Responses to Reviewer 1

Provided below are responses to reviewer comments, which are highlighted using bold text.

Summary

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 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 realworld 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.

The author is thankful for the detailed comments provided by the reviewers. Both reviewers found the paper to be well-written and without eroor but felt that it was not orginal. No substantial changes have been made to the manuscript besides some additional text to better highlight the research undertaken in the use of the new methodologies. While not any one method presented in the manuscript is a significant original contribution, the synthesis of methods together with small improvements of existing methods represents an original contribution to higher-order wavelet analysis. The literature regarding the subject has primarily focused on its theoretical and geophysical applications and to a lesser extent on the statistical aspects of the subject. This paper represents the first synthesis and detailed discussion of various statistical procedures that should be considered when applying higher-order wavelet analysis. This paper largely follows the overall structure of the well-known works of Grinsted (2004) and Torrence and Compo (1998), which bridged gaps between the signal processing aspects of wavelet analysis and statistical facets of the subject. Indeed, the manuscript has put higher-order wavelet analysis in a statistical framework and bridges that same gap as the aforementioned works. The author has also created the first higher-order wavelet analysis Matlab software package corresponding to the paper, which will be of importance to a broader geophysical community.

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Specific Points

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.

The earlier work of Jamsek et al. (2003) focused on the signal processing aspects of Fourier-based bispectral analysis. The present manuscript represents an improvement from that earlier work in that the author has extended the formalism to wavelet analysis and used statistical hypothesis testing. Also included in the present manuscript are applications of new methods from traditional wavelet analysis to higher-order wavelet analysis. To the author's knowledge, no such up-to-date synthesis currently exists.

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.

While the author agrees that Monte Carlo methods are widely used, their use in higher-wavelet order analysis has received little attention. The author reminds the reader of the use of such methods in wavelet analysis before proceding to more specialized topics later in the manuscript. However, the author agrees that this part of the paper should be not be listed as an objective and therefore the text on Page 1709 Line 4 has been deleted.

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.

Controlling false positive detection represents an important and long-established topic in statistics. Yet, its necessity in wavelet analysis was only first realized years after the influential work of Torrence and Compo (1998) by Maraun and Kurths (2004) and later by Maraun et al. (2007), Schulte et al. (2015), and Schulte (2016). The inclusion of the Benjamini scheme in the manuscript represents an original contribution in that it bridges the gap between higher-order wavelet analysis and statistical hypothesis testing.

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.

The author respectfully disagrees that the bootstrapping method is not novel. To the author's knowledge, confidence interval estimation using the block bootstrapping method has never been applied to autobicoherence spectra. While the method is well known, its application in wavelet analysis is not straightforward. The difficulty of its application arises because the calculation of the autobicoherence spectrum uses wavelet coefficients at the three wavelet scales and the correlation structure of the wavelet coefficients differs at each of the scales. Therefore, a Monte Carlo simulation was conducted to carefully determine the appropriate block length needed to accurately estimate confidence intervals. In the Monte Carlo simulation, autobiocherence spectra of red-noise processes were calculated and the 95% confidence intervals of the autobicoherence estimates were calculated. The width of the confidence interval was computed at each to scale to determine when the confidence interval widths generally are the widest. The block length at which confidence intervals were generally the widest was determined to be the best estimate of the appropriate block length. The Monte Carlo analysis was a lengthy process that required some research. Details of the procedure are now included in the manuscript and are inserted on Page 1723 Line 5.

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.

The use of the smoothing operator to calculate local biphase and autobicoherence represents an improvement from the earlier work of Jamsek (2003) where the less efficient Fourier analysis was used. Moreover, its use links the earlier work of Grinsted (2004) with that of Jamsek et al. (2003), representing an original contribution in higher-order wavelet analysis. A researcher of higher-order wavelet analysis unaware of the work by Grinsted et al. (2004) would find the use of the smoothing operator in this work not so evident, again highlighting the importance of synthesis. The application of the smoothing operator to autobiocherence required some care because autobicoherence is calculated using wavelet coefficients at three different scales. Research was needed to determine precisely how the smoothing operators should be applied. Additionally, statistical significance of the local autobicoherence was addressed in this paper, which was not considered by Jamsek et al. (2003), again representing an original contribution to the field. The theoretical example used in this paper demonstrates the use of the local autobicoherence spectrum and shows how it can measure non-stationary non-linear behavior.

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

Please see response to comment 5.

7) The author illustrate the technique with QBO time series. However, the conclusions made are that the time series under study is skewed (negative phases are stronger than positive) and asymmetric (transition 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 knowledge is given by the suggested technique. That the technique works as expected is not a new knowledge.

The application of higher-order wavelet analysis to QBO time series alone represents an original contribution in that it has never been applied to it. The purpose of using this geophysical example was that nonlinearities in the time series are readily visible, allowing the reader to better connect the methods to a real-world example. This physical example is an important bench mark for future uses of the methods.

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 dependency 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 harmonic 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 "interaction" between something and something.

The author largely agrees with the assessment. The word “interaction” we be replaced by “statistical dependence” or “statistically dependent” where appropriate.

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