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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 #1

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Review of "Stability of ENSO and its tropical Pacific teleconnections over the Last Millennium" by Lewis, S. C. and A. N. LeGrande

Summary of key results

This study describes the variable character of ENSO properties simulated by an ensemble of last-millennium climate simulations, discusses their attribution and outlines possible repercussions on the interpretation of proxy-based ENSO reconstructions. ENSO characteristics are found to vary on decadal to centennial timescales, due to both, internal variability and external (mainly volcanic) forcing. The variable strength of ENSO teleconnection with a selection of tropical Pacific regions is found to not necessarily reflect changes in ENSO itself, but rather also changes in the teleconnection patterns. The authors therefore conclude that ENSO reconstructions based on proxies

from remote regions may lack robustness, and recommend to always interpret information from such proxies based on information from the Central Pacific.

General comment

The behavior of ENSO during the pre-instrumental period of the last millennium remains in large part uncertain. That non-stationarity of observed teleconnections can affect the robustness of proxy-based reconstructions of climate modes is a known possibility: this has been demonstrated for instance for the NAO using pseudo-proxy experiments (Lehner et al., 2012). This study promises a significant contribution to the ongoing discussion as the assessment of robustness of ENSO teleconnections is here conditioned by the background climate state, thereby attempting to clarify "which expressions of ENSO are being recorded in proxy archives under differing climatic boundary conditions". I found the study interesting, but before recommending this discussion paper for final publication, I ask the authors to consider a few thoughts and concerns I had while reading it.

As a general comment, the paper is overall well written and structured, and fits with the scope of the journal. However, I found myself sometimes wondering about whether all the results reported were really necessary considering the main aim reported above, while in other occasions I felt that more in depth analysis or more clarity was needed to support the conclusions.

A first point concerns the considered ensemble of simulations: I wander why MPI-ESM-P, for instance, is not included in the list (a millennial control run is also available in the CNIP reportistory for this model). Also, there are at least two full-forcing past1000 simulations with GISS-E2-R available, which one is used here? And why not use both?

A focus on statistical significance is of course important to substantiate all the results in section 4.1 and 4.2, and especially those concerning differences in ENSO statistics/metrics through the last millennium as these are part of the main conclusions of the study. The methods are well described in section 2.3, but I failed to see the significances for instance for figures 7 and 8.

Also, I wander why the authors decided to use the 20CR when only the 1976-2005 period of historical simulations is considered (Table 1). Why not use the whole period covered by the 20CR, or use instead more or different available reanalysis products? As the authors also report in the introduction, the instrumental record provides limited guidance for understanding the range of ENSO behaviors. Still, observations indicate that ENSO properties have changed over the last several decades, in particular with increased frequency of so-called Central Pacific events in most recent decades - the ones considered in this assessment (see, for instance: Pascolini-Campbell et al., 2014). It has been emphasized that different "types" of El Niño exist during the observational period that have substantially different characteristics (including different teleconnections, as shown for instance by Graf and Zanchettin, 2012). This observed behavior should be considered when discussing the simulations-reanalyses comparison over such a short and peculiar period of time.

A similar question concerns the limited temporal domain used for the Mid-Holocene simulations: is 100-year a long enough period to guarantee robust estimates about ENSO behavior, given the variability that is reported about the last millennium?

The authors should consider expanding the "model evaluation" section and related discussion: in fact, they mention six metrics used to evaluate ENSO, but in the following text there is very limited discussion on this.

A deeper analysis could substantiate interpretation of some results which appears at occasions to be not conclusive. For instance, concerning the difference between historical and last millennium simulations in Figure 1 (section 4.2), the authors provide an only vague interpretation (1592/21-25), while I felt it was exactly the aim of this study to provide an answer to this regard. I also did not find conclusive the analysis of internal versus externally forced ENSO variability in section 4.1. The authors themselves agree that this is the case (1598/26-29), so I wander what the aim of this section is: overall,

I suggest the authors to either deepen the analysis or largely restructure/reduce this section. Some specific concerns/suggestions I have on this are: when external forcing is considered, such as variable solar irradiance, why not substantiating the results with a wavelet coherence analysis (1590/15-24)? Also, the assessment of the role of volcanic forcing is too vague: no result is shown (e.g., from a superposed epoch analysis as typically done in these cases), only three major eruptions are reported in Figure 3 (but not the 1815 Tambora, why?), and only one eruption is discussed in the text. Later on, volcanic forcing (1591/23) as well as combined volcanic and solar forcing (1591/27) are reported again as a possible important factor for ENSO evolution. The summarizing paragraph (1591/21-1592/2) appears again to be too vague ("may be... may reveal...").

Minor/specific points

1581/7: typo ("is a is a")

1581/8: maybe Zou et al. (2014) is a worthy addition here

1581/17: I guess it is "does NOT capture"

1585/14: remove "in"

1585/18: isn't it Fig. 1 (and not Fig. S1)?

1587/13: please check that acronym SD is defined

1587/21: is MIROC5 the same as MIROC-ESM?

1588/5: I am not sure what "physically plausible" means in this instance, maybe expand a bit?

1590/15-21: Can you be more specific here about the role of solar activity? Are the prevailing La Nina like conditions induced by increased solar activity a result of this study or from previous ones? Actually Figure 3 does not seem to show this as the 1258 seems rather associated to cold anomalies.

1591/27: combination of

1592/17: resemble

1593/4-5: I wander whether the linear relationship is really different for the two experiments, or, rather, the regression is for both not significant (and then differences do not really matter).

1593/6-7: I was not able to see where significance is reported? I think it is important to report it since by eye I wouldn't say that for some regions/variables the changes are so dramatic. . .

1593/15: same as above: where is significance reported?

1594/5: sites in the tropical . . .

1596/23: we find that ENSO. . .

1597/3-5: I think the use of parentheses here is confusing

1597/25: "the stability . . . is . . . variable" sounds strange, so maybe rephrase?

1598/6: why necessarily?

1598/8: volcanic)

Fig. 1 caption: check space in "La Niña"

Fig. 2: there is a strong peak at 6-year period in the historical IPSL-CM5A-LR simulation, any thoughts on this?

Fig. 3: the anomalies for bcc-csm1-1 are noticeably mostly negative, so I wander how anomalies are exactly calculated (not from full-period average?)

Fig. 4 caption: check panel for 20CR precip

Fig. 5: maybe it could be useful to add a Box-Whisker plot for the past1000 simulations, to see how they compare with the piControl.

Fig. 8: To me it seems that the only changes in the West Pacific for temperature are associated to volcanic eruptions (1258, Kuwae). Does this support the hypothesis of a volcanic influence? The question is also how much short-term effects could affect the long-term (100 year in this case) statistics. Was any smoothing applied to the series? How would the statistics change if the data around the years of major eruptions are removed from the analysis?

Supp. Fig. 2: should one of the "showing" be removed?

Supp. Fig. 6: what does the blue shading indicate in panel a?

References

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