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Interactive comment on “Interannual coherent variability of SSTA and SSHA in the Tropical Indian Ocean” by J. Q. Feng

Anonymous Referee #3

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The author of this study uses singular value decomposition (SVD) and extended associated pattern analysis (EAPA) to examine coherent variations in SSTA and SSHA in the Indian Ocean. Specifically this study has two main components: 1) the author demonstrates the SSTA-SSHA co-variability in the Indian Ocean with a SVD of the cross-covariance matrix of both variables, and 2) he attempts to determine the mechanisms underlying this co-variability using EAPA. The main concern with this paper is the lack of interesting, original results relative to previous studies. Furthermore, the presentation and support of the results provided here are much poorer than those of the other papers. While many of these past studies are mentioned, there is no attempt to establish why the analysis in the present paper represents an improvement or an advancement of knowledge. I recommend that this paper should not be published in its current form, and only with a complete re-scoping of the objectives of this work should

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it be considered for future publication.

The first part of the paper follows the work of Leuliette et al. (1999) very closely. There is nothing original in this portion of the paper, and it is odd that it would represent such a large part of the paper considering the author was aware of and referenced Leuliette et al. (1999). Simply performing the analysis on a smaller region does not make it different without bringing more to the table. In the second part of the paper, as far as I can tell, the author shows that Rossby waves exist and then make inferences stemming from their simple existence. The argument that this then explains the co-variability between SSH and SST is not convincing. Further analysis is needed, including perhaps the incorporation of additional variables/observations, to make this portion of the paper more convincing and interesting.

Specific Comments: 1) References need to be provided for both the SST and SSH datasets. In particular, where was the SSH dataset obtained from? Did the author do his own gridding (not easy to do well) or did it come from a product like AVISO?

2) Why was the choice made to use reanalysis SST? Any why only up to 2008? Over the time period of interest, there are perfectly good satellite-based products available.

3) Section 2, line 9: Why use an ENSO index computed from a reconstructed SST product? At the very least, the index should be computed from the same SST dataset used in the analysis.

4) Section 2, line 12: I don't think the author's description/understanding of SVD and EOF is widely held in the community. EOF analysis does not mean that involves the analysis of only one variable. Also, EOFs can be (and often is) computed using SVD. I don't understand why the author views EOFs and SVD to be two competing or different methods of analysis.

5) While it is good that the author provided references to the EAPA, further details on the method should be included in the paper.

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6) Figures 1 and 2 have no units, which is fine if that is the way the author wishes to show them, but a note should be made in the figure caption. It is more common to put the “physical units” on either the spatial maps or the expansion coefficients, while the other would represent the “loading” of the corresponding pattern of time series.

7) There are many, many grammar and typing errors in this paper. While these can be corrected, at times it takes away from the meaning of the paper. Several times I found that the errors made led to an understanding of the work that the author certainly did not intend. I leave it up to the author to fix these mistakes, as it is beyond the scope of this review to find them all.

Interactive comment on Ocean Sci. Discuss., 9, 1, 2012.

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