

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

### **Anonymous Referee #1**

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The authors apply a methodology based on a simple data assimilation method to reconstruct the climate of the last millennium using a selection of 130 proxy records and an existing ensemble of 10 simulations performed with CESM1. Many discussions are currently occurring about the methods that should be used to estimate past climate variations and the study presented here provides a very valuable contribution in this framework, in particular as data assimilation is a relatively new field in paleoclimatology. I thus consider that the paper deserves publications in *Climate of the Past*. Nevertheless, additional information must be included in the revised version to improve the clarity of the method. This does not imply any significant change in the methodology itself or in the conclusions but this is mandatory to my point of view to allow the reader to understand the proposed methodology and its advantages and limitations compared to other approaches that are currently developed by other groups.

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1/ The authors argue that online data assimilation is much more expensive than offline data assimilation and is not necessarily 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) in this framework may be useful.

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.

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

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is 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).

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?

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

6/ I suggest to add a sub-section, in the relatively short section on results, comparing

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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.

Specific points

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

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.

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.

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

New references: Matsikaris A., M. Widmann, and J. Jungclaus On-line and off-line data assimilation in palaeoclimatology: a case study. *Clim. Past*, 11, 81-93, 2015  
Goosse H., E Cresspin, S. Dubinkina, M.F. Loutre, M. E. Mann, H. Renssen, Y. Sallaz-Damaz, and D. Shindell. The role of forcing and internal dynamics in explaining the “Medieval Climate Anomaly”. *Clim. Dyn.* 39, 2847–2866, 2012  
Steiger N. and G. Hakim. Multi-time scale data assimilation for atmosphere–ocean state estimates. *Clim. Past Discuss.*, 11, 3729-3757, doi

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