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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.

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The authors use a network approach to analyze the connections in a set of paleo-records in the North Atlantic region. The connectivity in the network is then related to a previous NAO reconstruction. This relation is used to expand the NAO reconstruction back in time. The improvement of reconstruction techniques is an important subject and the NAO is a dominant mode of variability. I don't doubt that the authors have a deep knowledge of networks and that the analysis is well performed. However, there are many steps in the analysis which are not familiar to the average reader. I will strongly suggest that the authors try – wherever possible – to relate the network prop-

erties to more physical properties. If the paper gets too long they could delete section 5.4 which seems a bit out of topic.

In many cases, giving direct physical (climatological) interpretations of specific network characteristics is difficult. For example, a link between two records (nodes) does not necessarily imply similar "internal" temperature variability, but could also originate from a similar influence of extremes in winter precipitation (for some proxies) or a shared external forcing. Links between clusters are just aggregated connections between climatologically meaningful regions with the aim of defining and calculating a more robust measure that minimizes local effects. However, it is still difficult to directly attribute a strong linkage to a physical property. We will try to explain the meaning of network properties in each step in more detail in our revised manuscript and hope that this will make it clearer to a general audience.

So, as I see it. the paper certainly deserves to be published but it could benefit from a more pedagogical approach.

We thank the reviewer for this positive overall recommendation.

1) While I in general find that the paper is well written I also find that it is very technical. The analysis includes several steps and it is not always easy to see the physical content. For example, what does Fig. 5 actually mean? It looks a little as the impact of the NAO on the temperature; negative correlations between the NAO and temperatures in middle Europe and positive correlations in Greenland and Scandinavia. But I guess it is more a picture of how tele-connection (or coherent?) patterns depend on the NAO which in itself can be seen as a tele-connection? Perhaps the authors could use observed temperatures to demonstrate how the tele-connection patterns look in the two phases of the NAO? The spatial coherence is already used for the spatial clustering of proxies.

Figure 5 shows the correlation of the connectivity between groups of records with the NAO reconstruction by Ortega et al. This is not directly a (temperature related) telecon-

nection between different regions, but potentially influenced by secondary effects, like winter precipitation extremes in tree ring records, as well. More explicitly, thick red lines indicate that positive NAO phases coincide with relatively many cross-cluster links, the number of which is reduced during negative NAO phases. For blue links, the relation is just the opposite (more links during negative NAO phases than during positive ones).

More generally, I think it would be good if the authors tried (even more) to relate the network results to quantities of a more simple and well-known character.

Please, see the response to the general comment.

2) I noticed that the reconstruction does not perform well regarding the correlation in the cross-validation test. Nonetheless, the authors use it to predict the sign of the NAO for which the method seems to be correct about 70 % of the time. I don't really understand the explanation the authors give (p13). It would help if a figure of the Ortega reconstruction and the new reconstruction was shown.

We will exchange Fig. 6 by the attached figure in which the Ortega reconstruction is included.

On p13, we refer to Fig. S5 to explain the mismatch in sign due to different timings of transitions. There one can see, that there is a mismatch leading to a lower prediction rate of correct signs around the periods characterized by rapid transitions between two different types of NAO phases (like the one happening during the 12th century). Nevertheless, the transitions themselves are resolved correctly in most cases.

As for the comparison of the present reconstruction with other reconstructions it should be noted that both the reconstruction methodology and the proxy selection will be important. I would suggest that the authors produce a reconstruction from their proxies using a simple multiple regression scheme between the NAO and the proxies. This might help getting an idea of which improvements the network method actually brings.

If one uses the proxy network used in this study and instrumental NAO data as cali-

bration data this would basically result in a reconstruction similar to the one by Ortega et al. They have chosen the most suitable paleoclimate records in the region to fit to an instrumental NAO time series. The data underlying the Ortega reconstruction have a significant overlap with our selection. Our approach is to use precisely the non-stationary relationship between many paleoclimate archives and the NAO. Thus, any linear regression with this extended data set cannot yield the same results as the network based study, since most of the records do not have a stationary correlation with the NAO. They are thus unsuitable for a linear regression, which relies on a stationary relationship.

3) Section 3.2: It seems that the similarity is defined from the p-values alone. In my understanding it should be based on a combination of the size of the correlation and the p-value. As a large correlation can be insignificant so can a small p-value be connected to a weak correlation.

