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Climate of the Past
Discussions

Interactive comment on "Random and externally controlled occurrence of Dansgaard-Oeschger events" *by* Johannes Lohmann and Peter D. Ditlevsen

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We thank Takahito Mitsui for carefully reading and evaluating of our manuscript. His comments have been very helpful and have improved the quality of the revised manuscript. In the following, we list all referee comments and our corresponding responses.

1. "I confused about the terms, "stadial rate" and "interstadial rate". I wondered if the stadail rate is the transition rate from stadial to interstadial or vice versa. I'm temped to call them "warming (cooling) rate" or " warming (cooling) event rate".

We agree that this terminology is confusing and will adopt a version of the reviewers proposition. Our original terminology was meant the following way: Stadial rate is the rate of transitioning from stadial to interstadial, thus it corresponds to the warming rate. In the same way, the interstadial rate can be referred to as the cooling rate. We would like to propose the following change: The transition rate from stadial to interstadial (and vice versa) is referred to as warming (cooling) transition rate.

2. "Eq. (3) sounds counterintuitive because the insolation reduces the warming rate $\lambda 1$ and the ice volume increases the warming rate. Similarly, the insolation increases the cooling rate $\lambda 2$ and the ice volume decreases the cooling rate. Is there any possible explanation for this?"

Thanks for this observation regarding Eq. (3). It is in fact a mistake, since we mixed up the right-hand sides for $\lambda 1$ and $\lambda 2$. When changing this, the interpretation is not counter-intuitive anymore: $\lambda 1$ is the warming transition rate (increase by insolation and decrease by ice volume) and $\lambda 2$ is the cooling transition rate (decrease by insolation and increase by ice volume). It has now been corrected.

3. "I suggest to explicitly show the relation between the stadial rate and S(t), and that between the interstadial rate and I(t) for the reduced two-process model, like Eq. (3). Otherwise, it's not entirely clear whether the insolation (the global ice volume) indeed promotes or inhibits the warming (cooling) events."

We agree that this will improve clarity and will add an equation in the manuscript.

4. "The integrated insolation above 350 W/m 2 (Huybers, 2006) is chosen as a forcing. Why don't you choose the summer solstice daily-mean insolation, which is also common? Is it a consequence of some optimization? If so, it is worthy to be mentioned.

We thank the referee for this important comment and agree that we need to address this in the manuscript. We did not use the integrated insolation forcing as a result of an optimization, but rather used it as a first choice since we believed it is the relevant quantity for the phenomenon at hand, capturing the notion of positive degree days in high latitudes. However, we also conducted the fitting routine with daily-mean summer solstice insolation at 65 deg North, and obtained results that are equivalent to the case with integrated insolation. Specifically, we again find insolation control of stadial durations and ice volume control of interstadial durations. The fitting error with full forcing is then RMSD_sum = 0.62, and thus slightly worse than when using integrated insolation. We also find a reduced model with a very good fit of RMSD_sum = 0.69. The good agreement is not surprising because the two forcings look very much alike.

We thus added the following paragraph at the end of the discussion section:

“The results do not depend critically on the specific insolation forcing we used. To illustrate this, we also tried the daily-mean summer solstice insolation at 65 deg North and obtained results that are very much in line with what has been presented here. Specifically, we again find insolation control of stadial durations and ice volume control of interstadial durations. The fitting error with full forcing is RMSD_sum = 0.62, and thus only slightly worse than when using the integrated insolation presented in this paper. We also find a reduced model with a very good fit of RMSD_sum = 0.69. With this work we do not attempt to study which kind of insolation forcing might lead to the best fit, since the results would not be statistically significant given the small sample size of DO events and the fact that we already obtain a very close fit with both integrated and summer soltice insolation.”

5. “The authors mention "While the distribution of waiting times in between warming events is well modeled by an exponential distribution (not shown here)," (P9. Line 14-15). This is the fact from the observation since Ditlevsen’s early works. The exponential distribution is true for the stationary one-process model but not true for the stationary two-process model as shown by by Eq. (1). The latter inconsistency is OK because the authors rejects the model in the end. However, is the exponential distribution consistent with the non-stationary two process model? If so, why?”

We thank the referee for this question and would like to offer the following explanation. Instead of discussing whether (samples of) the distributions of the different models are consistent with one another (e.g. exponential distribution and non-stationary two-process model), we think it is more instructive to focus directly on whether the empirical data distribution is consistent with the different model distributions. The empirical distribution of inter-warming times in the data lies in fact very close to an exponential distribution of the same mean and is thus clearly consistent with it (using one-sided KS test: p=0.96). Interestingly, the data is also consistent with the distribution of the two-process model using parameters estimated from data, albeit with lower significance (p=0.30). When using the non-stationary two-process model, we find again strong correspondence of data and model distribution (p=0.92). The reason for this is following: The data distribution has a large dispersion (coefficient of variation CV = 1.129) , which is close to the one of the exponential distribution (CV=1.0). The stationary two-process model is, however, less dispersed (CV=0.708), as discussed in the manuscript. If we vary the parameters of the two-process model in time, the mean is varying and thus we expect the stationary distribution to be “smeared out” (i.e. more dispersed), which we indeed find for our best-fit non-stationary two-process model (CV=1.156). Thus it is close to the data distribution and presumably consistent with an exponential distribution. We would like to omit a discussion of this in the manuscript since the results of the paper are consistent with these considerations and the aim of this work was to go beyond the stationary statistics of waiting times in between warming events.

6. “How is the observation of the exponential distribution consistent with the following statement?: "In the limiting case of a DO cycle comprised of a very large number of independent processes, one finds a Gaussian distribution of waiting times" (P10. Line 9-10).”

Thanks for this interesting comment. We would like to point out that these two things are not supposed to be consistent, because we do not claim that a DO cycle is comprised of such a large sequence of independent processes. If we consider two independent processes, there is already a departure from exponential statistics. Here, we merely mention that it would tend to Gaussian statistics if there were more independent processes. The variance of this Gaussian would also decrease as the number of processes is increased. For clarity, we propose to address this by expanding the corresponding paragraph of the revised manuscript in the following way:

“This model gives rise to a more regular sequence of warming events, compared to the one-process model. This is because one DO cycle is the sum of two independent processes and thus its duration does not follow an exponential distribution (coefficient of variation CV = 1.0), but Eq. (1), which is less dispersed (CV = 0.708). In the limiting case of a DO cycle comprised of a very large sequence of N independent and stationary processes, one finds a Gaussian distribution of waiting times with decreasing variance as N grows. This would then correspond to an almost evenly-spaced sequence of events, which is not supported by the observations.”

7. “In Eq. (1), both exponents are $-\lambda$ 1 T . Is this right?”

Thanks. The first exponent is $-\lambda$2 T, which has been corrected in the manuscript.

8. “P2. Line 12: Is "single events" fine?”

Changed it to “individual events”.

9. “P3. Line 4 and in Fig. 6: "ky" -> "kyr" (if you want to correct)”

ok

10. “P3. Line 24-25: Svensson et al. (2006) -> (Svensson et al., 2006)”

ok

11. “P4. Line 1: "withing" -> "within” “

ok

12. “ P8. Line 3: What do you mean by "range 1". Is this the value of the standard

deviation? “

With “range 1” we mean that the amplitude (maximum – minimum value) of the signal is 1. Has been clarified in the manuscript.

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