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Climate of the Past Discussions

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

S. McGregor et al.

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Firstly, we would like to thank anonymous reviewer 1 for their positive and constructive review of our submitted manuscript.

We have listed each of the anonymous reviewer 1's major comments below along with our response to the comment.

Major comment 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).

Response: The location and type of proxies used in each of the ENSO reconstructions is displayed in Figure 2 of the manuscript. However, to make this information clearer we have added a new column to Table 1 which indicates the region the ENSO proxy is derived.

The Unified ENSO Proxy (UEP) defined in this study has roughly equal contributions from 9 of the 10 ENSO input proxies (see Table 2 in the manuscript). Upon analysis, it was found that 7 of the 9 ENSO proxies used in this study include tree ring data from the USA region and of these 7, 2 are solely based on North American tree ring data (Cook 2000; Cook 2008). Note, the remaining two ENSO proxies use coral based information from the Pacific and Indian ocean regions (Evans et al. 2002, Cobb et al. 2003). As suggested by the reviewer, this could bias the output of this analysis towards a particular region, in this case the tele-connected North American region. We do not think this is the case though, because weighting data presented in the original studies for 3 of the remaining five ENSO time series suggests that the North American tree ring data is not the dominant contributor to the resulting reconstruction (Stahle et al. 1998; Evans et al. 2001; Braganza et al. 2009). Rather, the weightings spread over a range of different regions.

However, in light of the reviewers' comment we have now added a sentences " However, we must note that the North American region could be overrepresented as 7 out of the ten input ENSO reconstructions used in this study utilise North American tree ring data in their ENSO reconstruction derivation. This may result in a common mode that is more highly weighted to this ENSO teleconnection region." at the end of the ENSO proxy data section of the revised manuscript.

Major comment 2: You could argue that the UEP is an uncalibrated reconstruction. However some of the published reconstructions that make up the UEP are calibrated

CPD
5, C972–C978, 2009

to the 20th century while others are not calibrated at all, or fitted in using different (spatial) methods. This means that 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.

Response: The reviewer is correct, the UEP is not (nor does it claim to be) a purely uncalibrated reconstruction of ENSO variability of the past as some of the input ENSO proxies have been calibrated. To clarify this point we have added the sentence "Normally, the output of a PCA would not be calibrated to the observations, however, here as several of the input proxies are calibrated the resulting ENSO proxy (the UEP) is not completely uncalibrated to the twentieth century observations." after the first sentence of the second paragraph in the 'Methods' section of the revised manuscript.

However, we do note the temporal variability of the UEP is not solely or heavily reliant on the calibrated input ENSO reconstructions. This is indirectly displayed in two parts of the manuscript, (i) the weightings for the PCA which display almost equal weighting across 9 of the 10 input reconstructions, and (ii) when testing the robustness of the first mode using subsets of the original input reconstruction, which showed the UEP is the dominant signal of the first PC of any subset containing 5 of the original 10 input reconstructions. To emphasize this point in the revised manuscript we have added the bracketed words to the last sentence in paragraph 7 of the 'Methods' section "(Therefore, the UEP is robust regardless of the number of proxies selected (assuming it is 5 or more) and what proxies are selected from the original network (i.e., whether proxies are included that were calibrated to 20th century observations)".

To highlight the role of calibrated input reconstructions on the running variance of the UEP we calculated the 16-year running variance for each of the 252 subset first PCs.

CPD

5, C972–C978, 2009

Correlation coefficients between each of these 252 running variances and the running variance of the UEP were then calculated, where we find all 252 correlation coefficients are statistically significant above the 99% level and larger than 0.83. We have added words to this effect in paragraph 2 of the 'Changes in ENSO Variance' section of the revised manuscript.

Also, just to clarify, the 16-year running variance of the Braganza et al. (2009) reconstruction displays a correlation coefficient of 0.75 when compared to the running variance of the UEP (see Figure 1 of this response document).

