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Climate of the Past Discussions

Interactive comment on "A spatio-temporal reconstruction of sea-surface temperatures in the North Atlantic during Dansgaard-Oeschger events 5–8" *by* Mari F. Jensen et al.

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I have reviewed the article entitled "A spatio-temporal reconstruction of sea-surface temperatures in the North Atlantic during Dansgaard-Oeschger events 5–8" by M. F. Jensen et al. The article performs a Proxy Surrogate Reconstruction based on a number of proxies from marine sediments and SST fields borrowed from coupled atmosphere-ocean climate model simulations. The authors evaluate the performance of the methodology and assess their main uncertainties. The final product enables the authors exploring some physical mechanisms that might be important in previous DO events.

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General Comment:

Although the methodology is not entirely new, it is to my knowledge the first time that this technique has been applied to reconstruct climate states so far from current conditions. Uncertainties are large and populate most aspects of the methodology that limit the robustness of the findings. Still, the method seems reasonably robust and the conclusions coherent. With a healthy level of caution, the methodology has the potential to shed light into complex and important physical mechanisms that remain poorly understood. The structure is sensible, the selection of figures adequate, and the text is easy to read. For these reasons, I think the manuscript merits being published, perhaps with few minor corrections that in my opinion could help to clarify some details of the methodology I found a bit confusing when reading it the fist time.

Specific comments:

- The abstracts reads a bit optimistic regarding the uncertainties. Contrary to the Conclusions section, it ends highlighting the robustness, rather than the limitations and this being an ‘encouraging fist attempt’. In my opinion, the conservative tone of the conclusions (e.g. Page 14, Lines 25-27) could be reproduced as well in the abstract.

- The paragraph in the Introduction starting in Page 3, Line 26 enumerates conclusions. Therefore, it does not introduce the problem, and I think this text should be moved to/merged with the final section

- The three first lines in Section 2.1 can be merged with the following paragraph to avoid such short one. Also, it is a bit misleading, as it is not clear whether it was Lorenz or Graham, the first study introducing the PSR. What “approach” exactly introduced Lorenz? (By the way, I know the answer: Lorenz introduced the analog method, while Graham introduced PSR)

- In Page 4, expressions in parenthesis such as “see below” “see Sect...” are a bit obvious and might be removed. Also, it reads “A collection of years from one or several model simulations are treated as pool...” I think this is not entirely correct. As far as I understood, a collection of years is used, together with a low-pass filter, to BUILD a climate state. The method is based on the search across such “climate states”, rather than on individual years. Am I right? If so, please clarify. If not, please clarify.

- Page 4, Lines 13-15: Be careful with this argument. Although in principle the method gives you a date, and you can draw any variable concurrent with that date from the GCMs pool, this does not mean that any variable can be reconstructed. Only those variables that are physically and strongly connected with SST, which is the only variable constrained by the proxy information, can be reconstructed to a certain degree. This pertains for example the test done with NGRIP temperatures in Section 4.3, which by the way could perhaps be evaluated using only synthetic data in Section 3.1.

- The name “Data pool” in section 2.2 is a bit misleading. Firstly, because models are also data, although they are not described here. Secondly, because I’m not sure if “pool” is an adequate naming for a set of marine records. I would call it “Proxy records” or something similar. Note however that this does not apply to section 2.3, whose name “Model Pool” is in its context accurate and descriptive.

- Page 6, Lines 19-20: How exactly is such a test carried out? Where are the results shown? I guess this pertains the gray shadow in Fig. 6 Is it so? I could not understand the details of how such a uncertainty interval was obtained.

- Page 6. Regarding the simulation, only 200 years out of the 500 available are used. Why so? (This also applies to Line 8 in Page 7) Given that the pool size is critical for analog-like reconstructions, wouldn’t it be better to keep as many years as possible? Even if such years are less reliable individually (for spinup considerations, I imagine), their inclusion in the pool enlarges its variability with more heterogeneous “climate states”. Therefore, it can only enrich the pool with heterodox, unlikely states which might be, perhaps even by chance, more adequate to reproduce the exceptional situations under DO events.

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- Page 7, Lines 5. The term “8 unforced simulations” is misleading. Here, it refers to the fresh water only, but readers might think that it refers to absence of forcings at all, including orbital, GHG, solar, etc. This is, “unforced” seems to mean pure control simulations, which as far as I understood is not what these simulations consists of. Note that this remark applies to many instances through the manuscript. I would advise reviewing every instance of the word “forcing” in the text and reword it accordingly to clarify that the term reefers to “normal salt concentration”, but the rest of the variables being normally forced.

- As a general note regarding Section 2.3, it would be nice to sum up how many years there are in total available within the pool. Anyway, it seems clear that there are fewer years than those that are being reconstructed. This is a rather undesirable situation (normally there are many more analogs available than necessary, and some authors still complain about the small size of the pool given the large dimensionality of the problem). Although I understand that this is unavoidable, perhaps it is worth to point this out, adding few words of caution that at least demonstrate that the authors are aware of this fact.

- Page 7, Lines 25- Why only 30? Are they continuous periods? Or are they 30 random, fully disconnected samples? In any case, why such a choice?

- I’m not sure if the design of the experiment in Section 3.1 is the best one. It uses the same model to produce the target and draw analogs. It does not even contaminate the synthetic proxies to mimic a more realistic scenario. Therefore, the results suppose a very optimistic upper bound, barely representative of the actual performance of the technique. With the available data, it is rather easy to design more illustrative experiments which lead to tighter bounds of the uncertainty: adding noise or using a different model (e.g. CCSM4) as target are some examples.

- Page 8, Line 11. The Fig. 4 is barely explained. After having read the paper a couple of times, I still do not fully understand it. I think more details should be provided.

- Page 8, Line 20. How is this correlation/standard deviation calculated? Over the 14 locations? Are the values shown in the Figure the results of such cuantities averaged over the full period? I think this part lacks details that facilitate the read of the conclusions.

- Page 8, Lines 27-30: I'm not sure if the inclusion of the “unfiltered data” is necessary. Such data is not described before (only averaged climate states are expected at this point), therefore it is a bit misleading. The test is in any case incorrect, as it makes little sense to compare proxy data (representative of low-pass frequency) with yearly averages, and the conclusion are rather trivial (obviously the year-to-year variability is larger than the low-pass filtered). Therefore, I think it would be better to remove such test from the text and Fig. 5 for the sake of clarity.

- Page 10, Line 13-15: Again, how is this interval exactly calculated. In Fig. 2. shouldn't the black line be included within the gray shadow?

- Fig 1: The figure lacks a colour scale.

- Fig. 2: Why are there gaps in some cores (6, 10)?

- Fig 4: The label reads “Euclidean distance”, rather than RMSE. This figure is hard to understand, and further details could be added to facilitate its read. For instance, are there 10 black dots per column? What is the number of rows? Further, this figure exhibits a lot of structure. The black dots are far from homogeneously distributed, which in my opinion deserves some more attention that the one demonstrated in the text.

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