Dear Prof. Goose,
thank you again for inviting a revised manuscript. We have tried to address all further comments and questions from the reviewers. The modified parts have been marked in the revised manuscript.

Response to reviewer 1

This is my second review of this paper. The paper has strongly improved following the first round of review and is now in very good shape, almost ready for publication. It will constitute an interesting input to existing literature on the subject. Nevertheless, I still have a few suggestions for clarifications, which may be mandatory from my point of view, to allow it to be published.

We thank the reviewer for their overall positive response.

1) P. 2, L. 24-26, the AMO potential impacts are claimed to be not "consistently represented in the proxy data", but the reader is left to understand what this is meaning and how such a strong conclusion has been reached. Furthermore, it is a bit weird to have this claim concerning a preliminary result already in the introduction. I would advise to clearly explain in a paragraph or so why the AMV is not consistently represented in the proxy data (which analysis, metrics used…) and to move this in the results section of the analysis

We agree that mentioning this point in the introductory section could be considered a misplacement of information. We have therefore decided to shift the statement mentioning the negative results for AMO/AMV to the end of the conclusions section. Specifically, by applying exactly the same procedure that has been successfully used in our manuscript for extending an existing benchmark NAO reconstruction backwards in time to a similar long-term reconstruction of AMV, we obtained results which did not perform better than a random prediction when comparing the modeled and reference AMV phase. From this observation, we have to tentatively conclude that the considered combination of paleoclimate archives did not allow for modeling the AMV index as target variable using our analysis method. We believe that this finding is important enough (as a cautionary note) to be stated at a prominent place in the manuscript, and have therefore added a corresponding paragraph in the conclusions section. In turn, we think that discussing such negative results in more detail in the results section would not be very helpful.

2) P. 3, L. 32: "stationary manner". I do not get why the authors are doing this claim. Pseudo-proxy approaches are allowing to apply a given statistical methodology to output of climate models, where the true NAO is known, to see notably if there is a kind of stationarity in the reconstruction quality. It is not assuming any stationarity hypothesis. This is mainly a way to test statistical methodology and see within a model world if it works properly when everything is known i.e. reconstructing an index from a few locations, while the dynamical index is known and this, for a long timeframe (last millennium simulations for instance). Can you please further clarify what you have in mind here?

We apologize for any possible misunderstanding regarding this point. In fact, we fully agree that it is possible to have pseudo-proxies also with non-stationary relationships to a target variable, even if they of course have a stationary relationship to the local climate variables.

We argue, that the actual relationship between the NAO index (target variable) and the multiplicity of terrestrial paleoclimate archives considered in our study is potentially complex and in most cases (except for some of the ice core records) not sufficiently well constrained to infer a particular statistical model. In this regard, it would not be sufficient to construct a pseudo-proxy by some relationship to a single model output variable, but actually necessary to model each proxy as a result of the combined action of different variables like summer temperature, precipitation (in our case likely winter extremes) and others. The precise extent to which each variable contributes to a specific archive in a possibly non-stationary manner is in our view not known well enough and would demand sophisticated forward models for all types of paleoclimate archives used in this study. We are not aware of previous studies describing the application of pseudo-proxies that exhibit the corresponding degree of complexity. In our opinion, developing (and subsequently applying) sophisticated pseudo-proxies which are able to reflect this complex relationship (possibly mediated through extreme rainfall, strong storms, etc.) would rather justify a separate study and would expand the present paper beyond reasonable limits. We have clarified this point in our revised manuscript.

3) P. 12, L. 19: Usually the calibration/validation approach is made with an ensemble approach (through random selection of different independent calibration and validation periods), leading to a distribution of r2 that allows to have a better idea on the performance of the statistical model used.

In the paleoclimate literature, splitting the records into two pieces is a standard procedure as well. As we have relatively few independent data points, an ensemble approach (e.g. using block-bootstrapping) as suggested by the reviewer actually yields very similar results. Consequently, we think that splitting the full time interval into two halves provides more insights into the performance of our approach, since the number of records and potentially also the quality of the individual reconstructions decrease as one goes back in time. In turn, this intrinsic difference between different time periods would be widely overlooked in an ensemble approach. Therefore, we prefer to maintain the approach as described in our manuscript.

Response to reviewer 2

In my review of the original version I suggested that the authors tried to relate the network properties to more physical properties and in general to take a more pedagogical approach.

I am happy that the authors have followed these suggestions. While the paper is still challenging in all its technical details it is now much more easy to understand the physical reasoning.

As far as I can see the authors have also satisfactorily addressed the comments and suggestions from the other reviewers.

I will therefore now recommend that the paper is accepted for publication.

We thank the reviewer for their overall positive recommendation.

We attempted to clarify this paragraph. It now reads:
"For example, a persistent positive phase of the NAO can enhance winter precipitation in Northern Europe, which in turn has an indirect influence on tree growth during the subsequent summer. The corresponding opposite effect of a negative NAO phase is expected to be much smaller. A similar relationship is expected to be present in Central and Southern Europe, but here increased precipitation is commonly associated with negative NAO phases, while positive NAO phases foster dry conditions and even droughts."

We have added a corresponding remark, that the time series x and y are normalized to keep the notation simple.

Complex network theory provides a great variety of measures for characterizing network properties (clustering coefficients, network transitivity, betweenness centrality, etc.), which have already been utilized in the context of functional climate network analysis in applications to reanalysis data, climate model outputs or dense station grids. All these methods typically request a sufficient number of individual grid points for their appropriate estimation and interpretation, which is not provided in the case of our paleoclimate network. We have further clarified this point in our revised manuscript.

We have removed the second "size".

There are indeed more regions highlighted by our cluster analysis. However, some of them do not contain proxy records. In turn, we think that it is important to show the whole domain on which we have applied our cluster analysis for transparency and reproducibility reasons.

We have deleted this opening.