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Climate of the Past Discussions

Interactive comment on "Objective identification of climate states from Greenland ice cores for the last glacial period" *by* D. J. Peavoy and C. Franzke

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This is an interesting analysis of Greenland ice core records of rapid climate changes, the Dansgaard-Oeschger events in the last glacial climate period. The study uses a Bayesian approach to evaluate relative probabilities of models against each other as best describing the observe records. The paper contributes to our understanding of the structure of the data, which is necessary and helpful for identifying and understanding the climate dynamics.

I recommend the paper published with the revisions suggested below. These are mainly concerned with the general readability and helping the non-expert reader. Furthermore, I agree with most of the comments by Referee # 1.

First and foremost, I am confused about the 3 climate states: ($S_i = 0$) The "interstadial" (slowly cooling warm state), ($S_i = 1$) the "DO" (rapid shift from stadial to interstadial), and ($S_i = 2$) the "termination" (rapid shift from interstadial to stadial). These corresponds to the three panels in Fig. 4. (This is in contrast to the interpretation by Referee # 1, who is right?) What happens to the fourth state (let's denote it $S_i = 4$), the "stadial", where I would assume $\mu_4 \approx 0$? I would also very much like to see the case with 4 states in Table 1, where I guess the log-likelihood becomes smaller (larger negative value).

Regarding the terminology, I am not sure that everybody agrees on naming the rapid warming events "DO events", I think some consider the interstadials DO events. Regarding terminations, this is used for rapid warming (termination of glacial climate) from glacial to interglacial climates as well. I am not sure, what would be the best terminology, but something more close to the "canonical" terminology: Greenland interstadial (GIS) and Greenland stadial (GS). The two transition states I am not sure about, but perhaps "initiation" (for termination) and "termination" for DO, to be more consistent with geological terminology. I'm not an expert, so please check it out.

Regarding the "absolute value model", it seems silly to me to include that at all, since it is apparently a factor e^{1000} less likely than the "increments model"! The reason is, as I understand, that for the "absolute value model" there are no correlations between neighboring points, which of course makes it virtually impossible to obtain the observed series (that also explains why the exponent $\sim 10^3$ is of the order of the number of points in the data sets). If the authors insist on keeping that model I suggest they provide a governing equation (similar to their un-numbered! first equation) for that as well. It could be something like this, if I understand it right:

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$$X_i = I[\sum_{k=1}^{j-1} \lambda_k < u_i \leq \sum_{k=1}^{j} \lambda_k](\mu_j + \sigma_j \eta_i)$$

where u_i is a uniformly on (0,1] distributed random variable, and $\eta_i \in \mathcal{N}(0,1)$.

Section 2.1 was difficult for me to follow. I think if it should be useful for most readers of CP, it needs a little more explanations (should the "D" in the last equation be an "X"?). I suggest that you expand it and move into an appendix, then you also avoid writing "Readers familiar with Bayesian statistics can skip to Section 3" . Only people with substantial familiarity with Bayesian statistics stand a chance with Section 2.1!

In figure 2 the plots should be long and on top of each other (also noted by referee # 1). The confusion for me was that the the states looks like point states. They are in fact extended over the transition periods? Perhaps a third panel with the last state would make this even more clear. Doing it this way would also give the opportunity to indicate the "canonical" DO events (warmings) as, say vertical lines through all three plots, not confusing red and blue dots.

Regarding the difference between the ice core records, it is quite striking how different the results for GISP2 are from GRIP/NGRIP. The latter have much better dating, but the differences cast a doubt on the robustness of the method. This should be expanded on. Is it something as simple as different temporal resolutions? If that is the case, it should be easy to check by redoing the analysis on the coarsest common resolution for the three records. As a side remark: The mean of 2 permil (there is some sloppy-ness in units, which should be corrected) for the DO-warming is a nice result, however, it should not be compared with the result by Rahmstoft (2003), since this is an analysis of the GISP2 ice core, where you get 0.7 permil or so (units should be permil/x years,

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and in my taste the first equation should be $X_{i+1} = X_i + \mu_{S_i}\Delta t + ...$).

Regarding waiting time analysis: λ_j is the probability that no events happens in 50 years with waiting time T_j. The result that mean waiting time is 1400 years is surprising, it should rather be 3000-4000 years (25 DO events in 100.000 years), I found it to be around 2800 years. Please comment.

Best regards, Peter Ditlevsen

Interactive comment on Clim. Past Discuss., 6, 1209, 2010.