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Ocean Science Discussions

Interactive comment on "ENSO components of the Atlantic multidecadal oscillation and their relation to North Atlantic interannual coastal sea level anomalies" *by* J. Park and G. Dusek

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Comments for Anonymous Referee #2

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The manuscript discusses links between ENSO and the AMO and then goes on to speculate on the cause of sea level anomalies on the east coast of the United States. The manuscript is interesting and should be published. However I have some concerns regarding the statistical methods used. The manuscript would be considerably strengthened if the mechanisms which give rise to the sea level anomalies at the various locations were more fully discussed.

Specific comments

1) The ordering of manuscript could be improved. Figure 1 is introduced before the data that is plotted is described.

Please note that in response to Reviewer #1, the manuscript has been reorganized. Specifically: The section (ENSO Modes in the AMOI) has been reorganized to completely define AMOI(ENSO) before it is used elsewhere. The original idea was to present the index data and EOF reconstructed timeseries (figure 1) with a discussion of their relevant similarities, followed by definition of the AMOI(ENSO) modes. This required a deferred definition of the AMOI(ENSO) modes. The reorganized version is now logically consistent and gives a more natural description of the three panels in figure 1.

We acknowledge the reviewers concern, and believe that the changes above partly address this concern. We do note that the first mention of Figure 1 in the manuscript is parenthetical "(as discussed below and illustrated in Fig. 1)", and serves only to motivate the ensuing discussion relating the climate indices to each other. The first time that Figure 1 is directly mentioned in the paper is in section 2 (ENSO modes in the AMOI), and there the data sources are described and referenced.

2) I assume the MEI and AMOI data are monthly this should be stated. It is clearly stated that the sea level data have had the seasonal cycle removed and have been detrended. This is mentioned for the MEI and AMOI but would probably be sensible given the focus on interannual variability. Anyhow this should be clarified.

Thank you for the clarification. It is now explicitly mentioned in section 2 where the data are described that we are dealing with monthly values.

3) page 3677, line 5: I find it difficult to see a visual correlation. Why not just state the correlation.

We acknowledge the reviewers point. The mention of 'a visual correlation' was in-

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tended to motivate the reader based on a qualitative assessment of the data so that the following hypothesis and analysis were consistent with a casual inspection of the data. We agree that without a quantitative assessment, the statement/motivation are too subjective (these statistics are quantified later in the analysis). Therefore, we have removed this statement.

4) I have some concerns about the EOF analysis. How robust is it? Are the EOFs degenerate? This is most likely for the higher modes given the number that are considered. For instance if you shorten the time series by 20 years the you get the same patterns and results in Table 1 (and the paper as a whole).

We agree that concerns of degenerate EOF's are not substantial for the 'lower' modes that we engage in the analysis. We computed the SSA decompositions with different total number of modes (30 – 80) and with varying block sizes (60 to 150 months), the response of the lower modes were robust with no real change.

5) Page 3677, line 23: There is barely a peak at 6 years and it certainly isn't significant, so it shouldn't be discussed.

For context, the sentence in question is referring to figure 2, and reads: "Interesting features include the band of enhanced variability around a period of 10 years, and the smaller, smooth peaks between 3 and 6 years that are similar to features in the MEI spectrum."

We agree that the amplitude of this 'smaller, smooth peak' is of the same scale as the 95% confidence interval for spectral amplitude, and in that sense is not significant (there is a 5% chance that the amplitude of the 'peak' is due to random variance). However, the fact that the same spectral feature (qualified as 'smaller, smooth peaks') is evidenced in both the AMOI and MEI spectrums, implies that this 'smaller, smooth peak' is not just an expression of random variance, but represents an underlying physical process expressed in both Pacific and Atlantic sea surface temperatures.

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6) Figure 3 (and for that matter 4, 7 and 8). The dashed confidence lines look a bit low. Have you accounted for any autocorrelation in the time series. In fact it might be appropriate to state how the confidence limits are arrived at.

We agree that the method for computing confidence intervals with coherence estimates should be referenced. We have added a citation for the method and referenced this in the text (Thompson (1979), Coherence significance levels, J. Atmos. Sci., 36, 2020-2021). As mentioned in the Discussion section, the use of climate indices in relation to raw observations provides increased signal-to-noise and statistical significance.

7) Page 3678, line 27: Display the equation rather than include it inline (for clarity).

This equation has been promoted to a separate line and equation number.

8) Section 3: A map of the tide gauge locations would be useful. It could also be used as part of a schematic that outlines mechanisms.

We agree that this is a useful addition to the paper, and have added a map as figure 5.

9) Page 3681, line 7: There is barely a peak at 3.2 years for Boston. I'm particularly concerned at the confidence limits on Figure 7 as they all appear at the same value, yet the sea level time series are of different lengths.

Given that that the sea level anomaly timeseries are roughly of the same length, the dominant factor in the confidence intervals is the spectral smoothing used in the periodograms. In fact, the confidence intervals are different, but on the scale of figures 7 and 8, indistinguishable. The corresponding confidence levels are listed below:

[1] "Key West Coherence CI 0.135" [1] "Pensacola Coherence CI 0.137" [1] "Charleston Coherence CI 0.134" [1] "Baltimore Coherence CI 0.138" [1] "Portland Coherence CI 0.134" [1] "Boston Coherence CI 0.137"

10) Page 3686, line 27: I think "conclude" is a bit strong here without a direct mechanism, I would prefer "suggest".

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We agree that suggest is more appropriate and have changed the manuscript accordingly.

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