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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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General comments

The author tests the ability of empirical mode decomposition to extract multidecadal variability from sea level records through three different experiment. The author builds a well definite signal, and applies on it the EMD, in such a way that he knows before applying the techniques what modes he need to find. He build a base signal on which add a second signal that change in different experiments. In the first experiment ha adds some sinusoidal signals, in the second two non stationary signals, and in the third an extreme event. In the experiments he observes that the acceleration term obtained trough EMD is underestimated or overestimated. In the third experiment he

observes the presence of not prescribed oscillations.

Saying the writer that there are some cases in literature connected to sea level in which people identify each IMF with a particular physical phenomenon, this paper can be interesting to underline that EMD has to be used with care.

However, some relevant comments about the "case 2" and in general about the non utilization of "noise test" (Wu et al., 2004) are requested. Considering the many questions I have for the author, also about some used procedures, in general the paper doesn't appear written with enough scruple.

Specific comments

1. Pag 1836, line 12
The signal on which you run the EMD is built only trough noise. Explain better this, please. You should also explain why this experiment is interesting for the target of your paper.
2. Pag 1839, line 7
I suggest to insert the value of the correlation of SOI and PDO also before you have worked on them.
3. Pag 1840, line 11
Where will you note that none accurately captures the input seasonal variation?
4. When you compute the correlation between the "best IMF" and the simulated oscillation for the 1000 simulations, it should be interesting for the reader seeing an histogram (for the case 1) to have a better idea of the distribution of this parameter (with also a mean value with error).

5. Pag 1842, line 12
   Please, explein better the following part, it is not clear: “We isolated this signal by looking at the autocorrelation of the remaining IMFs uncorrelated with either PDO or ENSO. The IMF with an autocorrelation greater than 0.9 at a lag of 1 year was selected.“

6. Pag 1836, from line 20
   You cite (Wu et al., 2004) saying what they do in their paper, but I know that they do an other thing. I know that they propose a test useful when you analyze a signal in which is present some noise. The test is useful to identify the IMFs due to noise (“non significative IMFs”), in such a way to not consider them for a physic discussion about the intrinsic oscillations present in the signal. Perhaps do you talk about the work present in the other reference you cite in line 20, or about (Wu et al., 2007)? This confused me because you propose your approach as alternative also to their works, but actually I have a comment exactly on the test of (Wu et al., 2004), in particular I don’t understand why you don’t apply the test (and so I write what follow in 8).

7. You decide to use the random noise to represent high-frequency variability. You chose a noise with a variance to match the variance of the difference between the original data and the model. This signal is actually due both to noise part and some other signal with appreciable characteristic frequencies (one way to appreciate these is, for example, applying the EMD on this). So using the noise to represent this “high frequency variability”, you actually represent only the noise of this (and you should say this).

8. Performing your experiment, in any of 1000 run, I don’t understand why you don’t apply the noise test (Wu et al., 2004), that give you the possibility to isolate, and not consider, the part of signal due to noise (“non significative signal”). I know that clearly in the assumption that you represent the “high frequency signal” with

noise, all the noise is significative (because you insert it!), nevertheless in this way you discuss also about IMFs due to noise. The crucial point of this, is that performing the EMD on a generic real signal you can apply the test and so avoid to consider the part of signal due to noise. The problem of non apply the test could be that, if you find a “problematic” IMF, you are finding a “problem” in a IMF that could be not actually significative (i.e. due to noise), so in a IMF that is actually due to a part of signal that you can avoid to consider. I observe that you don’t discuss about the first IMFs, and usually applying the test you discover that IMFs due to noise are the first but it’s not absolutely a rule; so in any of the 1000 simulations, if you find a “problem” in one IMF, before say that this is a real “problem” you should ascertain that is not due to noise, applying the test.

9. Comment on “Case 2”
   You study if it is possible to reproduce each simulated signal trough one IMF. It should be observed a conceptual difference that exist between case 1 and 2. Actually, in principle, you can reproduce each simulated signal trough one IMF (for each signal) only in the case 1, because sinusoids respond to the definition of a IMF (Huang et al., 1998). In the case 2, instead, because of ENSO/PDO doesn’t respond to IMF’s definition, you know already in principle that you can’t capture this signal trough a single IMF. In principle, you should need at least two IMF (the sum of two IMF doesn’t have to respond to the definition of IMF) to reproduce that signal.

   So a part of the signal of ENSO/PDO is diffused (necessarily) in other IMFs and we can expect this before performing the EMD.

