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Nonlinear Processes in Geophysics
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Interactive Comment

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

Anonymous Referee #3

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Chambers addresses a pressing problem in this manuscript: the performance of the empirical mode decomposition (EMD) with respect to its application on sea level records. EMD has been increasingly applied to sea level data in the recent years. Some authors have used it to look at different modes of variability, while others have used the EMD to extract nonlinear signals with the aim to provide information about a possible acceleration/deceleration in sea level. In this manuscript Chambers clearly demonstrates some important disadvantages of the EMD with respect to such applications that have not been adequately discussed before. The manuscript is very well written and structured and there are only a few minor issues from my point of view. Therefore, I recommend the paper to be published in NPGP if these issues are addressed.

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Interactive Discussion
Discussion Paper

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.

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.

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?

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.

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

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