**Climate
of the Past**

Discussions

EGU

Interactive comment on "Was the Little Ice Age more or less El Niño-like than the Mediaeval Climate Anomaly? Evidence from hydrological and temperature proxy data" *by* L. M. K. Henke et al.

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We thank the reviewer for taking the time to provide a review. We have broken down the comments into subheadings (italic) followed by our responses in plain text.

1. ENSO definition ENSO is an interannual phenomenon. Looking at 30-year averages and calling that ENSO is more than a stretch. That choice was not explained particularly well in the paper, though it has major consequences for the research, blurring interannual and decadal signals, and drastically reducing the number of degrees of

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freedom.

[this is the same as our response to reviewer 1's comment 4.] Both reviewers raised the issue of reconciling 30-year averages and the interannual ENSO timescales. Our aim here was not to comment on ENSO interannual variability, frequency or amplitude a la Li et al (2013) and Emile-Geay et al (2013), but rather ENSO-like climate change similar to many long-term, lower resolution proxy studies (cf. Moy et al. 2002, Conroy et al. 2008, Yan et al. 2011). This was clearly not conveyed very well and we apologise for the confusion caused. We absolutely agree that it is not possible to translate this directly into ENSO variability, frequency and intensity and thank the reviewers for highlighting the need to clarify this.

2. Climate target The whole point of pseudoproxy experiments is to have a known target to compare to. The authors choose instead an assimilated product (20CRv2), which is then "replicated in the time dimension to get a length of 1000 years" (B4, L12). This is madness. Why not use a CMIP5/PMIP3 simulation? Since the authors are not using their method to generate an actual reconstruction that they seem to believe in, they might as well stay in pseudoproxy land, where they would have perfect control over the experimental design. Instead, they have to make up some data from the 20C reanalysis through unspecified means, which is downright scary. At the very least, the generating mechanism for the data needs to be transparent.

In the revised manuscript, we will use a modified scheme for calibrating and validating our proxy networks.

First, we will continue to use the annual mean 20CRv2 reanalysis data to derive our EOF pattern. This has the disadvantage that it is not based on 30-year mean data, and will therefore be somewhat different from the true long-term pattern of El Nino-like climate change. (It is not possible to derive a 30 year mean pattern from 20CRv2 because the data series is less than 200 years long.) However, this annual mean 20CRv2 pattern is likely to be more representative of the true 30-year mean pattern

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than GCM derived patterns. This is based on the fact that GCM annual mean patterns correlate with their own 30-year mean pattern more strongly than GCM annual mean patterns correlate with the annual mean 20CRv2 pattern.

We will then run our calibration experiment, wherein we derive our optimal networks of proxy locations, using PMIP last 1000 year simulations, as suggested by the reviewer. Because the PMIP simulations show a different ENSO pattern than 20CRv2, the quality of our reconstruction will be degraded. However, on balance, we agree that this is preferable to our previous reconstruction method that used 20CRv2 alone at the cost of satisfactorily representing degrees of freedom in the data.

Our validation experiment will proceed using a PMIP last 1000 year simulation from a different GCM. This presents a difficult target for our networks, but one that makes some allowance for the fact that the real El Nino pattern that we will run our reconstruction on is different from the El Nino pattern presented by the PMIP simulation we will use to calibrate our network. When calculating CE, rather than using white noise to compare our pseudoproxy reconstruction against as in the original manuscript, we will use a control (ie unforced) simulation from the GCM used to provide the PMIP validation simulation processed via our networks in exactly the same way. In practice, the spectrum of the control reconstruction is very close to white due to the small variation in unforced ENSO dynamics on this timescale. However, it is not inconceivable that our reconstruction method introduces some unwanted autocorrelation, and so we agree with the reviewer that it makes sense to revise this.

Finally, we will repeat the calibration / validation process across all available PMIP GCMs, investigating the impact of using different calibration and validation simulations, omitting only those GCMs that have a poor representation of ENSO.

3. Pseudoproxy design The field has evolved a bit in recent years, now enabling the generation of more realistic pseudoproxies using proxy system models (Tolwinski-Ward et al, 2011; Dee et al 2015). Using an explicit process model of tree-ring growth,

Evans and al (2015) found quite different results from the usual "climate + white noise" pseudoproxy design. They authors pay lip service to it in section 5.1 but don't actually use it. Given that their paper is entirely centered on pseudoproxies and that all these PSMs are publicly available, they have no excuse but to keep up with the times

As indicated by the reviewer, one possible approach is to build pseudoproxies using a proxy system model. However, for many proxies this requires access to isotope enabled GCM data, which is not an option for the majority of available PMIP GCMs. We therefore concede this point as a caveat in our network selection and validation and one for further research. The VSlite tree ring model (Tolwinksi-Ward et al., 2011) only requires temperature and precipitation as inputs, however. We will therefore test the choice of VSlite over the simple pseudoproxies derived using the method of Smerdon et al. (2012).

