

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

**Anonymous Referee #2**

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First I have to say that, although I am a statistician/mathematician working on climate applications for years, I am neither a climatologist per se, nor a specialist in paleoclimate. Hence, I did completely not review the climate and DO modelling part, but rather the statistical concepts and details. Of course, if the associate Editor of CP considers that this review is not valuable, it can be ignored.

### **SUMMARY AND GENERAL COMMENTS**

Although I think some clarifications are needed for a clear understanding of the conclusions reached, I have found this article scientifically interesting, well written, and certainly addressing relevant questions clearly within the scope of CP.

My general feeling is that it is an interesting (potentially quite controversial) article that

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would need some clarifications.

As described in the introduction Section, the authors basically state that: 1. Non-linear systems, e.g., with threshold (such as DO events) imply responses at a preferred time scale. 2. A red noise random process yields response on all time scale. Then, red noise might be not adapted to model DO events

I have the feeling that this article is an addition to the Braun et al. (2007) paper in the sense that only Section 3 presents some new technical/statistical properties of the two-state model described in Braun et al. (2007).

My two main comments concern needs of clarification for: - what the authors mean by "a significance of more than 99%" - the initial assumptions leading to the conclusion that red noise is not applicable to estimate the significance of the spectral peak of DO events. (See specific comments for both)

### **SPECIFIC COMMENTS**

1- The authors should clarify what they mean by "a significance of more than 99%" in the abstract and in the Introduction Section. It should be stated if it is a good or a bad point (I guess it depends on the "null hypothesis" tested. So, what is the "null hypothesis"?). Does it mean that it is not significant at 95% (i.e., at confidence level 0.05) and ONLY significant at 99% (0.01 level)? Indeed, this kind of significance test can easily be misinterpreted by the reader. Technical details are not necessarily needed but some (partly methodological) explanations of what is tested or estimated here would be helpful.

2- A very naive question/remark: On page 1807, line 18-19, the authors state that the "simple two-state model has six parameters". One could ask how many parameters have the alternative approach(es)? And what are the alternative approach(es) (if any)?

3- In Section 3, one interesting point here is that DO-like events can be realistically simulated without red noise components. However, I am not sure to see why the com-

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parison with a theoretical red noise process is relevant since the initial simulations and forcing are not supposed to follow one. If the authors think that this comparison is fair, they should more detail and explain why. Indeed, this implies that the authors assume that the forcing is a random Gaussian distributed variable with white noise power signature. In other words, it seems to me that one can conclude that the red noise is not applicable to estimate the significance of the spectral peak of DO events but ONLY based on the Gaussian-distributed noise assumption in the constructed experiment.

In conclusion, I recommend publication of this article in *Climate of the Past* after some clarifications and revisions.

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