Clim. Past Discuss., 2, S833–S835, 2007
www.clim-past-discuss.net/2/S833/2007/

Climate of the Past Discussions

Interactive comment on "The DO-climate events are noise induced: statistical investigation of the claimed 1470 years cycle" *by* P. D. Ditlevsen et al.

P. Huybers (Referee)

phuybers@fas.harvard.edu

Received and published: 24 January 2007

The opening line of the abstract, that "[t]he significance of the apparent 1470 years cycle in the recurrence of the DO events... is debated", accurately expresses the difficulty in determining whether or not the regularity observed in the Greenland delta-18O record is indicative of underlying periodic behavior. The authors consider two plausible and contrasting hypotheses: that the DO events are spaced at random intervals or that they are associated with an underlying periodic forcing but which is obscured owing to the presence of age-model error. It is further considered whether the DO events are consistent with a stochastic resonance model. The manuscript succeeds in pushing forward our understanding of DO events by defining specific models and quantitatively assessing the consistency of these models with the observations. For this reason, the

manuscript ought to eventually be published. There are, however, several issues which require greater attention — these are listed below in order of importance.

1. It is concluded that the DO events are most consistent with a random waiting-time model. It appears, however, that the other two models — periodic variability subject to age-model error and stochastic resonance — could also be made to fit the observations. In particular, the authors described their age-model uncertainty estimates as "optimistic". Were a less optimistic estimate assumed, the Rayleigh's R distribution (shown in Figure 3c) would be expected to shift to lower values and thus be more consistent with the observations. Some of the stochastic model realizations (shown in Figure 3e) also seem consistent with the observations. Thus it appears difficult from the results of these tests to distinguish between any of these models. That is, the test seems to lack statistical power. Under these circumstances it is hard to understand why it is concluded that the DO events are probably stochastic. Either some greater explanation is required or the manuscript's conclusion and title ought to be revised in accordance with the difficulty of distinguishing between the various models.

2. The focus of discussion on the "weakest" (a=0.1) parametrization of the stochastic resonance model seems odd. Discussion ought to be focused on the stochastic resonance model which yields a distribution of Rayleigh's R and standard deviations which are most consistent with the observations. The description of the model is also confusing. More explanation needs to be provided for the various terms in the stochastic resonance model: What does the term dB refer to? Define "a" as the amplitude. It is unclear how sigma is chosen. Finally, I think that the reference to a "Nyquist frequency" in the context of sampling for the R value will confuse many readers.

3. Some greater discussion seems required for why the GISP2 results are regarded as an anomaly, and that the NGRIP chronology is preferred. It seems rather unlikely that random age-model errors in the GISP2 record would conspire to increase the apparent regularity of the 1470 year pacing.

CPD

2, S833–S835, 2007

Minor comments:

p1280, lines3: "recently been refuted" seems rather strong.

p1280, line20: "extend" should be "extent".

p1284, line26: "to" should be "too".

Interactive comment on Clim. Past Discuss., 2, 1277, 2006.