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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.

Anonymous Referee #2

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In this manuscript, the authors are analysing the fluctuations of a well-known climatic record (the isotopic data from a Greenland ice core) in order to detect possible "Early warning signals" that could happen just before the major climatic transitions known as Dansgaard-Oeschger events. It is furthermore suggested that the characteristics of the fluctuations (variance, autocorrelation, and DFA exponent) can be used to discriminate between the different mechanisms that have been proposed for explaining these events, namely a "tipping point" in a double well, noise induced transitions or self-sustained oscillations. The paper is interesting and raises many interesting questions. Still, I believe some significant revisions are needed before considering publication in Climate of the Past.

Major comments.

1/ I am not convinced at all that the manuscript does "prove" in any way that the "double well" mechanism is more likely than another one. This paper stands in fact quite far from a convincing demonstration. The discussion exposed here is mostly based on simple models that are certainly not generic but only illustrative. The example chosen for the "double well" looks indeed qualitatively better than the other ones, but it is not clear to me that this is linked to the structure of the model or to some choices in the model parameters. In fact, the grouping of DO mechanisms in three distinct categories is a bit artificial. A much more important discussion would be in terms of noise level (fluctuations vs deterministic evolution) and sampling time versus correlation time or dynamical times. For instance, a self-sustained van der Pol oscillator with a sufficiently large time separation (ie. a fast-slow system) is (locally) structuraly identical to the double well with an external forcing : it can be described as a self-sustained oscillation or as a "tipping point in a double well", depending on time scales of interest. Similarly, the "noise induced" mechanism is not distinguishable from a double well with a very high noise level. So it seems to me that the detection (or non-detection) of EWS in the ice core record should be interpreted in physical terms (for instance dynamical time scales versus sampling times scales, or amplitude of "noise" versus "signal", whatever this could mean). Classifying the DO hypothesis into three simplified groups seems to me quite misleading and probably not physically relevant.

2/ The EWS techniques have been proven useful in simple (mostly theoretical) cases. In particular, the phenomenology of slowing-down can easily be understood in the context of the Langevin equation with additive noise (eg. Ditlevsen et al GRL 2010). Still, the real world may be more complex, and it is not clear to me that EWS techniques are applicable in the context of multiplicative noise for instance (see Kuehn 2011). Unfortunately, climate variability is often described in terms of multiplicative noise... This would require some discussion and some caveats from the authors.

3/ The trends shown on Fig.8 are not clear at all during the last 700 years preceding the transition : it appears that correlation, variance and DFA exponent are either constant or even decreasing during the centuries before the transition. Besides, 700 years is quite a long duration compared to the dynamical time constants involved in the thermohaline circulation changes. If the authors want to discuss the relevance of their analysis to the DO question, it is necessary that they provide a full discussion of these time scales (cf. comment #1). It seems to me that the authors are avoiding this discussion in the manuscript (cf. p 4280 line 21) "To explain this decrease, we refer to Kuehn (2011)...". I tried to find some explanation in Kuehn (2011) but I have not find any convincing one (on the contrary, it seems that correlation should always keep increasing for instance...).

4/ The final conclusion obtained here is the exact opposite of Ditlevsen et al (GRL 2010). Again, the discussion is quite poor and the authors argue that the different result may be linked to a different averaging procedure (p 4281 line 20): "(Ditlevsen et al.) did not considered the average behaviour of the ensemble while we think that this may be a step of fundamental importance". If the authors consider it of fundamental importance, then they should better explain how their procedure is different from the Ditlevsen et al one, which is not very clear to me. More importantly, looking again at Ditlevsen et al., it appears that they do not get trends on variance and correlation over the 800 years preceding the transition, thus their conclusion of the absence of any EWS. In fact, in this submitted manuscript, the authors obtain EXACTLY the same result (no trend over the last 700 years, as mentioned above in point #3), so the difference is probably not technical, but only a matter taste and of choosing the relevant time scales... so back again to my comment #1. If some "slowing-down" occur one or two millenia before the transition, is it relevant to the dynamics of DO events ? (implicitly no, according to Ditlevsen et al...). And if so, why should it stop hundred of years before the transition ? These points appear to be the central questions raised by the manuscript... and a thorough discussion is obviously needed here.

More specific points

5/ The phase space reconstruction technique used in 2.1 seems to me quite reminis-

cent of Singular Spectrum Analysis. Could the authors comment on that ?

6/ line 25 p 4273: "The radial distribution has a heavy tail...". This is not obvious at all in Fig.1a since the black line (normal distribution) lies almost within the shaded area...

7/ line 7&12 p 4275: the symbols c, sigma and alpha are used before being defined (line 17). This part of the text should refer to Fig.2: in the current version, the results of Figure 2 are only presented after the model result discussions (line 22 p 4278) which is awkward.

8/ The equation used for the simple models (Ditlevsen et al 2010; Crommelin et al 2004; Abshagen et al 2004..) are not written down, but only the parameter values of these models (values which therefore cannot be explained or justified). This does not help the reader to understand the results, or to get a feeling of the generality of the results. I believe it is necessary to write the equations and to say a few words on the parameter choices that have been made here.

9/ The notion of "external forcing" is mentionned several times (for the "double well" conceptual model and for DO events). When talking about Climate or the Earth system, "external" could mean "extraterrestrial" (or astronomical) for instance... I don't think this is what the authors had in mind, but I cannot see any other well defined notion of "external". Should this be in opposition of some "internal forcings" ? Internal wrt Earth (volcanism) ? Internal/External wrt Ocean or Atmosphere dynamics (CO2) ? Internal/External wrt some arbitrary choice of a submodel (again the slow-fast distinction in the van der Pol model... comment #1) ? I think it is dangerous to use words that have different meanings in different contexts: "external forcing" is meaningless and should be avoided.

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