In the new manuscript we propose to also process our real proxy data using Inverse Transform Sampling so that they are more normally distributed (Emile-Geay et al. 2016). In the absence of detailed proxy models for each proxy, this is a sensible way of making the data more comparable to our simple pseudoproxies.

In addition, we propose to investigate another very important source of error in pseudoproxy reconstructions that we can explore with the range of GCMs available to us: the error in GCM representation of climate's spatiotemporal variability. These errors lead to suboptimal network selection due to inaccurate representation of the ENSO-like climate change pattern and other patterns of climate variability. They will also give us some limited insight into what extent our results are likely to be compromised by changes in the pattern of El Nino-like climate change (identified as an issue by Power et al. 2013 for future climate change).

As the reviewer notes, real proxies may show a response to a combination of these (and other) factors. Evans et al. (2014) explored this by testing a reconstruction based on pseudoproxies that respond to both temperature and precipitation. In our paper,

we acknowledge this issue, and focus on proxies that are as much as possible just a function of either temperature or precipitation. This leads to the exclusion of some proxies that are well-known to be related to ENSO, but that respond to, for example, both temperature and salinity or temperature and precipitation (see response to reviewer's comment 4 below).

4. Unjustified proxy selection Nowhere in the paper could I find a rational justification for the choice of proxies. It leaves out the best ENSO proxy developed for the past millennium (Cobb et al, 2003), and incorporates many dubious ones from regions that have only marginal teleconnections to ENSO.

[this is largely the same as our response to reviewer 1's comment 1.]

We endeavoured to include all precipitation and temperature proxies currently publicly available for the study region. As reviewer 1 notes, there are indeed several coral records available that were not included I this study. The reason for this is that it was stated explicitly in the associated publications that the recorded d18O signal was in fact a combination of SST and SSS (or SST and precipitation). Most authors also state that it is not possible to disentangle the two climate variables(McGregor & Gagan 2004, Hereid et al. 2013, Osborne et al. 2014). While this is not an issue when, for example, one is using the single proxy as a recorder of ENSO events in a location where SST and SSS have the same-sign impact on the d18O of the coral, it does present a problem for our method of reconstruction. As we are using temperature- and precipitation-specific EOF patterns, we need proxies that are as univariate as possible (i.e. only respond to one of these two climate variables).

The point is that we use any precipitation or temperature proxy, and have their contribution to the reconstruction decided by the EOF weighting. The empirically informed add-one-in algorithm further informs the usefulness of the proxies in reconstructing the ENSO-like signal. If a certain proxy is also significantly influenced by other climatic oscillations, for example, it would be less likely to be included in the final proxy network

(Section 4). The Cobb et al. (2003) coral record is in fact included in our reconstruction, as can be seen in Table 2.

As noted in the manuscript (Section 2.1), the choice to exclude records with a dating error >=60 years is somewhat arbitrary but chosen as it is twice the averaging period of 30 years. We apologise if it was not clear how this dating error was then determined for individual proxies. We took the dating error from the original publications where it was reported, and else referred to age model results as found in the raw data files. In the latter case the maximum error reported for the past 2000 years was used – for longer records, larger errors further back in time were thus not taken into account as they are irrelevant for the period of interest.

5. fanciful statistics This comment consists of four parts, which we address here:

a) the statistical methodology used for reconstruction is an extreme version of principal component regression, truncated at 1 pattern. This in itself is not the worst thing, but PCR is known to yield extremely biased reconstructions in the presence of noise. Total least squares (orthogonal regression) can mitigate that a bit, but PCR isn't outlandish as a first pass. However, the effect of this truncation on ENSO reconstruction should be assessed. For instance, it is known that ENSO tends to be split among different modes (most commonly, EOFs 1 and 2 of monthly-mean SST), so only selecting one can leave out an important part of the signal.

We have experimented with using a more "optimal detection" style technique, where we regress data onto multiple EOFs in order to combat the bias the arises from mixing other signals not due to El Nino-like change into our reconstruction. However, we found that even at 2 EOFs data availability is too low to constrain estimates of changes in time. It is for this reason that we settled on an the empirical technique, whereby rather than use all available data (as in standard PCR) we successively add pseudoproxies that most improve (or least damage) the reconstruction in order to find the best possible network. We therefore attempt to avoid networks that mix in large non-El

Nino-like signals, which may occur if we use all available proxies weighted by one EOF. The success or failure of the network will then be measured by the new validation procedure. (See response to comment 2 above.) We will make this issue clearer in the new manuscript.

