Clim. Past Discuss.,
https://doi.org/10.5194/cp-2017-41-AC2, 2017

Climate of the Past
Discussions

Interactive comment on "Reconstructing Late Holocene North Atlantic atmospheric circulation changes using functional paleoclimate networks" *by* Jasper G. Franke et al.

Jasper G. Franke et al.

jasper.franke@pik-potsdam.de

Received and published: 15 June 2017

This paper presents an interesting statistical methodology to analyze links between different proxy records through a functional network. The authors apply it to a few continental data centered around the east side of the North Atlantic region and covering the last two millennia. Then, they hypothesize that the main network patterns found are related with NAO variations, and use this possibility to try to reconstruct the main signs of NAO variations over the last two millennia. I have found the paper well-written and mainly clear, except at a few occasions where clear definitions were missing I think. The new statistical approach is well explained and sounds promising. It indeed

provides an original viewpoint concerning the variations of different records over the last two millennia, the non-stationarity of the links, etc. In that sense, this paper is interesting and deserves to be published.

We are grateful to the reviewer for this positive overall judgement of our manuscript.

Nevertheless, I have found the scientific objective of the paper a bit blurry so that it needs to be clarified. For instance, it is not very clear to me why the authors finally jump towards a NAO reconstruction, which is furthermore poorly validated when looking at the few tests they have performed. From my point of view, it would have been nice to better validate the model through the use of pseudo-proxy approach for instance within a climate model, to demonstrate already that we can reconstruct NAO sign variations within a climate model world. I understand that I am here asking possibly a lot, and the paper is already complex, but I should admit that I am not entirely convinced by the approach as it stands. If the authors want to reconstruct the NAO over two millennia, why do not they use classical methods and applied them to their data? This remained unclear to me what the functional network approach brings here.

As already acknowledged by the reviewer, we make use of the non-stationarity of the relationship between the NAO and the different paleoclimate records. Most classical methods (like linear regression) that we are aware of in turn assume a stationary relationship, which is not a realistic assumption in this case. Thus, our approach tries to extend the knowledge of past NAO variability by relying on exactly the characteristic non-stationary influence of the NAO on many paleoclimate records which is undesirable in applications of other methods.

The same problem also makes it very hard to perform a test using pseudo-proxies. We do not conceptualize the paleoclimate records under study as sole records of temperature + noise but as also sharing additional influences like extreme winter precipitation, connected to the NAO. Thus, one would have to use very elaborate pseudo-proxies, which are also able to capture such effects. While this is indeed a very interesting question, it clearly surpasses the scope of this research paper. We agree, however, that it would be a subject worth further investigations.

Specific comments

- p. 1, l. 6: "intimately": not sure this intimacy has been really proven. I will use another word here, or just remove it.

This term can indeed be deleted.

- p. 1, l. 8-9: "strong co-variability (. . .) as being indicative of a positive phase of the NAO". I think this link needs to be better demonstrated to support such a strong claim.

Indeed, "co-variability" is enough. We think that the performance of the model, though not perfect, justifies the statement, that certain cross link densities correlate with certain NAO phases and can thus be used as indicators.

- p. 1, l. 21: the authors cite here the AMO (also called AMV), but they do not discuss it any more afterwards. Why such a focus on the NAO, while the AMV could have played a strong role in past climate variability as well? Please clarify.

We focus on NAO mainly, because the AMO does not seem to be unambiguously reflected in the spatial co-variability represented by the data. Efforts to employ the same methodology for an AMO reconstruction did not yield any reasonable results.

- p. 7, l. 18: "|CMtw|". Can you define clearly what the "||" is meaning here.

This describes the cardinality, the number of members of a set, in this case the number of records in a cluster. We will clarify this in our revised manuscript.

- P. 8, l. 20: I think it is worth describing in a few sentences what the Louvain algorithm is.

This is a standard algorithm for identifying cluster structures in network science. We believe that explaining the details of this algorithm beyond just giving a corresponding reference might rather distract the presentation of our work than providing any relevant information to the reader.

"probably related with NAO": this is a hypothesis. . . Why not the AMV (at low frequency, can play a large role. . .)?

Of course, AMV can play a large role, too. However, as mentioned above, we have not been able to establish any connection to AMV using our network method. We emphasize that NAO and AMO have distinct spatial patterns, and the available set of proxies used in our study appears more suitable for reconstructing low-frequency variations of the NAO rather than AMV associated with other characteristic spatial co-variability patterns.

- P. 9, l. 12: why 50-year smooth. Have you tried other smoothing?

Yes. The results do not change much for larger time windows, for much smaller ones the uncertainty of the pearson correlation becomes too large. We will add a corresponding note to our revised manuscript.

- P. 9, l.14: How many data in your network and Ortega et al. (2015) reconstruction are in common. It is worth clearly specifying which.

We will specify this in the table of records in our revised manuscript.

- Figure 3: the colors and numbers of clusters is unclear. Please be more precise on the methods used to make this figure.

We will clarify this in our revised manuscript.

- P. 10, l. 4-9 and Figure 4: I find the interpretation and choice of the slots shown a bit subjective. How do you choose them? How do you identify the main patterns?

The patterns selected for these features indeed represent a subjective choice and are just meant to illustrate both the general patterns as well as the limitations when just looking at the networks alone. To make the associated statement more objective is actually the main reason for using the linear model.

- Figure 5: the legend is not detailed enough to understand how it has been computed.

