

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

Anonymous Referee #2

Received and published: 2 July 2010

The authors present a statistical method to determine the different regimes of ice $\delta^{18}O$ changes in the Greenland ice cores. This method is able to identify 3 different regimes that are then used to identify the significant millennial scale climatic events of the last glacial period over 3 different ice cores (GRIP, GISP2, NorthGRIP). I am not able to judge the statistical approach so I comment only on the rest. First, I am embarrassed by the mechanism given for the three climatic states. I don't think that there is a general consensus on the mechanism of a Dansgaard-Oeschger with the three phases as given in the abstract. If there is clearly a gradual cooling, a sudden temperature increase and then a sudden temperature increase, the authors should be more cautious when presenting the mechanism. Similarly, the description of the DO cycles from I. 3 is misleading since the Bond cycles, association between Heinrich events and DO events and 1450 yrs periodicity are only seen over MIS 3. Since the authors do not

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discuss only MIS 3 later on, this should be made clear. Then, it has already largely been discussed that ice d18O should not directly be interpreted in terms of surface temperature changes. Some sentences mentioning such uncertainty due to change in the seasonality of the precipitation and change in the moisture source are necessary in the introduction. By the way, rapid warming can be up to 16°C and not only 10°C. For readers external to the statistical methods used (like me), it is not so clear how exactly this method improves the detection of DO events and especially, I do not understand why “models based on the temperature dynamics, rather than absolute values, are better supported by the data”. Such comparison is not done in the paper and Fig. 2 is so small that it is impossible to really see the comparison between the data and the determination of occurrence of large temperature increase or decrease with this approach. Why the GRIP analysis is so different from the NorthGRIP one (2 states instead of 3) while the timescale is the same. What is the impact of such difference for the periodicity of DO events? Then, GISP2 is very different from NorthGRIP and GRIP as for the periodicity of the events. Is this only a question of timescale? The influence of the timescale could easily be studied by testing the GICC05 timescale for NorthGRIP. An interesting figure would be a comparison of DO events identified by the statistical method and DO events identified in the original publications of the ice d18O records. This could lead to an interesting discussion on the definition of a DO event which is completely missing here since the paper mostly concentrates on the method. In general, this paper is lacking from a general perspective or conclusions that will make this paper useful for anyone working with Greenland ice core d18O records. The conclusions are a bit disappointing with respect to the title.

I leave the editor with the decision of minor or major revision.

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

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