Major comment 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 interannual timescale, there should be an assessment given for decadal and greater variability.

Response: Since the low frequency component of the UEP was not generated by a PCA of the low frequency component of the 10 original input proxies we cannot asses the contribution (the PCA weightings) of each input proxy on the low frequency variability. However, we have calculated the correlation coefficients between the multi-decadal UEP and the multi-decadal variability of each of the 10 original input proxies (see Table 6 in the revised manuscript) to give an indication of their relative importance. The results from this analysis are presented at the end of paragraph 2 in the "Multi-decadal Variability" subsection of the revised manuscript.

Major comment 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 Forc-

ing, Science,

Response: Done, please see the third sentence of the first paragraph in the 'The influence of Solar and Volcanic forcing' section of the revised manuscript. The sentence added reads "The study of Meehl et al. (2009) discusses the joint effects of the top-down stratospheric ozone mechanism and the bottom-up coupled air-sea mechanism which cools the equatorial Pacific in response to peaks in solar forcing."

Major comment 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.

Response: The reviewers' point is accepted and we have now added the caveat "We note that the statistical approach used to asses the relationship between solar forcing and ENSO mean state and variance is a good first step, however, it does not account for non-linearities in the climate system response to changes in solar forcing." to the end of the second paragraph in the 'The influence of Solar and Volcanic forcing' section of the revised manuscript.

Major comment 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

CPD
5, C972–C978, 2009

make this clear, in particular remove the term 'manifestation' line 28 Page 2194.

Response: As suggested, (i) the word "manifestation" was changed, it was replaced with "representation"; (ii) a sentence has been added as that discusses the statistical robustness of Pacific Ocean decadal variability. Please see the first two sentences of the second paragraph of the 'Multi-decadal Variability' subsection of the revised manuscript which reads "More recent literature has questioned the statistical robustness of the IPO/PDO as a climate mode, raising the possibility that the PDO/IPO could simply represent low frequency variability of ENSO (Rodgers et al., 2004; Schopf and Burgman, 2006). Regardless of its statistical robustness as a climate mode, it is an intriguing question whether such pronounced multi-decadal variability of the Pacific Ocean existed during the 250-yrs prior to the 20th century."

Major comment 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.

Response: This sentence now reads "Further to this, there are non-linearities within certain sub-samples of ENSO timeseries (i.e., periods in the observational record in which ENSO indices display significant skewness)". Please see sentence 4 of paragraph 3 in the 'Comparison with the instrumental record' section of the revised manuscript.

Technical Corrections

All suggested technical corrections have been carried out in the revised manuscript.

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

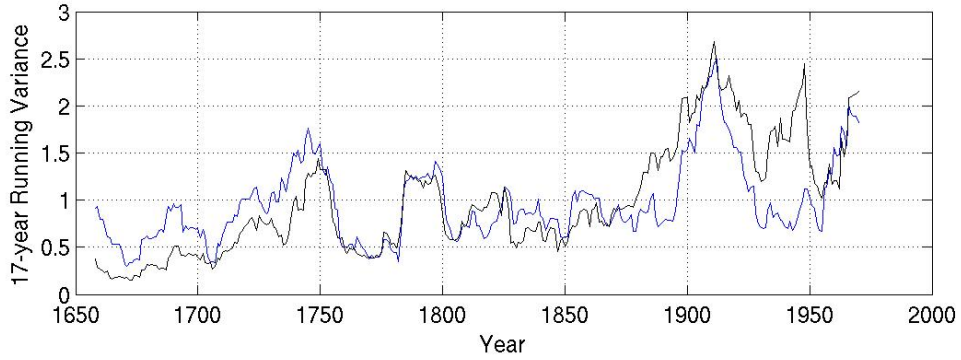

Fig. 1. the 16-year running variance of the normalized UEP (black) and the normalized ENSO reconstruction of Braganza et al. (2009) (blue).

CPD
5, C972–C978, 2009