

# “Atlantic Multidecadal Variability from the Last Millennium Reanalysis”

Singh et al

## Authors' Responses to Reviewers

We thank the reviewers for taking the time to provide detailed comments on our manuscript. The reviewers' original comments are shown in **black** with our responses in **blue**. All line numbers refer to those in the original manuscript.

In response to the reviewers comments, we have now added a new Supplemental Information (SI) addendum to the manuscript. As described in our responses below, the SI includes figures and text describing the CCSM4 mean state and variance, and multitaper spectra of the AMO index computed from the LMR.

### Reviewer #1

Dr. Garcia-Pintado

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.

Thank you for this thorough and helpful review. We think that revisions made in response to your criticisms and suggestions (described below) have made the manuscript both more readable and specific.

Answers to specific points:

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 title 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.

We have revised the title of the manuscript to “Atlantic Multidecadal Variability using the Last Millennium Reanalysis Framework.”

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 they 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).

We have added a Supplemental Information section that includes the multitaper spectra of the different reconstructed AMO indices described in the manuscript.

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.

Thank you for this critical comment on the methods. We didn't want to burden this paper with a repeat of details presented elsewhere, but we realize now that the description of the method was too brief and as a result unclear. In response to this, and a comment from reviewer #2, we have added new material near line 80 to describe the data assimilation approach in words, and to map out some details of the calculation that are described subsequently. Lines 100-110 have been rewritten and expanded to include information on the ensemble approach to the prior, and the ensemble square-root solution method. We provide sufficient detail that a reader familiar with ensemble filters should be able to reproduce the results, and references for more details for readers less familiar with this 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 p4.123-24 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 et al.(2016) it should be described here as well, as this is strongly related to the results.

All of these issues are now clarified in the revised text as described above.

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.

As part of the revisions described below, we have added to the description of the observation model, and note the additional sources of error that contribute to matrix  $R$ .

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- $T$  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—).

We note here that recent (e.g., Dee et al. 2016; doi: 10.1002/2016MS000677) and ongoing research has been devoted to exploring the impact of bivariate and more complex observation models for trees, but that is beyond the scope of the present paper.

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.

We have added a paragraph discussing this exact point. In particular, we note that while the reviewer's interpretation applies to a single proxy, the reconstruction at each location in fact depends on many proxies having unequal weighting. As a result, the temporal variability in the reconstructed time series at a point can be larger or smaller than that of a single record at that point. Validation of the results provides a way to know if the weighting, in aggregate, is appropriate. We have added several sentences to discuss how this method has been validated against instrumental data and independent (not assimilated) proxies.

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.

This is correct. We have clarified this in the Methods section.

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).

We have included a new figure in the Supplemental Information section showing the mean and variance of the surface temperature and SST in the CCSM4.

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

Specific comments:

p4.I5. The explanation for the calculation of the B matrix is certainly wrong "(computed as the sample mean of x\_b)". Correct.

Yes, thank you. This has been deleted.

p4.I32. Clarify why you chose these parameters (specifically the 31 coefficients) for the Lanczos filter. Add references as needed.

The Lanczos filter has superior spectral properties for the current application, and the coefficients are simple to compute. We have added a reference to Luchan (1979) for further information regarding the Lanczos filter and computing the filter weights.

p5.I6. 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 from Figures later on). Also, indicate units.

The range of scales (2 to 1000 yrs, with only 2 to 150 yrs shown) and the units (in units of variance of the AMO index,  $K^2$ ) have now been included in the caption to Figure 9.

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

We had (incorrectly) not removed the mean temperature from the area-averaged north Atlantic surface temperatures when plotting the AMO index in Figure 1a. We have corrected this in the revised Figure 1.

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 comment on this, and in other related discussion throughout the paper.

We have included this caveat in the revised manuscript:

*We note that because the prior is the same for every year, all temporal variability is determined by the proxies. As a result, the weighting in equation (1) involves a temporally invariant prior and temporally variable proxies so that, for any given proxy, reduced temporal variability may be expected. However, since the time series at any point in the reconstruction depends on the prior and many proxies, all having different errors, the variability at a proxy location may in fact be larger or smaller than that of a given proxy at that point.*

## Reviewer #2

Anonymous Referee

### Overview

In this paper, Singh and coauthors examine the characteristics of North Atlantic Multidecadal SST variability in the Last Millennium Reanalysis (LMR). The LMR is a simulation carried out with CCSM4 using data assimilation from proxies over the years 0-2000CE, and is mainly documented in a previous study. They find some known changes associated with the Atlantic Multidecadal Variability (AMV), e.g.: warming of the northern hemisphere, decrease in sea ice and northward shift in the ITCZ, and some unknown changes, in particular that there is a difference in the ocean heat transport between CCSM4 and the LMR. In addition, they carry out a wavelet analysis and show that there is no distinct peak at multidecadal timescales in the AMV index in the LMR, consistent with the notion that the AMV may be interpreted as a red noise process (which is in contrast to many modeling studies argue that there is multi-decadal peak in the AMV driven by oscillations in the AMOC). I think the results are interesting and fit in the broader discussion of the mechanisms and timescales of the AMV. The paper is well written, however I recommend some changes to improve the clarity in the presentation, which I elaborate in more detail below.