   You say (pag 1843, lines 1-4): “We know of none that find multiple modes that add up to correlate with an ENSO index. Thus, we argue it is more relevant to quantify if EMD can extract physically meaningful climate modes than whether it can extract modes with interannual and multi-decadal variability”. Performing the decomposition with other techniques you obtain different results, clearly we know that each techniques work in a different way. I agree that it's very important that a technique give you modes that have physical meaning. But with EMD, actually mathematically you already know before performing the analysis that you can't obtain this mode in a unique IMF (the same for PDO). After a decomposition, for sure if you retain for some reason that physically this signal is a "unique signal" you have to sum the IMF that give you the signal (clearly if you know already what you want to build, after performing EMD), but EMD can't say this to us (see for example Alberti et al., 2014. NOTE: the citation of this reference should be interesting to give the reader the awareness, although this "critical" paper, that EMD is a delicate tool but useful when used in the right way).

Actually, the fact that a part of the signal of ENSO/PDO is diffused in other IMFs seems an explication of what follow:

- you said that in some cases the correlation of ENSO and the sum of two IMFs is larger than the correlation of ENSO and only one IMFs.
- The amplitudes of the IMFs representative of the ENSO and PDO is less than simulated signal, and in principle this could be actually due to the fact that a part of ENSO and PDO signal is distributed in other IMFs.
- Besides, looking very crudely at fig. 5 seems that the sum of "unsimulated low frequency" IMF and the "PDO IMF" give a good approximation of the total PDO signal, except for first years (regarding this, however, I already said in 5 you that it wasn't clear what you said in pag 1842, line 12).

Considering these observations, what you observe seems coherent with the math of the EMD, and so I have some doubt about this procedure, and I request some comment about.

According to me, it should be interesting to perform the same experiment using, instead of ENSO and PDO signal like simulated signal, some IMFs ("simulated IMFs") obtained performing EMD on an other signal. I suggest to do it. You could

use also the ENSO and PDO to extract and define the "simulated IMFs". I think this procedure should be interesting because in this case, like in case 1, the EMD could actually extract the "simulated IMFs" in a new IMFs from a theoretical point of view.

10. Pag 1844, line 1 (About the case 3)
You say: "By enforcing an unrealistic balance of equal highs and lows, the method creates a low-frequency oscillation that does not exist." However I think that should be necessary comment the result of EMD's application to "case 3" comparing this with "case 3 without add the extreme event". I say this because also in "case 3 without add the extreme event" I expect that you will obtain some oscillation that "does not exist" (no prescribed oscillations), and this should be clarified.

11. Period IMFs
How do you obtain the periods of IMFs? From instantaneous frequency, from values peak-peak or?

Technical corrections

1. Pag 1837, line 7
Before introduce the cases, you should add that you will analyze three cases. After this talk about them.

2. "Data and methods"
I suggest to present the three cases in a more schematic way, to give a more immediate vision to the reader. After presented Y_{base}, you could insert an analytic expressions of the Y_{case_i} (i.e. $Y_{case_i} = Y_{base} + Y_{simulated_i}$). At this point you could use, for example, some subparagraphs, in which present the three cases defining explicit also $Y_{simulated_i}$. (You could use the same division in "Results and

analysis"). I suggest also to insert the analytic expression for the third case using a Dirac's delta to underline that is only one the point in which you insert the extreme value.

3. Pag 1842, line 11
   The sentence:
   "In addition, we found in nearly every case (99 %) the EMD computed an IMF with a periodic signal between the ENSO and the PDO signal."
   should be:
   "In addition, we found in nearly every case (99 %) the EMD computed an IMF with a periodicity between the periodicity of the IMFs designed to describe ENSO and the PDO."

4. Figures
   In figures in which the average periods of IMFs are missed, I suggest to insert them.

5. Pag 1847, line 10
   The title of the follow reference is not correct.
   Huang, N. E., Shen, Z., Long, S. R., Wu, M. C., Shih, E. H., Zheng, Q., Tung, C. C., and Liu, H. H.: The Empirical Mode Decomposition and the Hilbert spectrum for non stationary time series analysis, P. R. Soc. London, 454, 903–995, 1998.

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- Huang, N. E., Shen, Z., Long, S. R., Wu, M. C., Shih, E. H., Zheng, Q., Tung, C. C., and Liu, H. H.: *"The empirical mode decomposition and the Hilbert spectrum for nonlinear and non-stationary time series analysis"*, P. R. Soc. London, 454, 903–995, 1998.

- T. Alberti , F. Lepreti , A. Vecchio , E. Bevacqua , V. Capparelli , and V. Carbone: *"Natural periodicities and Northern Hemisphere-Southern Hemisphere connection of fast temperature changes during the last glacial period: EPICA and NGRIP revisited"*, Clim. Past, 10, 1751-1762, 2014
- Wu, Z. and Huang, N. E.: *"A study of the characteristics of white noise using the empirical mode decomposition method"*, P. R. Soc. London A, 460, 1597–1611, doi:10.1098/rspa.2003.1221, 2004.

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