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

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In response to the main concerns raised by Referee 1 we have amended the manuscript to provide more detail on the way in which forcings are accounted for and expanded the discussion on PaleoR uncertainties. We also provide additional justification for selection of proxy data and the decision to excluded proxies from time periods where their climatic signal is ambiguous.

1/ The authors argue that online data assimilation is much more expensive that offline data assimilation and is not necessary advantageous (e.g. Page 4162). This is perfectly fine but the authors should at least mention the potential advantages of online data assimilation. Discussing the results of the recent study of Matsikaris et al. (2015)

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in this framework may be useful.

Matsikaris et al. (2015) found little difference between on-line and off-line approaches, but mentioned that online DA could be advantageous where research objectives demanded a continuous simulation, especially if time and cost were not an issue. A comment mentioning has been included in the revised text.

2/ Page 4164, it is mentioned that "Each year of the LME represents an individual multivariate realization of a physically plausible climate state. In this respect the interannual temporal continuity of the LME can be discarded, thereby giving an effective ensemble size of 11 560 members". I indeed agree that, for the surface variables that are discussed here, maintaining interannual continuity is probably not useful in many cases. Nevertheless, forcing is changing through time. For example, some years have large volcanic eruptions; anthropogenic forcing is strong at the end of the period. If I understand well the methodology, a year in the beginning of the period not directly affected by any volcanic eruption can have a best analogue showing a strong volcanic impact or characterized by a much larger greenhouse gas forcing that actually observed during the period investigated. This would mean that the model-data agreement may occur for wrong reasons, a wrong forcing compensating for some biases in the model, for instance. I do not know if this occurs often or not but this should at least be mentioned. If possible, adding some diagnostics to show if samples from the recent decades are often used as analogs over the pre-industrial period or if years with a strong volcanic impact are predominantly selected for periods when such effects are expected would be very useful. In the same lines, the authors correctly argue that their method is more adapted to take into account the non-stationarity of teleconnections than many standard ones. Nevertheless, if this non-stationarity is related to temporal changes in the forcing, mixing different years/periods might introduce additional problems.

This is not really an issue because the analogue selection will choose the climate state that best matches the proxy data, regardless of how the model generated the climate state. A study investigating whether the model produces a forced response

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that is consistent with proxy evidence could potentially be achieved within the PaleoR framework, however such an investigation is beyond the scope of the current study. If non-stationary during a given time period is the result of anomalous forcing that produces a climate state not simulated by the model, our method will struggle to precisely resolve that time period, however the ensemble approach will go some way towards cushioning the effects of this and the analogue selection will still return the closest approximation that the model can provide. Additional text has been added to the revised manuscript in Section 4.2 (Limitations) specifically discussing these points.

3/ The authors use a simple method. This is perfectly valid and it is certainly interesting to compare its results to more sophisticated ones. Nevertheless, the uncertainty in the proxies is not explicitly accounted for in Eq. 1. The number of analogues selected ï£ijis not objectively determined. 50 are chosen as it seems to give good results but, depending on the similarities between reconstructions and model results, a larger or a smaller number of analogues can be more justified during some periods I guess. Consequently, the uncertainty range given is only illustrative. This is an important point and this should be mentioned explicitly. The way the comparison with the range given by reanalyses over the 20th century is presented is also too optimistic for the proposed approach to my point of view as the latter use an objective estimate of the range. Furthermore, a comparison with other methods applied in paleoclimatology, which provide more objective estimates of the uncertainty, as in Goosse et al. (2012) or Steiger et al. (2013), should be provided (although I agree that many problems remain there too).

Referee 1 raises several concerns here: (1) uncertainties are not accounted for in Eq.1; (2) that the optimum number of analogues is not objectively determined and may vary between time periods; (3) that the uncertainty range is only illustrative; and (4) that a comparison with other approaches to uncertainty should be included.

(1) Including an estimate of uncertainty in the Eq.1 would be appropriate in methods such as Kalman filtering, where one might seek to adjust the model simulation to a

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specific (uncertain) value. However, it would not be appropriate in our approach and would only add additional uncertainty to the analogue selection. We take a conservative approach to proxy uncertainty by evaluating the proxy signal for each time period and excluding proxies with ambiguous signals – as described in Section 2.1.

(2) We conducted a series of sensitivity experiments to determine the optimum number of analogues; an in-depth discussion on this is provided in Browning (2014) and Goodwin et al (2013)–as cited in the manuscript (Section 2.3).

(3 4) The uncertainty estimates implicitly encapsulate uncertainties associated with proxy dating, proxy climate signals, model uncertainties and the analogue selection process. An expanded discussion on uncertainties has been included in the revised manuscript (Section 4.2).

