conclusions, when discussing the recovery of acceleration:

"Finally, authors have asserted that the acceleration that comes out of an EMD process is more accurate, as they believe the IMFs better separate the high- and low-frequency fluctuations than linear least squares. Their argument assumes that the high-frequency variations and shorter-period non-stationary signals in the original time-series are biasing a quadratic fit to the original data. By eliminating these signals in the EMD process in specific IMFs, they believe the final IMF contains the "true" acceleration plus residual low-frequency variability. Even Fanzke (2011), who demonstrated that EMD was no better than this than ordinary least squares and a parametric model argued that EMD was still better if the trend was non-linear, especially exponential. Our experiments, however, show the opposite. The quadratic fit to the last IMF is either no more accurate than one fit with least squares to the full, unfiltered data set, or, in some cases, is significantly biased."

Comment # 3 ========== You decided to use random noise for the residual signal. However, Dangendorf et al. (2014, http://onlinelibrary.wiley.com/doi/10.1002/2014GL060538/abstract) have shown that the residual signal (after accounting for ENSO variations) in San Francisco is long-term correlated. Did you test whether a different choice of residual noise (i.e. long-term correlated noise, for instance simulated with an ARFIMA model) affects your simulations?

Reply ==== I did test a colored noise model (based on a AR(3) model) in an early experiment, and found the results were not significantly different than using random noise. I chose to use random noise for faster computations and to make it easier to reproduce my results. I make a note of that in the revised manuscript in the introduction.

"We ran another case using a colored noise model that exactly reproduces the autocovariance of the San Francisco tide gauge residuals. The results were nearly identical to the ones shown with the random residuals, so we choose to use random values as

they are faster to compute for the several thousand simulations we plan to run."

Comment # 4 ==========

Case 3: You include an extreme event in terms of monthly means. This is not a storm surge in its classical expression, which is defined as a high frequency event with a duration of a few hours or days. Your extreme event is rather comparable to an anomalous ENSO event connected with larger scale ocean dynamics

Reply ==== True, this is not a storm surge, but the reflection of a storm surge in a monthly average. For example, Tropical Storm Debby in June 2012 caused a storm surge of more than a meter at the tide gauge. This is reflected in the monthly mean for June as the highest June mean value in the record.

I have addressed your point with a slight revision of the sentence:

"Case 3 starts as the baseline model, adds random noise with a standard deviation of 60 mm (representative of the high-frequency variability in San Francisco sea level), then adds an extra 350 mm for January 1956 to represent the signal of a large anomalous high-water event, such as the effect of a large storm surge event on the monthly average, a large flooding event from sustained rainfall, or climatic variations in winds that can cause sustained high water levels."

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 1833, 2014.