

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

H. Braun et al.

Holger.Braun@iup.uni-heidelberg.de

Received and published: 20 October 2009

Response to the referee comment by Reik Donner on the manuscript "Limitations of red noise in analysing Dansgaard-Oeschger events".

H. Braun, P. Ditlevsen, J. Kurths, and M. Mudelsee

Holger.Braun@iup.uni-heidelberg.de

In section 2 of his report, the referee raises the aspect that in our manuscript, we use a mechanistic model for deriving our conclusions, but we do not follow a rigorous statistical approach. We agree with the referee that the manuscript would gain from a more explicit discussion of this aspect, and we will include such a discussion in the revised manuscript. But concerning the interpretation of the referee that we do not

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



follow a rigorous statistical approach, we would like to stress the following aspect: So far, methods of linear time series analysis have almost exclusively been used to study Dansgaard-Oeschger events. We are firmly convinced that, due to their apparent non-linearity, these events cannot adequately be analysed by means of linear methods. In this light, we regard our study as the starting point of a series of studies which in fact follow an even more rigorous (i.e. non-linear) statistical approach.

We further agree with the referee that a more rigorous distinction should be made between a first-order autoregressive (AR1) random process on the one hand and a red noise random process on the other hand. According to the standard interpretation in climatology/meteorology, however, a red noise random process represents a random process whose power spectral density decreases with increasing frequency (Gilman et al., “On the power spectrum of red noise”, J. Atmos. Sci., 20[2], 182-184, 1963). Since the power spectral density distribution as simulated with our model of Dansgaard-Oeschger events clearly shows a pronounced maximum in the intermediate (i.e., non-zero) frequency range (Figure 4 in the manuscript), we think that our results rule out not only the specific AR1-case, but in fact the much more general case of a monotonically decreasing power spectral density distribution. We thus think that the treatment of this aspect is correct in our manuscript, but we understand that a rigorous definition of the expression “red noise random process” may be helpful in the revised manuscript.

We also agree with the referee that the conceptual limitations of the presented analysis deserve a more detailed discussion. In particular the referee raises three main points:

2. 1. The similarity between the simplified model of Dansgaard-Oeschger events and the ocean-atmosphere model CLIMBER-2.

The referee raises the question if the power spectral density distribution of Dansgaard-Oeschger events in the “toy model” coincides with the one as simulated by the ocean-atmosphere model. We now tested this aspect and – according to our interpretation –

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

we think that there is indeed a convincing agreement between both models (Figure R1 in this reply). To clarify this issue we will include figure R1 in the revised manuscript.

2. 2. The influence of noise on the statistical properties of the “true” DO events and the influence of the model parameters of the statistical properties of the simulated DO events.

We agree with the referee that a detailed discussion of these aspects is not possible in the framework of the manuscript. We will, however, mention these aspects in the revised manuscript version, following the referee’s recommendation.

2. 3. The problem of a more sophisticated null-hypothesis.

We agree with the referee that the main message of the manuscript is that an AR1 process (or, more generally, any random process with a monotonically decreasing power spectral density distribution) should not be used to assess the statistical significance of Dansgaard-Oeschger events. We think that this is a relevant and novel conclusion, in particular because many colleagues referred (and still refer) to the 1470-year spectral peak in the GISP2 ice core data as being significant at the 99%-level. This interpretation, however, is based on the assumption of an AR1 random process (Schulz, 2002). In other words, it is not the scope of our study to actually test the significance of this spectral peak. Instead, we are illustrating that a simple non-linear model of Dansgaard-Oeschger events (which itself has similarities with an ocean-atmosphere model of intermediate complexity) has a non-trivial power spectrum with a spectral hump, even without the presence of any periodic forcing component. Our study thus shows that claiming statistical significance using the AR1 random process as null-hypothesis (as was done for the GISP2 ice core) is not appropriate.