The reviewer states that multiple EOFs should be used to resolve the ENSO-like pattern. On annual and longer timescales this is not typical, both in studies of paleoclimate (e.g. Braganza, 2009) or GCM studies of future climate change where all output is known (e.g. Power et al. 2013). Unfortunately it is also not possible to investigate multiple EOFs given the data limitations we face.

b) But the worst part of the procedure is the generation the pseudoproxies and then double-dipping between the calibration and verification periods. The authors claim that it "somewhat compromises the independence of the validation"; actually, it completely compromises it. There is no validation unless it is entirely independent of calibration.

This is a very good point and we thank reviewer 2 for raising it. To mitigate this, we will be using pairs of PMIP last-millennium model runs instead. In each case, model A will be used for calibration and model B for validation, ensuring complete separation. The new reconstruction has the weakness that it uses an ENSO-like pattern and temporal variability that is somewhat different to observations. However, because we are using pairs of models each with its own ENSO-like EOF pattern and temporal variability, we will produce and investigate a measure of the impact of this problem in our pseudoproxy experiments.

c) The bottom line is that by focusing on 30-year averages, they leave themselves so few degrees of freedom that there is no room for validation any more. This would be mitigated by using long GCM integrations, (1000 years / 30 âĹij33 d.o.f., not very high, but better than 1 or 2). Even better would be too look at annual signals, since so many of the proxies are annually resolved and since ENSO is in the 2-7y band.

As explained in our response to comment 1 above, our interest is in long-term ENSO-

like climate trends and not interannual variability. We therefore continue to investigate 30-year means. However, we are now proposing to use PMIP simulations for both pseudoproxy target and CE benchmarks to improve the representation of degrees of freedom, as the reviewer suggests.

d) Another major problem is that the CE statistic appears to be benchmarked against white noise predictors, whereas with 30 year averages one needs to account for very high levels of autocorrelation, which alone are enough to doom the results.

We now benchmark our CE statistic against control runs (ie unforced simulations) taken from the PMIP archive and fed through our networks in the same way as the PMIP last 1000 year simulations. Contrary to the reviewer's statement, inspection of the autocorrelation of unforced ENSO variability in climate models is close to zero for thirty year means. Nevertheless, we agree that it is useful to establish this for our networks.

6. spatial pattern the question of whether the MCA was El Niño or La Niña-like seems to have been largely motivated by the work of Mann et al (2009). Wang et al (2015) showed earlier last year that it was an artifact of the dataset and method they use, and that changing either would change the result. A paper focusing on this question should acknowledge this research.

Unlike reviewer 2's assumption that this was motivated by Mann et al. (2009), the main point of departure for this work was Yan et al. (2011). Our primary interest is in the possibility that temperature and precipitation patterns apparently show opposing ENSO-like patterns of change. However, we are happy to reference and discuss Wang et al. (2015).

7. unclear methods a lot of the methodology is unusual enough that it needs to be very thoroughly presented here. I deplore the lack of equations to explain how certain things were done. Public code would be another way to enable an actual peer-review of the methodology.

We apologise if reviewer 2 felt that the methodology was not clearly presented; we attempted to find the balance between conciseness and detail and have apparently not done this satisfactorily. We will revisit the Methods section and endeavour to clarify as much as possible.

8. Moy The record by Moy et al keeps being used as an "ENSO" proxy, though some of its authors have since retracted their interpretation of it. http://www.sciencedirect.com/science/article/pii/S0277379108001352 The community has got to stop clinging to an outdated interpretation

We do not know any literature that explicitly questions the interpretation of this record as an ENSO proxy. The paper cited by reviewer 2 does not call this into question either. The only sentence in it referring to this record states "The middle–late Holocene increase in the flux of clastic sediment to alpine lakes in the tropical Andes is similar to the record of El Niño frequency (Rodbell et al., 1999; Moy et al., 2002), and thus may reflect a fundamental change in the nature of ENSO at this time." (Rodbell et al. 2008). We are unable to act on the concerns of unnamed researchers outside the peer-review literature.

9. quirky problematization Science progresses by "standing on the shoulders of giants". While none of the published ENSO reconstructions are perfect (they are most likely all wrong), they should serve as a starting point of the analysis. While the paper does cite recent ENSO reconstruction at one point or other, it seems to happen as an afterthought; instead, the divergence between published estimates should really be the point of departure of the analysis.

We chose not to focus on these reconstructions in detail as our focus is really on comparing long-term trends paying particular attention to the difference between the MCA and the LIA. As stated in the discussion, those reconstructions focusing on interannual ENSO variability do not cover both these periods.

We hope that by clarifying our intention to look at ENSO-like climate trends rather

than interannual ENSO variability the reviewer can agree that these high-resolution but shorter ENSO reconstructions are of less interest (except for methodological similarities and differences). Instead, we are taking the divergences among the multitude of long-term, lower-resolution ENSO proxies as our starting point, and particularly the conclusions of Yan et al. (2011). We will endeavour to emphasise this more clearly in the introduction to the new manuscript.

