Clim. Past Discuss., 6, C683–C687, 2010
www.clim-past-discuss.net/6/C683/2010/

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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Received and published: 1 September 2010

Comments addressed to reviewer 1

The time period 82 - 11.5 kyr BP was chosen to compare NGRIP, GRIP and GISP2 data. The GISP2 data set, using the Meers/Sowers time scale, was considered only of sufficient resolution to 82 kyr BP.

The authors agree that they should modify their terminology when referring to the states inferred by the model, particularly the transitional regime from interstadial to stadial. This will also be reflected in a change in title and abstract to emphasize the Bayesian modelling approach rather than identification of climate states.

The revised manuscript will include a discussion of the different results obtained be-

tween GRIP and NGRIP. Although these two data sets agree upon the timing of DO cycles there are differences between the distribution of δ^{18}O absolute values and increments between the two series. These graphs will be displayed and commented upon. In particular the δ^{18}O for NGRIP is significantly lower and has broader "tails" in the distribution of its increments.

The reviewer correctly comments that the Bayesian approach is designed to ascertain average properties of the data. Our intention was to identify fluctuations in the time series that share the same statistical properties of DO transition events and analyse their recurrence properties in an averaged sense. This will be made clear in the concluding discussion.

The authors use the phrase "hidden state" in a statistical sense rather than what one observes in the time series. This will be modified to "latent state" and explained.

Indeed it would be interesting to include plots of increments for the real data and ensemble. Plots of the ensemble time series are unlikely to be very similar to the data sets because of the non-stationarity of the data. The statistical model was chosen to be independent of trends in the data and focus upon the increments.

The authors will remove the results related to the "absolute value" (Table 1b) as it performs so poorly against the "increments model" and will mention this in the text. Bayes factor values will be added to compare models in Table 1.

Changes will be made to improve the readability of the manuscript as suggested by the reviewer.

Comments addressed to reviewer 2

The authors agree that they should be more cautious when presenting a mechanism for DO cycles and focus mainly on the statistical methodology used and why it is appropriate for such time series. The results will be interpreted with a more extended discussion of the uncertainties involved when relating δ^{18}O to surface temperature and the correction of 16° C warming will be included.

The particular statistical model used was chosen because it is not affected by any trends/non-stationarity in the data and so one can work with the “raw data” rather than applying detrending or filtering methods. It is designed to detect events occurring within a noisy process.

Comparing models based upon the increments of δ^{18}O to the absolute values, using Bayes' factors, revealed that the increments model was overwhelmingly favoured. More emphasis will be placed upon how this conclusion was reached.

Further to the comments made by Reviewer 1 a discussion will be included of why one obtains a different result when using NGRIP and GRIP. This is likely to be related to the distribution of increments within the process. The influence of the time scale upon the differences between NGRIP and GISP2 will be investigated.

Figure 2 displays the posterior probability of DO events, identified by the statistical method, along with those identified in the original publications. All of those original events are identified with significant probability by our method along with several more. The aim of the paper was to identify DO events without stipulating a priori that they should be of a certain form. This we have managed. The extra events identified have similar statistical properties and may be important when analysing the timing of climate change during the last glacial. This will be further emphasised in the discussion.

Comments addressed to Dr Ditlevsen

The three states should be interpreted as “slowly cooling warm interstadial” ($S_i = 0$), “rapid warming” into interstadial ($S_i = 1$) and “rapid cooling” into stadial ($S_i = 2$). This will be clarified in the manuscript. The fourth state, that one presumes would apply to

the stadial, was not discernible from $S_i = 0$ and indeed this is reflected in the smaller marginal likelihood for a four state model. This will be included in Table 1. This implies that the fluctuations within the stadial and interstadial are similar.

The terminology of the paper will be improved. In particular the $S_i = 1$ will be referred to as the onset of Greenland Interstadial (GIS) and $S_i = 2$ to the termination of GIS.

The results of the absolute value model will be removed from Table 1 and comments made regarding why this model performs poorly compared with and increments model.

The authors agree that Section 2.1 should be expanded and moved to an appendix along with a more detailed discussion of the estimation of the Bayes Factors. The D in Section 2.1 was intended to represent a generic data set when computing a Bayes Factor. To avoid confusion this will change to X.

Figure 2 will be split into separate panels and replotted to show the duration of states identified by the model.

The different results for GISP2 and GRIP/NGRIP could be due to the different resolutions of the data sets and the method used to interpolate the data for the lower resolution GISP2 series. The rewritten manuscript will use a different method to interpolate data. Namely Gaussian Process regression. This is a more suitable method as it allows one to retain the roughness of the data.

The parameter λ_j is equal to the probability of being in state j and is related to the mean waiting time T_j by $\lambda_j = 1 - \exp(-50/T_j)$. As it is, T_j does not account for the duration of the state and so will be shorter than the actual waiting time reported elsewhere. This will be corrected and clarified in the modified manuscript to obtain a value that can be directly compared with previous work.

Comments addressed to M. Crucifix

The authors agree that the title should be modified to emphasise the Bayesian approach with its reliance upon priors. A discussion of the sensitivity of the result to the choice of priors will be included.

The revised manuscript will contain a rewritten and extended description of Section 2 with the details moved to the appendix. The hyperparameters will be explicitly listed and commented upon their choice. Indeed β is the histogram of the S_i, β_j being equal to the number of occurrences of S_j. This will be made explicit in a longer introduction to Bayesian modelling using conjugate priors.

Further details of the estimation of Bayes Factors will be included and the motivation for the particular choice of Laplace-Metropolis estimator. In the manuscript D and θ were intended as generic parameters to explain the concept of marginal likelihood; this will be clarified.

Regarding the interpretation of the data as true indices of climate, ideally it would be better to use a model that includes an observation noise as well as a process "noise". However, this is a difficult problem relating to the development of models that quantify the uncertainty in the ice core itself. The authors' model is intended to be a first approximation of the climate state.

The results of the four state model will be included in the revised manuscript and the parameter μ will be better interpreted in terms of the increase per time.

As with previous reviewers the authors agree that the discussion of the mechanism should be reduced and the main focus of the paper should be on methodology.

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