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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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We would like to thank both reviewers for their constructive criticism. Their comments are insightful and provide useful suggestions for improving the manuscript. We include hereby a point–by–point reply to the two reviewers' comments. A new version of the manuscript will follow.

Reply to Andrey Ganopolski

1. The reviewer is right. We are well aware of the fact that the use of EWS for assessing AMOC variability from $\delta^{18}O$ records is based on strong assumptions

on the origin of the noise, assumed to be the same as the signal (among the many other assumptions made in a study of this kind, e.g. on the relation between the proxy record and the climate it is supposed to be associated with). Indeed this caveat will be stressed more clearly in the manuscript.

2. Clearly, we can not make any claim of originality when suggesting the idea of crossing of bifurcation points or of bimodality in the climate system. Our only claim (from the first part of the paper) is that we think that bimodality from a (scalar) time series may not necessarily imply bimodality in the (multi–dimensional) climate system. We thus suggest a technique (phase space reconstruction) that may overcome this ambiguity, providing the distribution of the states in more dimensions (the number being determined by the condition that we want to completely unfold the dynamics of the system). Still, we want to make no claim to be the first to show bimodality in this time series, the time series is used as a test bed for the technique. Regarding the use of "tipping point" instead of "bifurcation point," the first term was chosen for being more generic than the second: a tipping point may determine an abrupt change in the system even without crossing a bifurcation point. The comment of the reviewer makes clear that our claims are not clear at the moment, and the manuscript will be modified accordingly.

3. The reviewer is right. We overlooked the possibility that, with stochastic resonance, the system exhibits EWS even if the transition is induced by noise. This is a very important point that will be included in the new manuscript.

4. Ditlevsen and Johnsen (2010) do not consider the ensemble behaviour. They only show the various events together, without computing an ensemble average. Given the low signal–to–noise ratio, the weak signal seen in our computations could easily be lost, as well as the clear EWSs preceding some of the events. We think that the parameters used in the computation do not play an important

role as the signal is robust (in the ensemble) to changes in the parameters used in the computation. Regarding the trends in the EWS in fig. 8, the trend in auto correlation and DFA exponent is very clear until 200-300 years before the onset of the transition, indeed not as clear for variance. The different number of time series used at various times is reflected in the large increase of error margins before -1000 years from the DO onset. Our results hold within these limits. EWS are seen in the ensemble, and the signal seems to be statistically significant even if not strong. The weakness of the signal may indeed point to the relevance of stochastic resonance mechanism. We will more clearly explain why, in our opinion, we come to opposite conclusion to Ditlevsen and Johnsen (2010).

5. The statement is indeed misleading, and will be changed. Our claim is rather that, as Kuehn (2011) shows in an idealised context, considering ensemble properties may uncover signals otherwise hidden by noise.

Reply to Reviewer #2

1. As stated also in the item 1 of the reply to the other reviewer, we agree that the limitations of our work, and in general of investigating EWS in paleoclimatic records, should be stated more clearly in the manuscript. Replying more specifically to reviewer #2, countless models could be developed showing similar EWS; what we tried to do has been to review the models that have been suggested for explaining DO events and classify them based on EWS. We then looked at the proxy data, trying to identify EWS that can suggest that one model is more appropriate than others. We agree that it can be difficult to distinguish just from time series analysis between various prototype models, as they indeed may share many characteristics. However, the EWS we find are clearly incompatible with many prototype models. This is our main point. In general, this operation, as

every inverse problem, is very uncertain. We are not aware of other models for DOs (e.g. using a van der Pol oscillator) but we are open to suggestions; our work is only an attempt to distinguish between models that have been suggested for DO events. This approach was described in section 2.3 on page 6.

2. We agree with the reviewer that the general limitations of EWS should be mentioned more explicitly in text, and will follow the suggestion of the reviewer.

3. As stated in the item 4 of the reply to reviewer #1, we agree that the trend in variance stops approximately 700 years before the DO onset. The trend is instead clearly present for the two other quantities until close to the onset. A decrease in variance, following an increase, is shown for instance in C. Kuehn (2011, A mathematical framework for critical transitions) in his figs. 9, 10 and 11. Unfortunately, it is hard, if not impossible, to estimate the time scale at which this decrease should take place, as we have no information about the rate at which the control parameters of the system are changing in the proxy data. This is a limit of this work, which should be discussed more clearly in the manuscript. Furthermore, recent work by Dakos et al. (2012, Robustness of variance and autocorrelation as indicators of critical slowing down.) suggests that autocorrelation is a more robust indicator than variance.

4. As discussed in item 4 of the other reply, the fundamental difference between our work and the work of Ditlevsen and Johnsen (2010) is that they do not consider the ensemble properties (meaning ensemble average) and, given the weakness of the signal, we think that it may be easily lost by only showing all the realisations together. Again, the difference between our results and theirs is more evident considering correlation and DFA exponent rather than variance. We do not agree that we should focus only on variance, when other (independent) quantities show a clear upwards trend that stops much later than for the variance. Indeed, the uncertainty connected with the time scale at which the upwards trend stops should

be mentioned more explicitly in the revised manuscript, but a trend is still present. It would be surprising if the trend was totally disconnected from the dynamics of the DO events, as it shows up in the ensemble, where the time series are synchronised on the DO onset. Again, the findings of Dakos et al. (2012) on the robustness of variance compared to autocorrelation should be considered here. We will improve the discussion including these points.

5. Yes, SSA is based, among other mathematical tools, on Takens' theorem. It is usually aimed at enhancing signal to noise ratio, rather than phase space reconstruction.

6. The reviewer is right, the difference between the normal distribution and the observed distribution is at the margin of the shaded area. This does not change the results.

7/8. We will change the text according to the suggestions.

9. External forcing should be understood here only in the purely mathematical sense of implying a non–autonomous system. This point will be clarified in the manuscript.

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