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Climate of the Past Discussions

Interactive comment on "Dansgaard-Oeschger events: tipping points in the climate system" *by* A. A. Cimatoribus et al.

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The manuscript by Cimatoribus et al. presents an attempt to distinguish between different concepts proposed for Dansgaard-Oeschger (DO) events using advanced statistical analysis of paleoclimate time series. The authors came to the conclusion that paleodata are best consistent with the concept of bi-stability and presence of some deterministic forcing and less consistent with the concepts of purely noise-induced and self-sustained oscillations. This conclusion follows from the presence of the "early warning signal" (EWS) prior to the onset of DO events. In this respect the manuscript by Cimatoribus et al. contradicts to the recent paper by Ditlevsen and Jonsen who came to the conclusion that the EWS is "wishful thinking". I believe, the manuscript by Cimatoribus et al. meets all CP requirements and deserves publication after some

revision.

General comments

1. This comment is related not only to the manuscript under consideration but also to the entire set of EWS papers looking (and usually finding) EWS in different paleoclimate records. All these studies are based on a rather strong (implicit) assumption that the signal and the noise in the paleoclimate records have the same origin and strongly related to some state variable, like the strength of the AMOC in the case of Greenland records. In fact, this is a rather questionable assumption. While many workers do believe that DO events are directly related to the changes in the AMOC, the noise, which the authors used to detect EWS, has very diverse origins: synoptic atmospheric variability, NAO, sea ice variability, etc. How much of the AMOC variability is in this noise is unclear. (In principle, this can be tested with the GCMs which incorporate d18O). This implies that the presence or absence of something which looks like EWS proves nothing. I would recommend to acknowledge this potential caveat.

2. The choice of cited papers raises some questions. On the page 4272 the authors write "Often, bimodality is implicitly assumed for DO events . . ." but no references are provided. As a result, the reader can get an impression that the concept of crossing of the tipping points is the idea invented by the authors. At the same time, when naming "wrong" concepts, the authors are very specific and cite the same papers many times. In fact, in the conceptual sense the only novelty of the manuscript by Cimatoribus et al. is the use of the term "tipping point" instead of traditional "bifurcation point", which is just a question of taste. Otherwise the concept of "crossings" was already very explicitly proposed (see their fig. 3) by Paillard and Labeyrie (1994). This idea was further developed in Ganopolski and Rahmstorf (2001). Of course, as the reviewer I cannot force the authors to cite my own papers. If the authors know more relevant papers, they can cite them instead. However, they cannot simply substitute credits to previous works by word "often". Moreover, it is not really "often". Many workers still believe that multistability is just an artifact of simple (intermediate complexity) models.

3. While I would prefer if the authors would cite Ganopolski and Rahmstorf (2001), they cited several times another my paper, Ganopolski and Rahmstorf (2002) and, unfortunately, always in the wrong context. First, in this paper we did not state that DO events occurred with periodicity of 1500 yr, simply, because this is untrue. The DO events are fundamentally aperiodic and during the last glacial cycle average interspike interval between DO events is about 4000 yrs rather than 1500 yrs. In our paper we wrote that the intervals between DO events tend to be close to multiples of 1500 which is not the same as 1500 yrs periodicity. This fact (and only for MIS 3) was first noted by Alley et al. (2001) based on the analysis of the GRIP ice core record. This conclusion is not supported by more recent ice core records. Second, the authors interpreted our mechanism as “noise induced” and therefore inconsistent with the existence of EWS. This is not true. As it obvious even from the title of our paper, in Ganopolski and Rahmstorf (2002) we tested Alley’s hypothesis about stochastic resonance. The authors should be aware that in the case of stochastic resonance, DO events are induced not by a pure noise, as in the case of coherence resonance, but by a combination of stochastic and deterministic forcings. In this case the transitions between different states tend to occur when the system is more close to the bifurcation points. Therefore the presence of EWS cannot rule out the mechanism of stochastic resonance and does not imply that deterministic forcing really crosses threshold values corresponding to the bifurcation points. The presence of EWS is only inconsistent with the concepts of purely noise-induced or internal oscillations.

4. More serious discussion of why Ditlevsen and Johnsen (2010) came to the opposite conclusion using the same approach is needed. The authors write on the page 4281 that “Ditlevsen and Johnsen (2010) came to opposite conclusions, but they did not consider the average behavior of the ensemble…” I do not understand what the authors want to say here. Similarly to the paper under consideration, Ditlevsen and Johnsen did consider average behavior of the entire ensemble. In addition, Fig. 8 looks strange to me. If one examine the last 1000 yrs before the onset of DO events, the EWS is hardly seen in all three characteristics. It is hard to understand why ESW is

more clearly seen between 2000 and 1000 yrs prior to the transition. In addition, many stadial intervals were shorter than 1000 yrs. Therefore the interval between 2000 and 1000 years is compiled from a much smaller number of DO events than the last 1000 yrs. Please comment on that.

5. In the abstract the authors write “No observational constraint has been forwarded to choose between different theories”. If the authors really believe that they are the first who ever used paleodata to test theories, then they are seriously mistaken. Even in the very special case of the study of ESW for DO events, they are not the first. This has been done already by Dakos et al. (2008) and by Ditlevsen and Johnsen (2010).

Specific comments

Abstract. “ The largest variability in temperature over the last sixty thousand years is connected with DO events”. This is not true. Even for Greenland the largest temperature change occurred during glacial-interglacial transition.

Page 4270, line 20. “with a periodicity of approximately 1500 yrs”. Please correct.

Page 4270, line 25. What do you understand here under “various simple models”?

Page 4271, line 1. “No observational constraint has been forwarded…” See my comment N6 and, please, be more specific.

Page 274, lines 17-20. “ It must be stressed … final choice”. I do not understand this sentence.

Page 4275, line 7. Please explain what are “c” and “sigma” before this sentence.

Interactive comment on Clim. Past Discuss., 8, 4269, 2012.