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Climate of the Past Discussions

Interactive comment on "Limitations of red noise in analysing Dansgaard-Oeschger events" *by* H. Braun et al.

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1 Introduction

Since their first description, Dansgaard-Oeschger (DO) events have fascinated palaeoclimatologists in a very special way. Almost regularly occurring, rapid warming and cooling transitions appear to be of importance not only in the past, but also for the future long-term climate evolution. Hence, strong interest has arised in a physical understanding of the processes that lead to the occurrence of such events. In parallel, statistics has provided first insights into the question whether or not DO events may be originated in certain classes of simple stochastic processes. The state-of-the-art in corresponding research is that first-order auto-regressive processes can be ruled out

as candidates for explaining the dynamics of DO events. Complementary to these results, it is however questionable whether such simple processes may actually be used as *candidates* against which the properties of DO events should be tested. The paper by Braun *et al.* addresses this important conceptual question, which is fundamental to any further *statistical* interpretation of DO events. Hence, the topic of this article is relevant and timely and deserves publication in Climate of the Past. I have however some comments the authors should to take into account for improving their presentation in a final version. Consequently, I recommend publication of the presented work after the points of concern discussed below have been sufficiently addressed.

2 Process-based vs. statistical understanding of DO events

The fundamental conflict that is addressed in this paper is the discrepancy between process-based (physical) and statistical understanding of the properties of DO events. The title of this paper suggests that the authors follow the *statistical* approach, however, they use a *mechanistic* model for deriving their conclusions, i.e., for ruling out the applicability of a particular stochastic process as a null hypothesis. Although this is particularly valid in the considered case, it would be much easier for the reader to follow the corresponding lines of argumentation if this point would have been made more explicitly by the authors in the beginning of the manuscript.

Section 1 of the paper contains a thorough introduction to the problems addressed in this contribution. Most parts are rigorously written and scientifically sound. There are however some points where the presentation of the authors' arguments would strongly benefit from even somewhat more precision. In particular, the authors identify a first-order auto-regressive process (whose power spectral density is explicitly given in Eq. (1)) with a red noise random process, which however displays somewhat different statistical features (i.e., a *rigorous* power-law tail of the power spectral density).

Although this mutual identification of two intrinsically (slightly) different processes is common practice in climatological literature, one might expect a fully correct treatment of this issue in a paper that essentially deals with the resulting spectral properties of such processes.

In order to derive their conclusions, the authors do not use observational data (since these contain too few DO events to allow for statistically robust results and have problematic features such as unequal spacing, dating uncertainties, high noise levels, etc.) but the output of a rather simple mechanistic "toy" model for the dynamics of DO events. In the case of random forcing of this model, the authors refer to threshold crossing events in their particular model as random DO events. In my opinion, the conceptual problems of this approach I list below deserve further discussion in the manuscript. Nonetheless, I agree that the way the authors address their basic problem is so far the only practical way, and that an even more sophisticated discussion, incorporating possible further methodological improvements, is clearly beyond the scope of the presented work. However, I think that the conceptual limitations of the presented analysis should be expressed much more clearly in the manuscript. In particular, I would like to mention the following points:

1. The authors state that their simplified model shows the same waiting time distributions as a more complex ocean-atmosphere model CLIMBER-2 (which is itself "only" a climate model of intermediate complexity), but do not provide evidence that also the corresponding higher-order statistical features (such as the power spectral density) of the "toy model" coincide with those of DO events simulated in the full model (not to speak about the comparison to real-world data). I have also not been able to obtain corresponding information from Braun (2007). Without this piece of information, I may however not be fully convinced that some properties of the simple model that lead to the conclusion that the occurrence of synthetic random DO events in this specific model is different from those of a red noise process could change in the high-dimensional system, which I expect

to be closer to reality. To give a well-known example from statistics: the additive superposition of simple AR[1] processes may yield stochastic processes with completely different statistical properties. It would be very supportive for their conclusions if the authors could provide arguments that also the higher-order statistical features of DO events in their simplified model do not significantly differ from those in a fully complex climate model.

