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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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We thank the referee for his review and suggestions to improvements. Generally, we agree with all his suggestions and think they will help to clarify the manuscript. We provide a comment (marked with C) to all specific comments (reproduced, marked with R) below. The manuscript will be updated accordingly when the full review is available.

R: 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.

C: We agree with the referee and will change the tone of the abstract in the revised manuscript

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R: 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

C: We will move and merge the paragraph with the final section

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R: 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)

C: We will merge the two paragraphs and make clear that Lorenz introduced the analog method

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R: 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.

C: The three "see-expressions" in parenthesis in Sec. 2.1 will be removed. We will

make clear that the search is done across the “built climate states” (you are right). “A low-passed filtered collection of years from one or several model simulations are treated as a pool of possible "climate states"

R: 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.

C: We will change the wording of the argument, thanks for pointing this out. We will also evaluate the agreement between NGRIP temperatures in the synthetic test. The correlation between temperatures at NGRIP from the PSR with synthetic data and the synthetic data (CCSM4 output as requested below) is 0.75, showing a connection with SSTs.

R: 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.

C: We will change the section name to “Proxy records”

R: 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.

C: We acknowledge that this test was not made clear in the manuscript and will change that in the revised manuscript.

We change the age model of each proxy record individually by +-500 years, thus giving 2^14 (14 proxy records) different perturbations for the entire set of proxy records. We find a PSR for each option, giving 2^14 different PSRs. The grey shading in Fig. 6 is the 90% confidence interval from all those PSRs. The results are also shown in Fig 7, where the black dots show where the PSR using the original age models do not fall within the same 90% confidence interval. I.e., grid cells without a black dot has a temperature anomaly that falls within the 90% confidence interval produced with the 2^14 PSRs. The dots are also included in Fig 9 for the CCSM4-model pool, but are missing in Fig 8 and we will test the HadCM3 nohose model pool accordingly. We note that because of the very large number of perturbations, it was technically infeasible to keep the full data for all 2^14 PSRs. We thus saved the results in bins of size 0.1°C.

R: 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.

C: We agree that the pool size should be maximized. Unfortunately, not all years are easily available. Line 8, Page 7: The remaining years were not available through the CCSM4 database, and so therefore, availability is the reason for not including these years. Page 6: We are currently checking whether the remaining 300 years of the

HadCM3 data pool are available. If so, we aim to perform a test with all years included to see whether this changes the result and if the spin-up years are chosen. The original reason for not including those years are spinup considerations, as you mention. The simulations are a model drift from an initial (pre-ind) climate state towards MIS3/LGM model climate. Including the spin-up years might be problematic as they may be influenced by unrealistic initial conditions (e.g., ocean at rest, homogeneous temperatures and salinities), in this case a control PI-simulation. While it may be interesting to do and (hopefully) show that we don't pick these states, the information they would yield would simply be too unreliable, and therefore we can a priori discard these as unfit for the method.

R: 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.

C: We will point out that we are talking about freshwater forcing, and not the boundary conditions. However, note that each of the "8 unforced simulations" consist of constant orbital, GHG and solar forcings. Differences only exists from simulation to simulation, not within, and the experiments are not necessarily more forced than a pre-industrial simulation.

R: 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.

C: We will point out the undesirable ratio between years in model pool and years reconstructed and sum up the number of years available within each pool. We try to reconstruct 501 points across 10 000 years using model pools with a size of 2198, 1520 or 2071 years which is clearly not enough to represent the whole interval of 30-40 ka. Although the 501 points are not individual years, the ratio between the points reconstructed and the model pool is more desirable. We did perform a principal component analysis of the proxy record and the HadCM3 model pool, showing that in both cases 9 principal components are needed to explain more than 90% of the temperature variability. This suggest that both the proxy records and the HadCM3 model pool have approximately 9 degrees of freedom.

R: 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?

