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Climate of the Past Discussions

Interactive comment on "Stability of ENSO and its tropical Pacific teleconnections over the Last Millennium" *by* S. C. Lewis and A. N. LeGrande

Anonymous Referee #3

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Recommendation: *Accept with major revisions*

This article uses the CMIP5/PMIP3 ensemble to gauge the stability of ENSO teleconnections on decadal to centennial timescales. This is an extremely important topic since many ENSO reconstructions rely on teleconnected proxies as predictors. The paper concludes that because these patterns are variable, one should aggregate records from large spatial areas to offset this local "noise". However, the paper in its current form offers little insight as to why the patterns change, how much of problem that is likely to be for a multi-proxy reconstruction, and whether the cure they propose would work in the face of inevitable dating uncertainties. I articulate those concerns below,

and recommend that the authors thoroughly take them into consideration before resubmission.

1 Main comments

1.1 Scope

- It seems that the goal of the paper is to evaluate the extent to which the potential nonstationarity of teleconnections for proxy-based reconstructions of ENSO. As such, there should be a more extensive review of such work. A non-exhaustive list would be: *Stahle et al.* [1998]; *Braganza et al.* [2009]; *Wilson et al.* [2010]; *McGregor et al.* [2010]; *Emile-Geay et al.* [2013a,b]; *Li et al.* [2011, 2013], few of which are acknowledged here.
- It also would seem natural to pick (at least) one of the networks used above and see how vulnerable they are to the changes in teleconnections identified in the paper, on the context of pseudoproxy experiments [PPEs *Smerdon*, 2011]. One wouldn't have to use fancy reconstruction methods for this: an analysis of the signal-to-noise ratio in the network and how it changes from century to century would be all that is needed.
- On the topic of literature review, the authors should include more on volcanic effects on ENSO [*Timmreck*, 2012, and references therein].
- The section on ENSO characteristics (4.1) would do well to acknowledge the considerable work that has already been done to characterize ENSO in CMIP5/PMIP3 models. In particular *Ault et al.* [2013] showed that piControl simulations are incompatible with a suite of recent reconstructions [*Emile-Geay et al.*, 2013a,b], while forced simulations are compatible, but seem to show a different

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phase relationship to the forcing. Also refer to *Karnauskas et al.* [2012] for a centennial-scale, ENSO-like oscillation that arises internally.

1.2 Mechanisms

The main point of models is the ability to diagnose the causes of climate change. In this case, what makes teleconnections wobble, and is this robust across models? Do we expect the mechanism(s) to be stronger or weaker in nature?

1.3 Statistical Considerations

Reference period It is good that the authors considered 100-year epochs within the past1000 ensemble, but it would have been logical to use a 100-year reference window for the historical or piControl simulations as well. I am surprised that they chose a 40-year span (1976-2006) and wonder how the results would change if they lengthened this reference period. For instance, the authors state "Although ENSO surface temperature anomalies across the Pacific are qualitatively similar, anomalies associated with the historical period (1976–2005) are generally of greater magnitude, particularly at remote locations outside the equatorial Pacific, including over North America and the south Pacific. *These differences in magnitude between the Last Millennium and the historical may relate to the differing boundary conditions during the historical period associated with anthropogenic forcings, such as long-lived greenhouse gases, or simply from the greater diversity of ENSO episodes represented in the longer Last Millennium simulation*." (emphasis mine). They need to rule out that this is not a sampling artifact due to comparing 100-year epochs to a 40y-long one.

Statistical tests the Kolmogorov-Smirnov test is widely used to compare distributions, and I have no issue with its use here. I would only point out that the price of

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it not making distributional assumptions is that it has relatively low power. If the datasets are Gaussian, the authors may be better served by other tests that make this assumption, especially if they mainly intend to detect changes in location or scale. Note that precipitation is notoriously non-Gaussian, but can be made Gaussian via a transformation (cf the Standardized Precipitation Index, or SPI).

Significance One of the most persistent problems in our field is that statistical tests are carried out assuming IID (independent and identically-distributed) data, which in many cases is not verified. Indeed, persistence from month to month or year to year often drastically reduces the number of degrees of freedom available for a test [*Wilks*, 2011]. Did the authors account for autocorrelation in tests presented in Fig. 4? Also, in Fig. 8, how significant are the variations in correlation? In many cases they look well within sampling error to me. It is imperative that the authors quantify this, because it is one of their main results ("it is evident in the model experiments that differing teleconnections may result at different points in time and may differ from present-day relationships"), and it may well evaporate in the face of statistical rigor. Do the correlations change sign altogether? How much would this bias a multiproxy reconstruction of ENSO?

Wavelet spectra It should be noted that the Morlet wavelet spectrum as implemented by *Torrence and Compo* [1998] does not conserve energy, hence is not fit for spectral analysis [*Liu et al.*, 2007]. The authors need to use the correction proposed in the latter paper and redo Fig. 2.

1.4 Combining proxies

The idea to use multiple proxies to average out noise is nothing new. Few people will disagree with the authors when they write "We argue that proxy insights into change and variability in ENSO system are likely to be most robust when evidence is be synthesised over large spatial areas [...] considering multi-dimensional information in the

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form of spatial patterns of change through time is likely to yield more robust insights in large-scale systems."

While there could be many ways of synthesising evidence over large spatial areas, it seems that the authors have in mind the usual compositing, since they cite *Li et al.* [2013] as an example thereof. The authors should be aware that dating uncertainties may complicate this matter a great deal. Indeed, *Comboul et al.* [2014] showed that linear combinations of time-uncertain proxies may considerably distort the spectrum of the signal reconstructed from them. For certain diagnostics, like variance, *McGregor et al.* [2013] argued that one should first compute those diagnostics locally, prior to compositing. I ask that the authors acknowledge this work, and perhaps other efforts, to provide more specific guidance as to how one should synthesise evidence over large spatial areas in the real world. Such things are much trickier with real proxies than with gridded, exactly-dated GCM output.

2 Editorial comments

The writing style is often long-winded. In many cases, this is because the authors are handwaving instead of basing their arguments on solid, quantitative proof. It is also rather imprecise and there are numerous omissions, some of which are pointed out here. A revised version should tighten up the writing.

1. “The MIROC-ESM model is excluded from this analysis as it exhibits large drift related error in the form of long-term trends that cannot be attributed to natural variability, but instead relate to deficiencies in model physics and numerics (Gupta et al., 2013) (Fig. 3)." (p1587, bottom) needlessly repeats p1584 L 29.

2. p1584 L26 “its representation of ENSO spectra is too short": improper terminology. Just say that ENSO is two biennial in this model, or that its dominant

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periodicity is too short. A spectrum is neither short nor long.

3. p1588 L16-17 “compared with observed" change to "compared with observations"

4. p1589 L 16 “In the historical,..." . In the historical what?

5. p1598 L 20 “a single climate model that well represents ENSO spatial dynamics, particularly on the western extent of the warm/cold tongue, would provide further insight into the apparent complexity of ENSO impacts through time.” Is there such a thing? I have yet to see a non-flux corrected CGCM whose Cold Tongue stays where it should be. Can the authors give an example?

6. The Bellenger et al reference has all author names duplicated : Bellenger, H., Bellenger, H., Guilyardi, E., Guilyardi, E., Leloup, J., Leloup, J., Lengaigne, M., Lengaigne, M., Vialard, J., and Vialard, J.: ENSO representation in climate models: from CMIP3 to CMIP5, Clim. Dynam., 42, 1999–2018, doi:10.1007/s00382-013-1783-z, 2013. Please check other references for similar mistakes.

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