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Nonlinear Processes in Geophysics
Discussions
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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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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 ========== However, as I understand it, as a simply mathematical procedure the identified signals do not necessarily match a "real" signal. Like in a classical EOF analysis, it could happen that a "real" signal is actually accounted for by the combination of two or more modes. Is this what is happening when more than one

IMF is correlated with a climate index in case 2? If so, my feeling is that the evaluation of the ability of EMD is not fair.

Reply ===== I agree with the reviewer that the "real" signals are likely spread over multiple modes, but I'm sure the reviewer also knows that many users of these techniques (EOFs and EMDs both) tend to focus on just a single mode and say it's the ENSO, or PDO mode, for example. That is the motivation for this. I have attempted to make this clearer in the revision by adding more verbiage in the introduction and conclusions. Here is the new text. Updated sentences are in [].

Intro

"However, there are some potential pitfalls that we believe have not been fully addressed in previous papers utilizing the method. First and foremost, EMD is a purely mathematical deconstruction of the data, with no regard to intrinsic covariance of the signals or physics. Second, it assumes that IMFs are comprised of fluctuating signals where the magnitude of nearby peaks and troughs are balanced to create a zero mean – an assumption not based on any physical requirement, as real observations can have quite large ranges in magnitudes, especially sea level data affected by climate signals and synoptic storm events. [Thus, it is unlikely that a single IMF from the EMD analysis can represent a real, physical climate variation. Because of the assumption in the method, it is more likely that multiple modes will be needed to quantify the physical climate mode. However, without some a priori knowledge of this mode, how can one know which IMFs to add together? In the worst case, the climate signal could be spread among a large number of modes. Already, several authors have performed an EMD on ENSO indices and argued they have extracted distinct modes of interannual to multi-decadal variability (Wu and Huang, 2004; Franzke, 2009; Pecai et al., 2010); their argument based solely on the fact such modes are extracted during the EMD process, but with no physical explanation for them.]" And in the Conclusions:

"EMD is a quick and relatively easy tool to identify possible multidecadal fluctuations in

a sea level record. [However, we have demonstrated that real climatic non-stationary signals are generally spread among multiple modes. Analyzing a single IMF for climate variability will likely lead to significantly biased interpretations. Thus, we feel that EMD analysis should not be used solely to quantify magnitude and phase of non-stationary climate variations, nor should analysis of climatic signals be based on a single IMF.]"

With these additions to the scope of the paper, we feel our analysis for Case 2 is perfectly justified.

Comment # 2 ========== What I find most important is the significant difference in the acceleration computed from the highest order IMF. In the introduction, the author states that, due to way the last IMF is computed, it is equivalent to a direct quadratic fitting of the time series. However, his results show that the two fittings differ. The author should explain this apparent inconsistency.

Reply ==== The EMD "acceleration" curve is based on a quadractic fit, but only to the final oscillatory IMF as discussed in the Introduction. I've modified it to make this more clear:

"There is also a subtlety in finding the last IMF that is not discussed in the literature. Since the EMD process requires fitting of cubic splines, the last IMF mode that can be calculated has more than one local minima and more than one local maxima, but fewer than four. The only way to get the final IMF shown in most studies, which shows a continuously increasing sea level mode, is to fit a quadratic to the final IMF from the EMD process, and plot the resulting fit. [This is conceptually no different than fitting a quadratic to the original time series, other than the fact it is done to the final mode, which has significant lower variance than the original data. This should] improve the estimate – provided there are no systematic errors or biases in the final IMF that would bias the result."

I have also revised the conclusions to explain what must be happening to result in different, biased accelerations:

"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.] 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. In the experiment with ENSO- and PDO-like oscillations, the acceleration estimated from the final IMF was nearly 100% too large on average. In individual experiments, the error was even more. [This is most likely due to the aliasing behavior of EMD, where some of the high-frequency variance is aliased into the low-frequency modes, as we have demonstrated.]"

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