

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

**Anonymous Referee #2**

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This manuscript assesses the limitations of the Empirical Mode Decomposition (EMD) used to separate long-term variations in a time series. The work has been motivated by a few publications applying such technique to investigate the behaviour of long term (multi-decadal) signals on sea level changes. I think that this type of studies is useful to attract attention on the possible shortcomings of relatively novel and apparently promising techniques before their use is widely accepted and extended.

I have not used myself EMD so I am not familiar with the methodology. 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

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case 2? If so, my feeling is that the evaluation of the ability of EMD is not fair.

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.

Overall, I think the manuscript is organized, well written and useful and therefore I recommend publication, if the clarifications above are accounted for.

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

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