2. Another issue that however goes clearly beyond the scope of this paper is the question of how, one the one hand, noise in the observational records influences the statistical properties of the "true" DO events, and of how, on the other hand, related effects due to parametrisation modify the properties of interest in both complex and simplified models. I don't think that these issues should (or even can) be extensively discussed within the framework of the presented manuscript. However, this point seems to be crucial for the interpretation of the obtained results and should thus at least be briefly mentioned in the paper.

3. A specific point of criticism is that the authors operate within a framework of statistical testing without properly addressing the corresponding null hypothesis. The actual take-home message I get is that one should *not* test the significance of DO events against the presence of a red noise process (although an AR[1] process is meant here again). An explicit alternative is however not clearly proposed. Do the authors recommend testing against the presence of noise-induced threshold crossing events in a specific low-dimensional dynamical system whose parameters need to be adjusted properly? The conclusion "the spectral properties of highly non-linear processes such as DO events can be fundamentally different from a red noise (or AR[1]) random process" [p. 1810, ll. 4-5] is actually no surprise, but directly follows from a corresponding statistical test (the corresponding results can already be found in the literature as cited by the authors). The argumentation that this fact implies an overestimation of significance of a statistical test (against red noise) however remains unclear to me, at least unless the au-

thors state their idea of a corresponding more sophisticated null hypothesis more clearly.

3 Technical comments

Apart from my general points of concern expressed above, I have several minor comments the authors should take into account in their revision:

1. In the abstract, the authors write "A red noise random process was used to evaluate the statistical significance of this peak...". This formulation suggests that this was part of the presented work. However, a corresponding test of observational records was described in the literature, as mentioned later [p. 1804, l. 22]. I recommend using another formulation to make this clearer.
2. When discussing the possible physical origin of a threshold crossing in DO events, one might mention the notion of "tipping points" in the climate system (Lenton *et al.*, 2008).
3. In nonlinear sciences, the framework of noise-induced periodicity in systems with thresholds is known as coherence resonance, in opposite to stochastic resonance where an existing weak periodic signal is amplified due to the presence of noise. It is somewhat surprising that the authors completely avoid using this term in their manuscript. In relationship to the problem discussed here, the reader of the presented paper might be interested to know that there are other cases where coherence resonance has been proposed for explaining different phenomena in the climate system (for example, glacial cycles (Pelletier, 2003), although in this specific case, stochastic resonance appears a more natural explanation due to the presence of periodic external (Milankovich) forcing).

4. On p. 1805, l. 24, "Pikovski" should read "Pikovsky" to be consistent with the bibliography.
5. From Section 2, it does not become clear whether the term "DO event" refers to the abrupt cooling or warming event (i.e., $f < T^-$ or $f > T^+$).
6. Since the authors use a low-dimensional model with six parameters, it would be interesting to get more information about how they tune their parameters towards the real-world scenario (where in particular the noise amplitude is unknown). Is there a problem with overfitting due to this considerable number of unknown variables?
7. Are the periods of the bi-sinusoidal forcing in the presented example of a deterministic model run inspired by solar cycles? If yes, this could be indicated in the manuscript.
8. Typo on p. 1808, l. 10: "sprectral" should read "spectral".
9. The authors use a rather specific kind of stochastic forcing for driving their low-dimensional model. How crucial is the particular choice of this forcing (distribution, cut-off frequency, power spectral exponent of the red noise part) for the obtained results? Can these results be generalised to more general types of noise?
10. Fig. 4: Why do the authors use 1000 different realisations of 50,000 years each for obtaining the PSD instead of taking disjoint windows from the full 100 Myr run?
11. Fig. 5: It would be interesting to see also how the variance of the waiting time distribution (not only its mean) changes with increasing noise level. The same holds for the properties of the spectral peak. (How do the significance with respect to the AR[1] background, peak-to-noise-ratio, or peak half-width change with σ?)

This would make it much easier for the reader to conclude within which range of noise amplitudes coherence resonance is actually relevant in the model driven with the prescribed parameters.

4 Conclusions

This paper addresses some important questions concerning the possibility of assessing significant differences between observed (or modelled) DO events and simple stochastic processes. This is a scientific problem of paramount relevance in present-day research on palaeoclimatology. The presented manuscript will however benefit from clarification of the points I have addressed above. After a corresponding revision, I recommend its publication in Climate of the Past.

References

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