

Interactive comment on “Pseudo-proxy evaluation of Climate Field Reconstruction methods of North Atlantic climate based on an annually resolved marine proxy network” by Maria Pyrina et al.

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Answer by M. Pyrina et al. to: Anonymous Referee #2

We would like to thank the Referee #2 for the constructive comments. We agree that our focus area is smaller compared to other studies that evaluate the spatial skill of reconstruction methods, but even though the region is smaller it regards the marine environment. This is a region that lacks past information with high temporal resolution and the reconstruction methods are tested in this region for the first time in the context of an absolutely dated annually resolved marine proxy. Even though the aforementioned differences to other studies are stated in the introduction we will reformulate the

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introduction in order to account for the broader significance of the work in a more clear way. In some cases we will include more results and further discussion, as suggested by the reviewer, and provide more explanations of our findings.

Major comments:

1) The pseudo-proxy experiments seem to be done for only one realization of the AR1 noise even though several models are used. Many of the conclusions – which are based on rather small differences in the correlation maps – could be different if another noise realization was used as shown in, e.g., Christiansen et al. 2009 (doi: 10.1175/2008JCLI2301.1). Preferably an ensemble of realizations should be used. Alternatively different realizations should be performed and the (possible) differences discussed.

Even though we think that an additional noise realization of the AR1 model will not produce results that will change the main conclusions of our work we will conduct our experiments with additional noise realizations of the AR1 model and include the results in the text. In this context we will choose a range of the parameter of the upper AR1 model that can be realistically expected from the real noise level contained in the proxy data.

2) Both the considered methods depend on EOF analysis and section 4.1 discusses the stationarity of the calibration coefficients. However, the paper does not include any comparison of the EOFs or the spatial correlation structure. I would advise the authors to compare the EOFs from the different data-sets and the different periods. The stationarity of the teleconnections could also be investigated by looking at the map of correlations between a grid-point with proxy-data and all other points.

Comparing the EOFs of the different periods will not address the stationarity of the calibration coefficients, as the reconstruction was performed in every case by using the leading EOFs calculated in the calibration period. So, even though the individual EOFs might be different between different periods, the leading EOFs from i.e. the period

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1950-1999 will roughly capture the same co-variability as the leading EOFs of another period and therefore no conclusions about the stationarity of the calibration coefficients can be made by the EOFs. The Principal Component Regression as we use it is not so much dependent on the individual EOFs. We rather use the PCs to fit our model and the EOFs are just used as a way to reduce the dimensionality of the predictors in only a few patterns. With the leading EOFs we can reconstruct the original anomaly pattern and therefore using 10 EOFs will lead to >90% of variance represented, irrespective to the period the EOFs are based on (i.e. $X_i = \sum pc_{i,j} * eof_j$)] Regarding the investigation of the stationarity of the teleconnections the reviewer is right. Therefore, we will perform the required calculations and discuss these results in the text.

3) In the conclusions and the abstract it is mentioned that the marine network can produce skillful spatial reconstructions for the eastern NA basin. But even in this area there seems to be a massive underestimation of the amplitude. I think this underestimation in general could be described more in both the text and in the abstract. 4) I guess the reconstructions are best in the areas close to the proxies. But I don't think this is discussed much in the paper. The position of the proxies could be indicated on the maps. 6) There has been a discussion in the literature of the reconstruction methods ability to get the amplitude right. Methods with temperature as the dependent variable, as those used here, are prone to underestimate the variability (Christiansen and Ljungqvist 2017, doi: 10.1002/2016RG000521 and references therein). The results in this paper seem to get an underestimation of the variability even in the case with noise-free proxies. The reason for the underestimation of the variability should be investigated and discussed.

Regarding the major comments 3, 4 and 6 we plan to elaborate more on these in the discussion, abstract and conclusions as the reviewer suggested. The positions of the proxies are plotted in all of the maps shown, but as we now acknowledge it is hard to see. Therefore we plan to improve the figures of the main text.

5) It would be nice to see a plot of the time-series of the 5 real-world proxies. This would

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also allow the reader to judge if the AR1 process used for the pseudo-proxies is sound. By the way, I am surprised that the authors did not show a real-world reconstruction based on the 5 Arctica islandica.

The 5 Arctica time-series are already published and we have cited those papers for the interested readers in the lines 97-100. However, showing these series here would not help to decide whether the AR1 process to represent the noise in sound or not. For that purpose, an analysis of the residuals resulting from a regression between the real proxies and the local water temperatures would be required. Those series are short, and thus it would be in any case difficult to decide if an AR1 model is enough. However, in the revised version we will additionally perform the experiments using additional noise realizations of the AR1 model to better account for the spread in the noise within the different proxy locations.

Minor comments: line 35: I think the abbreviations such as NA and PPE should also be defined in the text and not just in the abstract. line 54: Perhaps another word than "aggravating" should be used here. line 92: grid point -> gridded. line 111 and 30: It is confusing that "reconstructions" are used for different things. line 119: Why 1999 and not 2005? line 142: A degree sign is missing. Section 2.2.1: It would be better if single-letter symbols would be used in the formulas instead of Proxy, EOF, CC etc. line 193: The sentence beginning with "The key .." does not make much sense to me. l205: The pseudo-proxy review Smerdon 2001 (doi: 10.1002/wcc.149) should be cited somewhere. line 200: With only five (or less in section 3.3) proxies it does not seem to make any sense to make a EOF transformation of the proxies and keep all five modes. This step is usually done to reduce the number of degrees of freedom which is not necessary here. This should be discussed. line 223: What are the correlation of the other 4 proxies? It could be noted that values

We agree with the reviewer regarding all of the minor comments. Concerning the comment on line 200, Canonical Correlation Analysis identifies the pairs of co-variability patterns between the North Atlantic SSTs and the proxy SSTs that have maximum

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temporal correlation. CCA involves the inversion of the co-variance matrix of each field and although it can theoretically be applied to the original fields, co-variance matrices of geophysical fields tend to be near-singular and therefore its direct inversion leads to numerical instabilities. Some sort of regularization is needed and this is usually achieved by a prior EOF analysis. The reviewer is right that for the proxy field with only 5 records it does not make a difference, but the EOF analysis of the proxies just simplifies the inversion of the co-variance matrix, since it brings it to a diagonal form.

The comment related to the single-letter symbols is in our opinion a matter of taste, but we agree with the reviewer to have a consistent naming of the symbols throughout the text. Therefore we will address these issues in the revised version of the manuscript.

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