C: 30 random, fully disconnected samples. This number was chosen to be small enough to perform a rapid test, but larger than the low-pass filter. We performed a test with 501 random samples in addition (number of proxy time steps to reconstruct). The results are comparable, but as we present the results as averages over the samples, the 95% and 5% offsets get "averaged out" and smaller when 501 samples are used. Therefore, 30 samples seem like a more honest test.

R: 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.

C: We will change the target to CCSM4 data and keep the HadCM3 as the model pool. This was already done, but the results only briefly mentioned. The results are attached in Figure1 which shows the same as the Figure 3 in the manuscript, but with CCSM4 as the target (re-gridded to the HadCM3 grid). The resulting offsets between the PSR and the original CCSM4 values are similar, but the structure is different. We note that the correlation decreases outside the core-locations. We will update the manuscript accordingly.

R: 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.

C: We will provide more details. Comments are provided in the last point of this review.

R: 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.

C: We will provide more details. The quantities in Fig 5 is averaged over the full period and then over the 14 core locations

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R: 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.

C: We will remove those tests from Fig. 5 and only show diamonds+ circles (annual + JAS)

R: 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?

C: The intervals are the results from Fig. 6, r and A calculated for the full period for each core location. We will make it more clear. For the last part of the comment, do you mean Fig. 6? Black line should not be within the gray shadow since it is the proxy record, neither should the cyan line, since the gray shadow is the envelope of shifting each proxy +/- 500 years, while the cyan line is the result when nothing is shifted. If the shadow would be done by shifting each record by +/- 500 year by year the cyan would be within the shadow, but this would be too many permutations for us to perform. The shadow gives an envelope that illustrates uncertainty arising from the age models.

R: Fig 1: The figure lacks a colour scale.

C: We will add

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

C: Because the cores have %np values outside the range where the linear relationship between temperature and %np holds because the population of this particular species saturates near 100% or decreases to close to 0% (Page 6, line 8). Extrapolation of the temperature reconstruction would lead to very uncertain results that could greatly harm the analog method. On the other hand, unlike other methods the PSR is very robust to missing data of which we take advantage here.

R: 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.

C: We will change Euclidean distance to RMSE, and provide more information about the figure itself. There are 10 black dots per column, and 501 per row (every 20 years). There are 2198 rows in total. Black lines separate the different simulations.

The focus in the text is on GS9/GI8 where black dots are more homogenously distributed. However, we do agree that this is not true for the remaining time-interval and will discuss this briefly in the text

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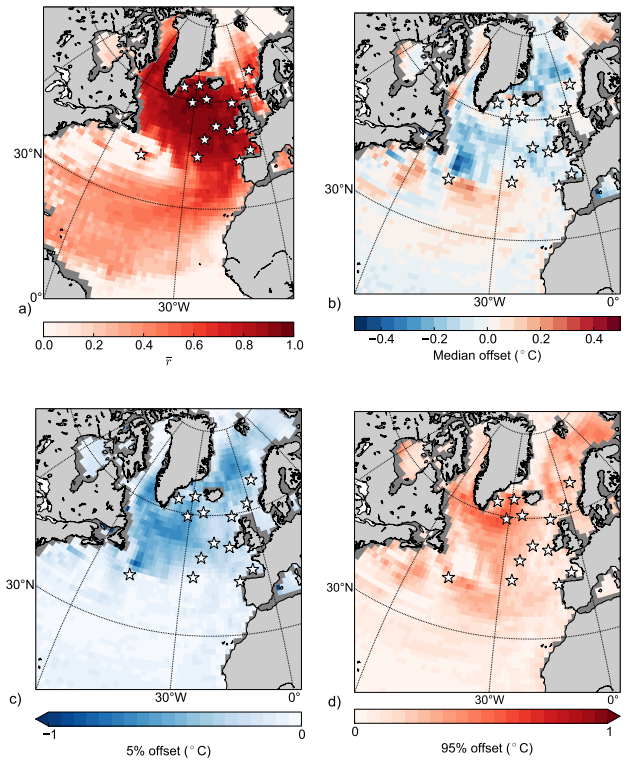

Fig. 1.