Since the statistical analysis of highly non-linear processes such as Dansgaard-Oeschger events is often not trivial, we think that it is beyond the scope of the present manuscript to actually suggest a particular null hypothesis that is suitable to test the statistical significance of the millennial spectral peak of Dansgaard-Oeschger events.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

In fact, it is not even clear that the power spectral density distribution is a suitable test statistic to distinguish between the hypotheses that have been proposed to explain the recurrence pattern of these events. Clearly, this aspect should be addressed before the problem is tackled which random process might serve as a more adequate null hypothesis! We thus advocate that future work should focus on the development of more realistic (i.e., complex) noise processes as null hypothesis and on the construction of more efficient measures of non-periodic (i.e., complex) regularity as test statistic. Based on these methodological improvements, future analyses might be able to better address the question what triggered the remarkable Dansgaard-Oeschger events in glacial climate.

Concerning section 3 in the referee report (technical comments):

3. 1. Thanks for pointing that out. We will change that formulation.

3. 2. We will include a short discussion concerning the concept of “tipping points”.

3. 3. We will mention the mechanism of “coherence resonance” in the revised manuscript. Besides, we would like to point out that following the argumentation of the referee concerning the explanation of “glacial cycles”, it is also possible to argue that a coherence resonance mechanism does not appear to be the most natural explanation for the recurrence pattern of Dansgaard-Oeschger events: The solar de Vries (~ 208 year) cycle was reported to persist during the last ice age (Wagner et al., “Presence of the solar de Vries cycle [~ 205 years] during the last ice age. *Geophys. Res. Lett.*, 28(2), 303-306, 2001), so also in the case of Dansgaard-Oeschger events there are at least indications for the existence of a quasi-periodic external forcing.

3. 4. Thanks. We will change that expression.

3. 5. The onset of a Dansgaard-Oeschger event is represented by the transition from stadial (“cold”) conditions to interstadial (“warm”) ones, at time t_0 in figure 2 in the manuscript. The opposite transition (at time t_1) represents the termination of an event.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Thus, the expression "Dansgaard-Oeschger event" is used for the time interval between these two transitions, i.e. for the entire duration of the "warm" state, following standard paleoclimatic nomenclature. We will mention this in the revised manuscript.

3. 6. We used the following approach to construct and tune the simple two-state model. This approach is described in detail in the publication of Braun et al., "A simple conceptual model of abrupt glacial climate events", *Nonlin. Proces. Geophys.*, 14, 709-721, 2007:

Our basic assumption is the existence of (i) two states of operation, (ii) threshold-crossing dynamics during the transitions between both states, and (iii) a millennial-scale relaxation process, represented by equations (1) and (2) in the publication of Braun et al., 2007. This assumption leads to six tunable model parameters, namely the threshold parameters A_0 , B_0 , A_1 , B_1 and the relaxation times τ_0 , τ_1 (figure 2 and table 1 in the present manuscript). These parameters were estimated by systematic model experiments with the ocean-atmosphere model CLIMBER-2.

For example, B_0 and B_1 represent the equilibrium values of the threshold function in the "cold" and "warm" state, respectively. This implies that the values of these parameters can be deduced by a model experiment with CLIMBER-2 in which the amplitude A of a small (i.e., sub-threshold) periodic forcing signal (i.e. a freshwater input) is gradually being increased, until at some critical value $A = C_1$ the forcing becomes supra-threshold (in the sense that the "cold" state gets unstable and only the "warm" one remains stable) and the system switches from the "cold" state to the "warm" one during a minimum of the forcing cycle (thus, $B_0 = -C_1 =$ about -9.7 mSv). When the amplitude of the forcing is further being increased, at some critical amplitude value $A = C_2$ the "warm" state also becomes unstable and the system switches back to the "cold" state during a maximum of the forcing cycle (thus, $B_1 = C_2 =$ about 11.2 mSv). As a consequence, it is possible to determine the parameter values B_0 and B_1 of the "toy model" with a high precision, solely from the existence of some critical forcing values in the model experiments with the ocean-atmosphere model CLIMBER-2.

Likewise, the values of the parameters τ_0 and τ_1 can be estimated from the output of the model CLIMBER-2, whose simulated North Atlantic (oceanic) temperature, salinity and density fields undergo a pronounced millennial-scale relaxation process following the "cold_to_warm" and the "warm_to_cold" transitions (thus, τ_0 is approximately equal to τ_1 = about 1000 years). Note that the existence of this relaxation process is also manifested in figure 3 in the present manuscript, which shows that during the transitions between both model states, the temperature curves as simulated with CLIMBER-2 show clear indications for an overshooting, followed by a millennial-scale relaxation process.