The black circles represent the geographic centers of each cluster of proxies. Red links indicate pairs of spatial clusters for which the temporal variations of the corresponding CLDs correlate positively with the NAO phase, while the blue ones indicate negative correlations. The color and width of the links are determined by the sign and values of the regression coefficients of the linear model as given in Tab. S2. We will clarify this in our revised manuscript.

- P. 12, l. 1-8: I find the numbers for the validation a bit worrying. Even the 68 and 71% are small when considering that in fact you have always 50% chance of being in one phase or another. The authors claim "our results have a certain value". Can you develop a statistical test to be more convincing? Value for what?

As we already argued in the manuscript, most of the mismatch is due to a difference in transition times between positive and negative NAO phases in the Ortega reconstruction and our model (a problem, which has been solved by wiggle matching in previous reconstructions). Thus far, we are not aware of any alternative framework yielding more convincing results.

- P. 13, l. 1: How do you prove the claim from this first sentence? This is not clear to my eye when looking at Fig. S5. You should better support this interesting claim.

In Fig. 5 one can see that at several occasions there is a mismatch during a transition between NAO phases, but our reconstruction seems to be lagged w.r.t. the Ortega reconstruction (e.g. around 1200, 1500 and 1850 CE).

- P. 13, l. 2: "period of strong, persistent positive or negative NAO": can you provide a clear definition of what this is?

Here we mean, that the Ortega reconstruction is far from 0 and consistently in one phase during a certain time window, unlike e.g. 1400 CE. We agree that this sentence should be rephrased, stating that a good consistency between the Ortega reconstruction and our model is commonly accompanied during periods with persistent NAO phases.

- P. 13, l. 15: "degree of belief": can you provide a definition for this?

This is the probability, determined from the MCMC ensemble, that the NAO is in a specific phase. This can be derived immediately from the linear model. Through using a MCMC based estimator we achieve uncertainty estimates of our model parameters. We then draw a large number of realizations of the model parameters and calculate an estimate of the NAO index based on the observed CLDs. The percentage lower (higher) than 0 is interpreted as the probability, that the NAO was in a certain phase. We will state this more explicitly in our revised manuscript.

- P. 14, l. 30: "Supplementary Fig. S7": I think that taking 50 years is maybe too short here, and can easily induce artificial non-stationarity just through low frequency.

We use 50-year windows to generate the network representations and, thus, need to use a similar time scale to demonstrate the non-stationarity of the relationship between the considered proxy records and the NAO. While it is true, that the estimated correlations will exhibit a certain temporal variability due to the short window size, this type of non-stationarity is not the main point here. What we want to demonstrate is the absence of a correlation between the records and the Ortega reconstruction at many times, which we believe cannot be an effect of the windowed analysis alone. This type of non-stationarity (correlation only at certain times) makes a direct, linear regression unsuitable.

- P. 15, l. 7: I do not understand well what is the final MCMC regression model and how the r2 of 0.58 is computed, can you please clarify?

The final regression model is the linear model given in eq. 5. The model parameters are estimated using an MCMC approach. The r^2 is given by the square of the correlation of the fitted model to the Ortega reconstruction. In any way, we argue, that this number might be due to overfitting of high-frequency variability, as the r^2 in the cross-validation is considerably lower.

- P. 15, l. 14-15: The claim that low-frequency temperature variations are related with solar and volcanism is not so clear in my mind, and on the opposite, there is a large debate on that. See for instance Schurer et al. (2013) or PAGES2K-PMIP3 group (2015). The internal climate variability could have played a large role as well in the last two millennia, even to explain little ice age and medieval climate anomaly.

This formulation is indeed somewhat misleading. What we wanted to express here is, that while the NAO has an influence on low-frequency temperature variations there are other factors, like solar forcing, volcanism and other modes of internal variability which might be equally or even more important. Thus, a discrepancy between the observed temperature and the temperature expected from a reconstructed NAO phase is not problematic. We will clarify this in our revised manuscript.

- P. 17, l. 12: "most droughts indeed coincide". In a paper with such advanced statistical tools, I'm surprised to read that. Can you quantify this more precisely, to improve my degree of belief in this claim?

Out of the 20 drought events there is a tendency towards a positive NAO (P(NAO+) > 0.5) in 17 cases, with 12 of these with P(NAO+) > 0.66. We will add this information in our revised manuscript.

- P. 19, l. 2: "most likely": is there any statistical test supporting this adverb?

Here it is not meant to be a statistical term or represent a clear gradation of likelihood like used in the regular IPCC reports. We will clarify this in a revised manuscript.

- P. 19, l. 14: "Thus, our approach cannot yet be directly applied to the instrumental

record as regression target". Why that? Indeed, this would have been nice to further test the method on instrumental record (cf. pseudo-proxy approach from my main comments).

The main reason is, that thus far we have only been able to perform this analysis for windows of 50 years length. As the instrumental record goes back to 1821 (Vinther et al 2003) this leaves about 4 independent data points for the regression. If one is able to reduce the window size and complexity of the regression model further, one might be able to perform the analysis on the instrumental record, which is highly desirable. We will clarify this in our revised manuscript.

Bibliography: PAGES 2k–PMIP3 group (2015) Continental-scale temperature variability in PMIP3 simulations and PAGES 2k regional temperature reconstructions over the past millennium. Clim. Past, 11, 1673–1699, 2015 Schurer, A. P., S. F. B. Tett, et G. C. Hegerl, 2014 : Small influence of solar variability on climate over the past millennium. Nature Geoscience, 7 (2), 1–5, doi :10.1038/ngeo2040

We thank the reviewer for the suggestions and will consider them in a revised manuscript.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-41, 2017.