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Nonlinear Processes in Geophysics
Discussions
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Interactive comment on "Wavelet analysis for non-stationary, non-linear time series" *by* J. A. Schulte

Anonymous Referee #1

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The work "Wavelet analysis for non-stationary, non-linear time series" by J.A. Schulte is devoted to developing methods for wavelet bicoherence estimation with testing for statistical significance and estimating confidence bands. Correspondingly, the author claims five objectives of the work. As illustrative examples, simple mathematical signals are used as well as geophysical data (quasi-biennial oscillation time series).

Overall, the manuscript is clearly written. I regard it as quite correct. The field to which the work belongs (special methods for nonlinear characterization of time series taking into account statistical fluctuations of the estimates and controlling statistical significance of the conclusions) is important in geophysics and interesting for a wider physical audience.

However, I think that the presented results are not sufficiently original and novel to be

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published as a separate paper. They make an impression of relevant, but secondary and quite evident technical peculiarities which should be taken into account when applying the wavelet bicoherence estimation technique to real-world data. In my opinion, the author should either (i) show that these peculiarities are not so evident or (despite their evidence) unexpectedly fruitful or (ii) obtain new useful knowledge about real-world data with the aid of the methods considered. Both of these criteria are not met. Moreover, I stress my impression that the author CONSIDERS the estimation methods rather than SUGGESTS them. Below, I list more concrete and detailed critical remarks considering the objectives claimed in the Introduction one-by-one.

1) Before other comments, I note that almost the same formalism was already suggested and applied in several works. In particular, in Ref. [J.Jamsek et al // PHYSICAL REVIEW E, v. 76, 046221 (2007)] the authors did the same things, except that they did not estimate statistical significance. The latter was just not very important for their problems due to the presence of clearly constant biphase as compared to the periods of varying biphase.

2) Page 1709, lines 4-5. "... the first objective of this paper is to develop significance testing methods for higher-order wavelet analysis to aid physical interpretation of results".

In fact, the author just suggests to generate red-noise (AR(1)) surrogates, estimate wavelet bispectrum from them and compare it with the estimates obtained from the data at hand. This approach is widely used for many significance testing problems, e.g. for the wavelet coherence estimation as the author correctly points out (Jevereyeva et al, 2003; Grinsted et al, 2004). Thus, the author just reminds us here that it is relevant to perfom significance testing when estimating the wavelet bicoherence too (this is evident but it is good to remember about it in practice) and suggests to use a well-known approach for that. Thus, the first objective is achieved before doing any research.

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3) Page 1709, lines 9-10. "... second objective of this paper will be therefore to apply statistical methods controlling false positive detection."

This is also correct that multiple testing should be taken into account. This is relevant here since many values of the wavelet bispectrum are estimated. It is well-known that Bonferroni correction or a bit elaborated Benjamini corrections can be applied. The author just suggests to apply these techniques during the wavelet bicoherence estimation (namely, he prefers Benjamini FDR controlling scheme). No modification of the techniques is needed. Thus, the second objective is also achieved before doing any research.

4) Page 1709, lines 11-14. "The third objective of this paper will be to develop a procedure for calculating confidence intervals corresponding to the sample estimates, which represent a range of plausible values for the sample estimates".

Here, the authors suggests to use a bootstrapping technique with replacement. Taking into account autocorrelations of subsequent wavelet coefficients, it becomes block bootstrapping. It is Ok, but also well-known. Thus, again the authors suggests to use previously known approach.

5) Page 1709, lines 18-20. "Objective four of this paper will address the time interval selection problem. Such an approach has already been adopted in wavelet coherence analysis (Grinsted et al., 2004)."

Again, everything is correct and relevant, but the technique was suggested before for the cross-wavelet analysis. Here, the author just uses it for the wavelet bicoherence analysis. No special research is needed here and no spesial research is in fact performed by the autors concerning this point.

6) Page 1709, line 25. "objective five of this paper will be to introduce a local biphase spectrum".

Time-varying biphase spectrum was already considered e.g. in Ref. [J. Jamsek, A.

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Stefanovska, P. V. E. McClintock, and I. A. Khovanov, Phys. Rev. E 68, 016201 (2003)] where the authors used short-time Fourier transform. Thus, the idea itslef was already applied and the properties of the biphase were discussed with several examples. Here, the author implements the idea with wavelets but the modification is quite obvious (even if it was not applied before). Probably, the author can insist here on that the adaptive smoothing with operators S_scale and S_time used by him (following the work of Grinsted et al, 2004) are very fruitful and make the method especially efficient. However, no investigations of this point are described. The author just describes the idea (quite correct and relevant, but quite evident) and does not show that it gives unexpected (in any way) or especially useful results.

7) The author illustrate the techinque with QBO time series. However, the conclusions made are that the time seires under study is skewed (negative phases are stronger than positive) and asymmetric (trasition from easterlies to westerlies is more rapid than the opposite one). However, this can be seen by eye directly from the time series as he author states himself. Thus, it is not clear what an especially useful knoweldge is given by the suggested technique. That the technique works as expected is not a new knowledge.

8) Throughout the paper, the author often uses such term as "interaction of the components". E.g. page 1718, lines 22-24: "The power at lambda = 14 months therefore partially resulted from the interaction between its primary frequency component and its harmonic". It is not clear what "interaction" is implied here. The use of such a term seems quite vague. I agree that there is a statistical depedency between the phases of the two spectral components. In particular, it can be a result of a static quadratic nonlinearity of the "system under study", i.e. possibly there is a signal with the period of 28 months at the input of "the system under study", then the signal is squared so that the second harmoinc is generated. In this simple picture, no interaction takes place and no separate interacting modes are present. Certainly, other interpretations can be imagined. However, constancy of the biphase cannot be per se an unequivocal sign of

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"interaction" between something and something.

To summarize, the author presented correct and relevant technical peculiarities which should be taken into account when estimating wavelet bicoherence and biphase from time series. However, the current paper seems a reasonable combination of previously known approaches so that it does not deserve a separate publication. Probably, some of the points raised by the author deserve special attention and can be shown to be especially important and not so evident, but this is not reflected in the manuscript. Therefore, I do not recommend the paper for publication.

Interactive comment on Nonlin. Processes Geophys. Discuss., 2, 1705, 2015.