The model parameters A_0 and A_1 , in contrast, are much more difficult to determine from the output of the model CLIMBER-2 and are tunable over a wide range of parameter values. To reduce the number of free parameters, we decided to choose $A_0 = -A_1$ and to optimise this single remaining parameter by maximising the agreement between the model CLIMBER-2 and the "toy model". To avoid the problem of overfitting, we tested the agreement between both models in a number of simple model experiments (see e.g. figures 5 and 6 of the supplementary material accompanying the publication of Braun et al. [2007]). We thus think that we can claim that the agreement between the model CLIMBER-2 and our "toy model" is not by coincidence, but results from the ability of the "toy model" to mimic the main dynamical principles of Dansgaard-Oeschger events as simulated with the model CLIMBER-2. Of course our procedure implies that our choice of the six model parameters A_0 , A_1 , B_0 , B_1 , τ_0 and τ_1 depends strongly on the results obtained with CLIMBER-2. However, we do not see an alternative way to determine these parameters without avoiding the problem of overfitting.

As a final comment, it should be stressed that the output of the "toy model" is invariant to a common scaling of both the forcing f and the model parameters A_0 , A_1 , B_0 and B_1 . Thus, even if the noise amplitude in the "real world" were considerably different from our model simulations, the output of our "toy model" would still be the same, provided

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

that the model parameters were scaled accordingly.

3. 7. Yes, this is correct. We will indicate that in the revised manuscript.

3. 8. Thanks for pointing that out.

3. 9. We will address this issue in the revised manuscript.

3. 10. The main reason is that our approach enables us to compare our results with the results as obtained with the ocean atmosphere model CLIMBER-2 (compare figure R1 below). With that model, already the simulation of a single 10 Myr time series would take approximately 200 days of computational time on the accessible supercomputer, whereas it is possible to run dozens of 50,000 year runs (~1 day computational time each) at the same time.

3. 11. We will investigate these aspects and will include our results in the revised version of the manuscript.

Interactive comment on Clim. Past Discuss., 5, 1803, 2009.

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper



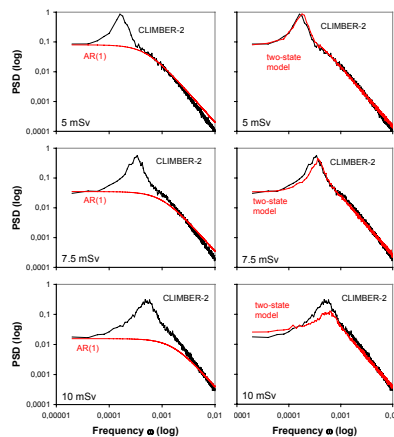


Figure R1. Comparison of the power spectral density (PSD) distributions as obtained from the three noise-driven processes. The black curve shows the distribution as obtained with the ocean-atmosphere model CLIMBER-2 over a Monte-Carlo ensemble of 100 members, each representing a 50,000-year long time series of random (i.e., noise-driven) DO events. The standard deviation of the freshwater noise in the model input is 5 mSv (top), 7.5 mSv (middle) and 10 mSv (bottom). 1 mSv = 1 milli-Sverdrup = 10^3 m³/s. The red curve shows the fitted distributions as obtained from a first-order autoregressive (AR1) random process (left) and from the “toy model” (two-state model). Note that in this comparison, we forced the ocean-atmosphere model CLIMBER-2 and the “toy model” with precisely the same random input. In the model CLIMBER-2, this input is added as a surface freshwater anomaly in the latitudinal belt between 50 and 70 °N in the North Atlantic. In the “toy model”, this input is implemented as the forcing function f . To optimise the agreement between both models, we only allow for a linear scaling of the output of the “toy model”, in order to account for the fact that this output is in freshwater flux units (mSv), whereas the output of the ocean-atmosphere model is in temperature units (K). We thus have only one tunable fitting parameter, that is, a simple factor of proportionality. Note that an AR1 process cannot reproduce the existence of the pronounced millennial spectral hump in the distribution as simulated by the ocean-atmosphere model. This feature, in contrast, is reproduced by the simple two-state “toy model”, as well as the width of this “resonance hump”, the position of its maximum as a function of the noise intensity, and the approximate $1/\omega^2$ -proportionality for high frequencies.

Fig. 1.