Correlation values can be artificially elevated by large persistence of individual proxies (e.g. in case of prominent low-frequency variations). This effect would be proxy- and site-specific. Therefore, taking the correlation value itself into account is not meaningful.

The similarity does not seem to take the sign of the correlation into account. From a physical point of view there is a big difference if two point are positively or negatively correlated. So is not a lot of information lost in this process?

As we argue in the manuscript, different proxy types can indeed lead to different signs and magnitudes of correlations. It might be possible to include the correlation strength (preferably measured by the corresponding p-value as a measure of statistical significance of the existence of a pairwise correlation) into our regression framework, but for simplicity reasons, we preferred to follow a binary approach.

When it comes to the NAO, the absolute values of correlations need to be taken into account in our regression framework (with the number of significant pairwise correla-

tions rather than their directionality as predictors), since the NAO can have opposite yet significant effects in different regions (like higher temperatures in Greenland and lower ones in Fennoscandia) for the same phase. Specifically, in our approach, we are only interested in the existence of a linkage at a given point in time, rather than its specific form.

4) Introduction, page 2: Networks probably have some advantages in some situations. However, networks were developed for studies of discrete phenomena such as those in sociology. In the study of climate we deal with fields that are continuous in both space and time. It therefore seems backwards to reduce the problem to a network. We must loose information that other methods based on fields take into account. I know that the present paper is not the place for a philosophical discussion but the concern could be mentioned.

We appreciate this comment. However, one could also argue vice versa, that a network is only using the information which is actually present in the given data set, without consideration of non-sampled regions. The latter would be (at least implicitly) the case in climate field reconstruction methods. In this sense, we consider the spatially discrete network structure as a more honest representation of the sparse data that we have in a paleoclimate context rather than a reduction or a step backwards. We mention this difference in philosophy in line 10 and will elaborate on this in more detail in our revised manuscript.

Is the A in Eq. 1 used anywhere?

Yes, for example in Eq. 4. We are confident that using this mathematical symbol, which is standard in complex network theory, will foster an easy access to our manuscript for readers that are familiar with networks. Therefore, we would like to keep both the mathematical and the verbal/intuitive representation of our formalism as parts of the manuscript.

Caption to Fig. 4 should be improved.

We will provide a more detailed caption in our revised manuscript.

Page 7, top: I don't see how the AAFT procedure can be applied to the 4 incomplete proxies. The AAFT includes a Fourier transform.

Data gaps have been filled by linear interpolation. As we are dealing with regularly sampled data with a very low number of missing data points in the first few centuries only, we are confident that this interpolation does not alter the overall results significantly.

Fig. 5: The bright areas are not easy to see. By the way: Is CE an accepted standard? It always takes me a while to figure out the direction of the axis.

We could of course make the bright areas darker, but they are meant to be in contrast to the dark areas, as they mark periods at which the probability for one or the other phase is not high enough to make any conclusive statements. We furthermore think that CE (Common Era) is nowadays a well established time scale without explicit religious connotation, and the direction of the axis is used in most publications on Late Holocene climate dynamics.

P9,l5: More recent and complete references are Christiansen 2014 (10.1175/JCLI-D-13-00299.1) and Christiansen and Ljungqvist 2017 (10.1002/2016RG000521).

We thank the reviewer for this suggestion. We will update the list of references accordingly.

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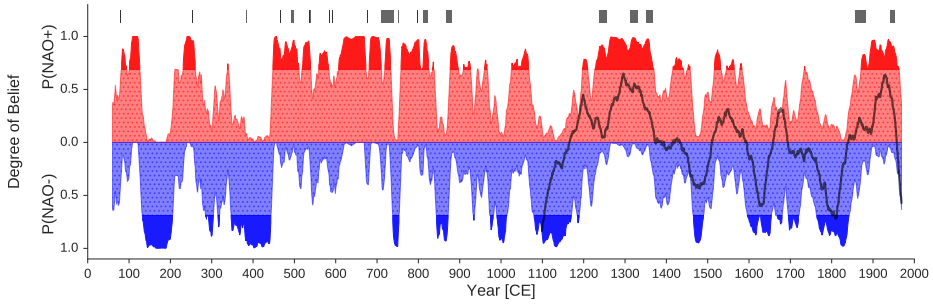
Degree of Belief
P(NAO+)
P(NAO-)
1.0
0.5
0.0
0.5
1.0
0
100
200
300
400
500
600
700
800
900
1000
1100
1200
1300
1400
1500
1600
1700
1800
1900
2000
Year [CE]

Fig. 1.