Yours faithfully,

Lilo Henke

References:

Braganza, K., Gergis, J. L., Power, S. B., Risbey, J. S., & Fowler, A. M. (2009). A multiproxy index of the El Niño-Southern Oscillation, A.D. 1525-1982. Journal of Geophysical Research: Atmospheres, 114, 1–17. doi:10.1029/2008JD010896

Cobb, K. M., Charles, C. D., Cheng, H., & Edwards, R. L. (2003). El Niño/Southern Oscillation and tropical Pacific climate during the last millennium. Nature, 424(July), 271–276. doi:10.1038/nature01779

Conroy, J. L., Overpeck, J. T., Cole, J. E., Shanahan, T. M., & Steinitz-Kannan, M. (2008). Holocene changes in eastern tropical Pacific climate inferred from a Galápagos lake sediment record. Quaternary Science Reviews, 27, 1166–1180. doi:10.1016/j.quascirev.2008.02.015

Emile-Geay, J., Cobb, K. M., Mann, M. E., & Wittenberg, A. T. (2013). Estimating central equatorial pacific SST variability over the past millennium. Part II: Reconstructions and Implications. Journal of Climate, 26, 2302–2328. doi:10.1175/JCLI-D-11-00511.1

Emile-Geay, J., & Tingley, M. (2016). Inferring climate variability from nonlinear proxies. Application to paleo-ENSO studies. Climate of the Past, 12, 31–50.

Evans, M. N., Smerdon, J. E., Kaplan, A., Tolwinski-Ward, S. E., & Gonzalez-Rouco,

J. F. (2014). Climate field reconstruction uncertainty arising from multivariate and non-linear properties of predictors. Geophysical Research Letters, 41(24), 9127–9134. doi:10.1002/2014GL062063

Hereid, K. a., Quinn, T. M., Taylor, F. W., Shen, C. C., Edwards, R. L., & Cheng, H. (2013). Coral record of reduced El Niño activity in the early 15th to middle 17th centuries. Geology, 41(October), 51–54. doi:10.1130/G33510.1

Li, J., Xie, S.-P., Cook, E. R., Morales, M. S., Christie, D. a., Johnson, N. C., Fang, K. (2013). El Niño modulations over the past seven centuries. Nature Climate Change, 3(9), 822–826. doi:10.1038/nclimate1936

McGregor, H. V., & Gagan, M. K. (2004). Western Pacific coral ??18O records of anomalous Holocene variability in the El Niño-Southern Oscillation. Geophysical Research Letters, 31, 1–4. doi:10.1029/2004GL019972

Moy, A. D., Seltzer, G. O., Rodbell, D. T., & Andersons, D. M. (2002). Variability of El Ni∼no/Southern Oscillation activity at millennial timescales during the Holocene epoch. Nature, 420, 162–165. doi:doi:10.1038/nature01194

Osborne, M. C., Dunbar, R. B., Mucciarone, D. a., Druffel, E., & Sanchez-Cabeza, J. A. (2014). A 215-yr coral δ18O time series from Palau records dynamics of the West Pacific Warm Pool following the end of the Little Ice Age. Coral Reefs, 33, 719–731. doi:10.1007/s00338-014-1146-1

Power, S., Delage, F., Chung, C., Kociuba, G., & Keay, K. (2013). Robust twenty-first-century projections of El Niño and related precipitation variability. Nature, 502(7472), 541–5. doi:10.1038/nature12580

Rodbell, D. T., Seltzer, G. O., Mark, B. G., Smith, J. A., & Abbott, M. B. (2008). Clastic sediment flux to tropical Andean lakes: records of glaciation and soil erosion. Quaternary Science Reviews, 27(15-16), 1612–1626. doi:10.1016/j.quascirev.2008.06.004

Smerdon, J. E. (2012). Climate models as a test bed for climate reconstruction meth-

ods: Pseudoproxy experiments. Wiley Interdisciplinary Reviews: Climate Change, 3(February), 63–77. doi:10.1002/wcc.149

Tolwinski-Ward, S. E., Evans, M. N., Hughes, M. K., & Anchukaitis, K. J. (2011). An efficient forward model of the climate controls on interannual variation in tree-ring width. Climate Dynamics, 36(11-12), 2419–2439. doi:10.1007/s00382-010-0945-5

Yan, H., Sun, L., Wang, Y., Huang, W., Qiu, S., & Yang, C. (2011). A record of the Southern Oscillation Index for the past 2,000 years from precipitation proxies. Nature Geoscience, 4(9), 611–614. doi:10.1038/ngeo1231

Interactive comment on Clim. Past Discuss., 11, 5549, 2015.