Clim. Past Discuss., 11, C1813–C1817, 2015
www.clim-past-discuss.net/11/C1813/2015/

Climate of the Past Discussions

Interactive comment on "The Paleoclimate reanalysis project" *by* S. A. Browning and I. D. Goodwin

Anonymous Referee #3

Received and published: 27 September 2015

The authors present a reconstruction of the climate of the past 1200 years based on information from proxy data and climate model simulations through selection of best-matching analogues. Some aspects of the reconstruction assessed and a comparison with purely proxy-based reconstructions for a selection of climate indices is presented. The manuscript presents an alternative way to reconstruct past climate states that may be relevant to the community, however, the method used and some of its limitations are not well discussed. Therefore, I recommend that the paper be revised significantly before publication in Climate of the Past.

1 General comments

The authors chose to reconstruct past climates using an analogue approach. While this is a viable approach that is computationally inexpensive, it also is quite limited in the specific setup chosen for this study. In particular, the influence of external forcings is not taken into account in the reconstruction. This is a fundamental limitation of the analogue approach set up chosen for this study that should be discussed more prominently in the manuscript. Also the choice to use all available years for analogue selection seems problematic in the light of studying teleconnections in case these are in fact non-stationary. With changes in teleconnections as with changes in external forcings, BMA might be selected for the wrong reasons and thus not be representative in locations away from proxy information. If BMA spread is representative of reconstruction uncertainty, then BMA spread could be used to identify such cases (also see the comment on BMA spread below).

There is some ambiguity in the description of the method as to whether the reconstruction was performed on yearly or decadal data. As far as I understand, the results are only presented as decadal averages, but the actual analogue selection is performed using yearly data. This seems somewhat inconsistent, in particular as the rationale for using decadal averages is to mitigate dating issues with the proxy data. In the lights of increasing the signal-to-noise ratio in the final output (decadal averages), I suggest the authors select best analogues on decadal data. This would also clarify how the 50BMA of yearly data are combined to decadal averages (given that there is no temporal consistency between the 50BMA of year 1 and year 2, ... estimating the variability of decadal averages from the pool of 50 x 10 BMA is not trivial).

Another arbitrary choice that needs further justification (or revision) is to limit the selection of analogues to robust climate signals in the proxies that exceed +/- 0.5 sigma. Thereby, the authors seem to suggest that there is information in knowing that a proxy time series is anomalous (at a given space-time location) whereas there is no informa-

tion in knowing that the anomaly is close to zero. This unjustifiable criterion may lead to the fact that BMA are selected that exhibit strong signals at proxy locations where the proxy data indicate no anomalies and thus BMA are selected that are in fact inconsistent with the proxy data. I strongly suggest the authors revise their choice to exclude small proxy anomalies from the BMA selection process.

The authors claim that the spread of the 50 BMA can be used as an estimate for the uncertainty of the reconstruction. This is an interesting concept, that is not pursued in the paper. One way to investigate whether the above assertion has some merit would be to compare maps of BMA spread and pseudoproxy correlation, as one would expect larger spread in areas with smaller correlation. Additionally, one may analyze the correlation between time series of BMA spread and the ensemble mean error. If the spread is indicative of the uncertainty, then there should be correlation between spread and error.

Finally, the evaluation of the method should be improved to build confidence in the reconstruction dataset. The authors perform in-sample validation for the first part of the evaluation (Figure 2). I strongly suggest that the approach is changed such that the proxies that are evaluated are not actually included for the selection of BMA to get an understanding of how well the reconstruction works in places where we do not have proxy data. Given that the BMA approach is computationally cheap, this should be easily achievable.

2 Specific comments

Sec. 2.3: The authors chose to select the 50 BMA without providing convincing arguments for this choice. I suggest to add a Figure to illustrate the BMA ensemble reliability (e.g. the spread to error ratio) for various choices of ensemble size.

Sec. 3.1: The discussion of LME vs. reconstruction correlation is not satisfactory as ba-

sically the correlation due to external forcing (LME mean) is compared with overstated correlation (see major comment above) of the best estimate of the internal variability (reconstruction). In particular, the statement “LME provides a realistic simulation of internal climate variability that is temporally inconsistent with most proxy evidence” is troubling. To assess whether LME’s internal variability is temporally inconsistent with the proxy evidence one may rather select the best matching member (out of 10 LME ensemble members) and compare the correlation of such a reconstruction to the BMA reconstruction. Such an approach could also be used illustrate the benefits of pooling all available years for BMA selection.

Sec. 3.3: In the statement L26ff, the authors seem to suggest that all differences between PaleoR index reconstructions the comparison indices indicate periods of non-canonical behavior the comparison indices cannot reproduce. This is based on the unsupported assumption that PaleoR is superior to the comparison indices. Please rephrase.

Sec. 4.1: the statement on L5 lacks support. From the manuscript at hand I cannot see that analogue selection should be superior to regression-based reconstruction methods, let alone proper data assimilation approaches.

Sec. 4.3: One way to account for proxy uncertainty is to include an estimate of proxy error into the standardization of the proxy / model differences used for analogue selection.

Figures 2 and 3: Correlations with tropical SST proxies are surprisingly low or negative (Figure 2a), whereas the pseudo-proxy analysis suggests that the reconstruction should actually produce strong correlations (Figure 3c). This is somewhat puzzling, please discuss.

Figure 4: The scales for the PaleoR reconstruction and the comparison indices differ. This is misleading and very bad form and should be changed. Also, the reason why the variability in PaleoR indices is considerably smaller than in the comparison indices

should be discussed.

Figure 5: The comparison of two different time slices seems somewhat arbitrary. Here I would prefer composites for individual indices if such an analysis is deemed necessary at all.

3 Technical corrections

P4163L15: low signal-to-noise

P4173L6: PaleoR NAO and the

P4174L2: expected to yield

Interactive comment on Clim. Past Discuss., 11, 4159, 2015.