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Interactive comment

Interactive comment on "Atlantic Multidecadal Variability from the Last Millennium Reanalysis" *by* Hansi K. A. Singh et al.

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This study conducts a global climate reanalysis over the years 0–2000CE using the so-named Last Millenium Reanalysis (LMR) approach as described by (Hakim et al. 2016) to evaluate the Atlantic Multidecadal Variability. The manuscript is well written and fits within the scope of Climate of the past.

I find the methods, results and discussion are appropriate. The overall quality is good, and I would recommend its publication.

I would, however, raise a few points that the authors should consider prior to final publication.

Answers to specific points:

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Discussion paper

1. Does the paper address relevant scientific questions within the scope of CP? Yes

2. Does the paper present novel concepts, ideas, tools, or data? Yes

3. Are substantial conclusions reached? Yes

4. Are the scientific methods and assumptions valid and clearly outlined? No. A more clear explanation of scientific methods is needed. See general comments below.

5. Are the results sufficient to support the interpretations and conclusions? Yes

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? Yes

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? The tittle is confusing, as it appear the study uses a previous reanalysis (a "Last Millenium Reanalysis") as dataset for the study, while with "Last Millenium Reanalysis", as it is made clear later, they refer to an an ensemble approach to do global past climate reanalyses. I would suggest to append "approach" to the current title.

9. Does the abstract provide a concise and complete summary? From the abstract only, and even in the paper, it is unclear until page 3 in the manuscript whether the authors have done any new assimilation in this study, or used a previous reanalysis product (LMR) as documented in Hakim et al. (2016). Later it is understood that the use the same LMR approach as in Hakim et al.(2016), but the author re-do the assimilation step. This should be made more clear in the abstract.

10. Is the overall presentation well structured and clear? Yes

11. Is the language fluent and precise? Yes

12. Are mathematical formulae, symbols, abbreviations, and units correctly defined

and used? Yes, in general.

13. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? See specific comments below.

14. Are the number and quality of references appropriate? Basic references to the data assimilation method are needed, as it is the base for the reanalysis on which the study is built upon.

15. Is the amount and quality of supplementary material appropriate? The authors indicate the use multitaper spectra to evaluate if amplitudes in the wavelet analysis are higher than red noise at 95% confidence level. I would suggest to include this analysis as supplementary material (if not in the manuscript itself).

General comments:

Section 2 (Methods) is poor. The authors should be more explanatory about some aspects in the assimilation approach. The assimilation approach itself is unclear. For example, they do not mention the word "ensemble" at all in the manuscript. Thus it would appear they use a standard Kalman filter, which is unfeasible given the size of the problem. One need to resort to Hakim et al.(2016) too often to understand the basics of their assimilation approach.

Hakim et al.(2016) use a square root Kalman Filter (Whitaker and Hamill, 2002), an ensemble approach, at the core of their assimilation method. If this paper also uses the same approach, the authors should at least include at this reference, indicate the ensemble size, and explain the ensemble generation method for the background fields. Lines p4.l23-24 indicate they used CCSM4 from CMIP5 as background. How exactly they transformed a single simulation in a background ensemble? Although all these could potentially assumed similar to Hakim el al.(2016) it should be described here as well, as this is strongly related to the results.

Also the explanation of the forward model H would benefit from stating more clearly what is H in general and how it was explicitly obtained here (1st paragraph in Section 2.4 in Hakim et al (2016) explains this, but at least the should summarise it here). Also as Hakim et al. (2016) mention, temperature alone is not a good predictor of tree-ring widths everywhere. This, for example, will result in the linear model regression for the tree-ring width with temperature as the only predictor variable having very high regression residuals. There is not really anything terrible in this in the DA context, as by construction the R matrix originates not only from measurement errors (i.e. errors in the proxy data), but also from errors in the forward operator H, as is then this case. But please note down, as this is not evident for readers not so familiar with assimilation.

I am also wondering about the general use of the linear model as H for all proxies. Have other relationships, apart from the linear one with temperature, been explored for H in the LMR? This could be e.g. a nonlinear proxy$\sim$T relationship, or the possible use of other state variables (moisture) or geographical coordinates as predictor variables (e.g. latitude —see related note by the Editor—).

A note is the background state for the LMR as described in Hakim et al.(2016) is biased by design (as temporal trend are removed from the background state), and it is up to the proxy data to bring the re-analysed fields to somewhere in between the two sources of information (the background and the observations). Hence the resulting reanalysis will give, also by design, a "smoothed" version of the variability indicated by the proxy observations. The authors should comment on this, as this would be indeed a factor leading to reduced amplitudes in the higher frequencies of the resulting time series, which can also explain that the multidecadal variability is not so evident in this reanalysis.

Also, if I have understood it right, the regression in Fig.2 (and related results) is based on the single member for CCSM while it is based on the mean of the ensemble for LMR. Right? Please, state more clearly.

A figure showing the mean and the variance of the ensemble background state (i.e.

prior ccsm4) for SST and surface temperature over the world would be helpful (for comparison with Figure 2).

On the other hand, I find the remaining Sections in the manuscript quite clear and adequate.

Specific comments:

p4.l5. The explanation for the calculation of the B matrix is certainly wrong "(computed as the sample mean of x_b)". Correct.

p4.l32. Clarify why you chose these parameters (specifically the 31 coefficients) for the Lanczos filter. Add references as needed.

p5.l6. As the wavelet transform is a series of bandpass filters, it is better to give also here the range for the scales used in the transform, apart from spacing between scales (even if it can be understood form Figures later on). Also, indicate units.

Figure 1. y-axis seems wrong in Fig. 1a. If not, explain units.

p6.12. As mentioned, the weaker amplitude in the LMR with respect to the other re-analyses (Fig. 1c) is likely related to my previous general note about the background state generation, related to temporally de-trending the background. The authors should could comment on this, and in other related discussion throughout the paper.

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