### General comments

1. It is quite hard to understand for a general audience how this data assimilation works, how it compares with other studies, and in particular why there are so many differences with CCSM4 prior. When discussing ocean and heat transports I don't understand why LMR is so different from CCSM4. In general, I find it hard to follow because there is no comparison with mechanisms operating in the real world and would be easier to understand if it was compared with direct observations (even if we know that observations over the 20th century are affected by anthropogenic forcings).

Thank you for this comment. We have rewritten portions of the method section to make the material more accessible for non specialists, and also to provide more details for readers that may wish to repeat these calculations. As for why the LMR is different than the CCSM4, it derives from the information obtained from the proxies. As we describe in the revisions, all temporal variability comes from the proxies (not from the prior), which is one reason they are different. Another is that the proxies also change the spatial covariance information (i.e., the prior and posterior state-covariance matrices differ as a result of the information in the proxies). Comprehensive analysis of the differences in dynamics is beyond the scope of this paper, but an area of current research.

2. In the paper AMV and AMO are used interchangeably, making it quite confusing to understand what the authors refer to. AMV is typically used to refer to internal+forced North Atlantic variability while AMO is used to refer to internal variability only (see Booth 2015).

We agree that AMV would imply both forced and unforced variability. We have removed most references to AMV, unless the reference is to both forced and unforced variability.

3. The AMV index is defined as the area average SST index from 120W to 0 and from 0 to 60N, and some studies are referenced including Clement et al. 2015. Clement et al. used 80W-0,0-60N as well as other studies which use 70/75W. In other words, 120W seems too far west, including a lot of not ocean (land) areas. The authors should check if their results are consistent when using the most commonly utilized index (80W-0,0- 60N).

The 120W is an unfortunate misprint in the manuscript. We computed the AMO index identically to that described in Clement et al (2015), with the western border of the bounding region at 80W, not 120W. We apologize for the confusion.

4. A lot of times the authors generally refer to similarities amongst figures in a very qualitative and general way. They have to be more quantitative and compute pattern correlations between each figure in order to draw any solid conclusion.

In our manuscript, we are not making quantitative claims about the similarities and differences between climate variables, only qualitative ones. Therefore, in the original manuscript, we chose not to include any quantitative measures when we compared fields between the various LMR time periods and the CCSM4 prior. We are happy to compute pattern correlations in any cases where the reviewer believes that it is necessary because we are making quantitative claims.

#### Specific comments

P1L20: Kushnir 1994 analysis was based on the hypotheses of the earlier Bjerknes 1964 paper  
We have now referenced both Kushnir (1994) and Bjerknes (1964).

P2L30: please cite also Murphy et al. 2017 and Bellucci et al. 2017 which are more recent papers addressing the role of external radiative forcing in driving the AMV  
We have added references to Murphy et al (2017) and Bellucci et al (2017).

P2L10: "Several dynamical studies have suggested that zonal and meridional oscillations in the AMOC on multidecadal time scales may drive changes in north Atlantic SSTs (Dijkstra et al., 2006, 2008)." Can you elaborate

what you mean by “zonal oscillations in the AMOC”? I am not aware of any zonal variations, usually the AMOC is seen as a meridional source of heat transport changes.

The description of the how zonal and meridional changes in the overturning drive a multidecadal oscillation in an ocean-only model may be found in Te Raa & Dijkstra (2002). We have included this reference and re-worded this sentence as follows:

*Several dynamical studies, using both ocean-only and fully-coupled models, have suggested that zonal and meridional variations in the AMOC on multidecadal time scales may drive changes in north Atlantic SSTs (for a description of the dynamical mechanism, see Te Raa & Dijkstra 2002; see also Dijkstra et al 2006, 2008).*

P2L20: Tandon and Kushner 2015 also find that there’s a lot of inter-model spread in the lead-lag correlation between the AMV and AMOC indices

We agree. We have added this caveat to the description of the Tandon & Kushner (2015) study.

P6L25: The fact that the anomalies are smaller after low-pass filtering is expected as a consequence of the low-pass filter.

We don’t think that this must be the case. In general, correlations will be dominated by high-frequency variability. If the high-frequency variability is uncorrelated with an index, but low-frequency is strongly correlated, it is possible to obtain higher correlations following filtering. We agree that in this case, the anomalies are, indeed, smaller following the low-pass filtering.

## References

Bellucci A., A. Mariotti, and S. Gualdi, 2017: The role of forcings in the 20th century North Atlantic multi-decadal variability: the 1940-1975 North Atlantic cooling case study. <https://doi.org/10.1175/JCLI-D-16-0301.1>

Bjerknes, J., 1964: Atlantic air sea interaction. *Adv. Geophys.*, 10, 1–82.

Booth, B. B., 2015: Why the Pacific is cool, *Science*, 347 (6225), 952, doi: 10.1126/science.aaa4840

Murphy, L.N., Bellomo, K. Cane, M., and A. Clement, 2017: The Role of Historical Forcings in Simulating the Observed Atlantic Multidecadal Oscillation. *Geophys. Res. Lett.*, 44, 2472-2480, doi: 10.1002/2016GL071337.