

Interactive comment on “A unified proxy for ENSO and PDO variability since 1650” by S. McGregor et al.

Anonymous Referee #1

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General Comments:

This paper presents a straightforward method for estimating pre instrumental ENSO variability, by decomposing common modes of variability from a range of previously published ENSO reconstructions.

This study depends significantly upon previous work, in particular labour intensive efforts to prepare paleo chronologies for climate reconstructions. The methodology does not represent a significant contribution to scientific progress.

Nevertheless, it is an important contribution to ENSO reconstruction work. The results are interesting, the logic is well thought out, and the work is certainly worthy of publication.

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Specific Comments:

While the manuscript has been well prepared and the logic behind the work is sound, I believe that the authors could include some additional caveats in the text for the reader's benefit.

1. Not all of the reconstructions chosen use widely spaced proxy networks. This may result in a common mode that is more highly weighted to an individual region. The spatial coverage of each individual reconstruction should be stated, along with any implication for the UEP (I understand that the weightings have been assessed).

2. You could argue that the UEP is an uncalibrated reconstruction. However some of the published reconstructions that make up the UEP are calibrated to the 20th century while others are not calibrated at all, or fitted in using different (spatial) methods. This means that the UEP is not strictly uncalibrated, and this has implications for apparent trends in the UEP. For instance Stahle et al (1998) show distinct changes in ENSO variability during the 20th century, to which the authors attach a caveat regarding calibration (which can cause false skill due to noise fitting). The Braganza et al (2009) reconstruction is uncalibrated and shows more modest increases in variability. While the method used here attempts to resolve these issues, they are not entirely accounted for, and this should be noted in the manuscript.

3. I doubt that all of the contributing indices have similar representation of low frequency variability. The contribution to low frequency variability in the UEP, from each reconstruction, is not discussed. Given that this information is provided at the inter-annual timescale, there should be an assessment given for decadal and greater variability.

4. An additional reference should be included in the discussion of natural forcing

Meehl G.A., J.M. Arblaster, K. Matthes, F. Sassi and H. van Loon, 2009: Amplifying the Pacific Climate System Response to a Small 11-Year Solar Cycle Forcing, Science,

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5. It should be noted as a caveat, that formal attribution of climate forcing is not typically done in the way presented. Specifically, one ideally requires a model for attribution. Statistical approaches such as used in this study are very limited. This is particularly true when assessing solar forcing (or greenhouse gas forcing). Unlike volcanic forcing (which is essentially instantaneous and large on climate scales) it is unclear at which point in time the system will respond to solar and gas forcing, or when the radiative forcing *change* is at its largest. I suggest that the reason the solar attribution of previous papers was not validated by this study is that the previous studies made over-reaching attributional statements based on the most simple (erroneous) causal relationships.

6. There is some doubt as to whether the PDO/IPO is a real climate mode or simply a statistical artifact (low frequency component alone). The text should make this clear, in particular remove the term 'manifestation' line 28 Page 2194.

7. Page 2186 Line 20. What is meant by "ENSO indices display significant skewness". Non linearity only within certain sub-samples of the timeseries? This should be clear.

Technical Corrections:

1. Page 2184 Lines 22-23. The collection of existing proxies should not be referred to as a 'network', but rather a 'set'.
2. Page 2184 Lines 28-30. I am not sure that this method resolves the 'dominant component covariability'. Suggest rewording this section.
3. Page 2184 Line 5 correlations between [the] dominant
4. Page 2200 Line 26 can induce [a] statistically
5. Page 2193 Line 24, the range [of] possible..

Interactive comment on Clim. Past Discuss., 5, 2177, 2009.