

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

**Anonymous Referee #2**

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### General comments

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

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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.

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.

### 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.

- 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.

- 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.

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

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

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- P. 9, l. 9: “probably related with NAO”: this is a hypothesis... Why not the AMV (at low frequency, can play a large role...)?
- P. 9, l. 12: why 50-year smooth. Have you tried other smoothing?
- 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.
- Figure 3: the colors and numbers of clusters is unclear. Please be more precise on the methods used to make this figure.
- 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?
- Figure 5: the legend is not detailed enough to understand how it has been computed.
- 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?
- 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.
- P. 13, l. 2: “period of strong, persistent positive or negative NAO”: can you provide a clear definition of what this is?
- P. 13, l. 15: “degree of belief”: can you provide a definition for this?
- 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.
- P. 15, l. 7: I do not understand well what is the final MCMC regression model and how the  $r^2$  of 0.58 is computed, can you please clarify?
- P. 15, l. 14-15: The claim that low-frequency temperature variations are related with

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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...

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

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.

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

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