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Interactive comment on “Sea surface temperature patterns in Tropical Atlantic: principal component analysis and nonlinear principal component analysis” by S. C. Kenfack et al.

Anonymous Referee #2

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The overall topic of the manuscript, applying non-linear principal component analysis techniques to the observational tropical Atlantic SST record, is certainly interesting and more research needs to be done here to improve our understanding of phenomena as the Atlantic cold tongue and their importance for the West-African monsoon. Furthermore, apparent similarities to the ENSO as its pacific counterpart, for which non-linear time series analysis techniques have been applied very successfully, make it appear very reasonable to apply similar approaches also in the Atlantic. Thus, I would like to encourage the authors to improve on the quality of their manuscript, since both the technique and the overall topic of the manuscript are certainly interesting. However, at its present state I can't recommend a publication.

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Discussion Paper



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First of all, I have to say that the logical structuring and language used do not meet the criteria for a scientific publication. This strongly affects the scientific quality of the manuscript, since it is often hard to follow the author's line of argumentation. I suggest that at least the abstract and the discussion section should be re-written and the authors should seek the help of a native speaker.

Furthermore, I have to admit that the significance of the scientific achievements presented here is not yet sufficient to justify a publication. Even though I'm not an expert on these methods, I understand that the Authors apply well-established methods to a new problem, in this case the Atlantic cold tongue. The method used to derive the NLPCA appears to be identical to Hsieh (2004) (He even also uses the first three PCAs). So I don't see a methodological advancement that would justify a publication.

On the other hand, the climatological relevance of the results found is not sufficiently discussed. Despite the fact, that the NLPCA performs slightly better than the PCA in terms of explained variability, what are major implications for our physical understanding of the system that we couldn't get using conventional techniques (e.g. the non-symmetrical weak and strong ACT). Here are some suggestions for additional analysis and discussion that could be performed: Could NLPCA e.g. be used as a benchmarking tool for climate models to assess their ability in reproducing the ACT variability? Maybe a subset of CMIP5 GCMs could be tested? How does this mode relate to other dominant modes of Atlantic variability, e.g. Atlantic multi-decadal variability? What are implications for the West-African Monsoon?

Additionally, the authors should not only derive seasonal anomalies but should also detrend their SST time series, since a global warming signal is apparent in their time series and not accounting for it makes PCA questionable and will likely have even stronger implications for non-linear techniques. E.g. Fig. 2 EOF 1 is all positive, I would suspect that this is a signature of a global trend. I further suspect that accounting for it will drastically alter the results.

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Major comments:

1 On P. 249 l 18 the authors write: “The strong ACT is more active than the weak one. Unlike in the Pacific Ocean, the spatial variability of this equatorial mode in the Atlantic Ocean which is similar to El Nino/Southern Oscillation (ENSO) in the Pacific is less linear than the latter.”

How is this statement justified? From my understanding, deviations from the PCA eigenvector indicate non-linearity. Comparing Fig. 4 and e.g. Fig. 3 of Hsieh (2004) that is derived for ENSO using an identical method, the U-Shaped NLPCA in the Hsieh paper indicates a more pronounced non-linearity for the ENSO. Please clarify. I even have the feeling that the ACT phenomenon is in fact much more linear than ENSO.

2 Fig. 5: How is the “normalized ACT index” defined? Can’t find it in the manuscript. And please add the time series of EOFa1 for comparison.

3: Fig. 2: Drop this figure. PCA should only be performed on detrended anomalies.

#4: On P. 249 l 25 the authors write: “We observe that the weak and strong ACTs are symmetric but nevertheless the intensities are different. The more active the Angola SST is, the larger is the ACT’s active surface. The reverse is also true!” I don’t understand the meaning of this paragraph. Please clarify. And how does it relate to the statement in the abstract that weak and strong ACT events are not symmetric.

Interactive comment on Nonlin. Processes Geophys. Discuss., 1, 235, 2014.

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