4/ It is mentioned page 4163 that the method accommodates well proxies with various resolutions. This point is not clear to me. For instance, if the method looks for analogs at decadal scale, how can it handle proxies with centennial scale resolution except by interpolating it at decadal scale? If this is the case, it is not very different from other methods. How does the approach compare with the recent study of Steiger and Hakim (2015) on this issue?

At no point in the manuscript do we state that proxy data are interpolated to a higher resolution. As explained in Section 2.1 we can produce variable resolution reconstructions by adjusting the resolution of each time period to suit the lowest resolution data. Therefore, if we were to use centennial scale data we would reconstruct centennial length time period–annual or decadal data would be coarsened to centennial means. The new Steiger and Hakim (2015) paper offers an approach that accommodates multiple time scales. This concept might be relevant for future versions of PaleoR. However, it would be important to ensure the low-resolution data represented low frequency climate variability and not simply increased dating uncertainty.

5/ Page 4163. The procedure used to select the proxies should be clarified. What

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is the criteria used to choose the 130 proxies? How are they calibrated to get the climatic variables compared to model results? What means "are normalized relative to the 1300–2000 AD long-term mean"? Is it just subtracting the mean or also dividing by the standard deviation? I am surprised that "only proxies displaying an unambiguous climatic signal defined as the decadal mean exceeding 0.5 std" are selected. To me, a proxy records displaying a climate close to the long term mean is also an interesting information. Do the authors expect that, in any case, if enough members are selected and no constraint is applied locally, the mean over the ensemble would lead to the climatological state?

Referee 1 raises several points here: (1) we need to clarify why we chose the 130 proxy records, (2) how did we determine which model variable the proxies represent, (3) clarification of the proxy normalization, (4) clarification of why we exclude ambiguous proxy signals.

(1) We have added text to Section 2.1 to clarify that the proxy data were selected to provide a broad spatial distribution of records, where the published literature indicates we should have high confidence in their climatic interpretation.

(2) As stated in Section 2.1 and 2.2, proxy data are interpreted in terms of their original published climatic interpretations. This information is also displayed for each proxy record in Table A1. Section 4.3 contains a discussion on how the proxy-model calibration might be improved in future versions.

(3) Added text to clarify that the proxy data are normalized to z-scores by subtracting the mean and dividing by the standard deviation. Removed misleading reference to the 1300-2000 normalisation period as this is not applicable to shorter duration records.

(4) Our primary motivation for this decision is that excluding proxy data from time periods where their climatic signal is ambiguous is a logical and conservative way to account for proxy uncertainty–this motivation is clearly articulated in Section 2.1. However, this is a very good point of discussion with potential implication beyond our study.

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Having worked extensively with a range of proxy data, we understand many of the challenges required to extract a climate signal from a proxy record. Proxies can be non-linear and inevitably contain a component of non-climatic noise. While they can be very good at recording anomalous conditions, we question the interpretation of mean signals in most proxy-based studies. When a strong signal is not present in a proxy record for a given time period, we can conclude with confidence that the proxy did not record a strong anomaly: it is another matter–requiring additional evidence–to conclude a strong anomaly did not occur (see Mann et al Nat Geosci (2012) for a related discussion). During testing and development we did experiment with including all proxy records. However, at each timestep proxies displaying non-anomalous (mean) conditions tended to dominate the analogue selection process at the expense of proxies displaying anomalous conditions. This is because there are many more analogues for mean conditions then there are for anomalous conditions. When proxies displaying ambiguous signals were included in the analogue selection the 50BMA ended up being no different to a random selection, and PaleoR had no skill at all. It is therefore not appropriate–at present–for this method to include (at each timestep) proxies that show ambiguous climate signals.

6/ I suggest to add a sub-section, in the relatively short section on results, comparing the estimate of continental-scale temperatures provided by PaleoR with recent reconstructions (PAGES 2K, 2013). This would be a nice complement to the comparison at local scale with proxies used to drive the model and allow to identify if the upscaling provided by PaleoR is similar to the ones given by more traditional methods.

We are currently working with Pages to produce a comprehensive methods intercomparison paper utilizing the Pages 2K version 2 dataset.

Specific points 1/ I would be more specific in the title, adding maybe something like ":application to the past millennium".

This is possible, but we would prefer to leave the title as is.

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2/ Page 4168. Various causes of discrepancies between PaleoR and local reconstructions are mentioned but model biases should already be added as a possible origin at this stage and not only in the discussion section.

Text modified to include reference to model biases.

3/ Page 4170, end of the page. The discrepancy between various estimates may have many origins (interpretation of the proxies used, method, model biases, etc.). It is thus impossible to my point of view to have a conclusion on the stationarity of the teleconnection or non-canonical behavior of ENSO on this basis.

Text has been modified to clarify this point.

4/ Page 4172, last line. I would add 'potentially' before 'able' has this has not been checked enough using independent data.

We agree, text has been modified

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