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Climate of the Past
Discussions

Interactive comment on "On the Role of Volcanism in Dansgaard-Oeschger Cycles" *by* Johannes Lohmann and Anders Svensson

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The authors of this interesting discussion paper study the temporal co-occurrence of Dansgaard-Oeschger (DO) event related rapid glacial climate changes inferred from a suite of published Greenland ice cores with synchronous sulfur spikes in bipolar high-latitude ice cores indicative of strong volcanic eruptions. They report detectable volcanic signatures within very few decades prior to the onset of DO events to occur more frequently than expected by chance. This finding may be indicative of strong volcanic activity superimposed to a complex and intrinsically bistable dynamics being able to provide the essential "momentum" for the climate system close to a transition point becoming forced to tip to the other state. This is an interesting observation –

generally consistent with our simplistic mechanistic understanding of the behavior of stochastic bistable systems – that clearly deserves being reported here and further studied in future work. It is important to note that the analysis presented here does not strictly imply evidence of causality between episodes of strong volcanic activity and the timing of DO events (yet provides indications that there might be some kind of statistical linkage), while a more detailed mechanistic explanation of such a link remains still somewhat vague. This is however not to be understood as a criticism to the present study, which raises a valuable point requiring future exploration.

Besides addressing a previously largely unexplored aspect of glacial climate variability, the present work is innovative in bringing together very recent paleoclimate datasets (with full awareness of their intrinsic uncertainties) with a simple yet appropriate statistical/stochastic analysis methodology. Potential issues regarding the employed datasets and their possible limitations have already been discussed in great detail by another reviewer, so that I do not want to further elaborate on this aspect here in detail.

One point of concern could be the fact that a large time interval (24.5-16.5 ka b2k) has not been analyzed, not because of an absence of DO events, but due to a lack of synchronicity of bipolar ice cores required for the identification of episodes of very strong volcanism. (So we do not have an absence of volcanic activity during that time period, but just an absence of synchronizable events, as far as I understand the present work.) This implies that the available record of bipolar sulfur anomalies in ice cores has to be cut at specific points that largely reflect the author's choice. Since the affected time interval includes only a single DO event (GI-2), this might not affect the statistic much at first glance. However, in the end this depends markedly on the specific formulation of the statistical hypothesis addressed, which should be further clarified in a revised version of this interesting manuscript.

In a nutshell, the authors use an analysis methodology that is largely equivalent to event coincidence analysis (ECA) introduced by Donges et al. (2011, 2016). In what follows, I will adopt their notion, being fully aware of the corresponding limitations. ECA is based on “counting” the number of co-occurrences between two types of events, where co-occurrence is defined by introducing two parameters. (a) A time shift parameter (allowing the quantification of delayed responses, which is essentially assumed to be zero by the authors of the present study, a setting that may be questioned according to substantial inertia of the studied type of climate transition essentially happening in the Atlantic ocean). (b) A tolerance window (addressing time uncertainty of events along with possibly distributed response times, which is also considered by the authors of this study). One question to the authors would therefore be if it would not make more sense for their work to also consider a minimum response time instead of only a maximum possible delay (as their free parameter “Tolerance”).

Next, ECA can be employed to address two related yet different research questions. (a) The likelihood of an event of type A (DO event) to be closely preceded by at least one (the latter is important, since events may also cluster in time) event of type B (sulfur spike/strong volcanic activity). This is what Donges et al. refer to as “precursor event coincidence rate”. (b) The likelihood of an event of type B closely preceding at least one event of type A (in the present context, “at least” would be irrelevant since DO events to not occur in such close succession). Donges et al. termed this “trigger event coincidence rate”.

Along with the former distinction, we get to the point where the missing time window around GI-2, as well as the definition of the full time span of observations becomes potentially relevant. When using precursor rates, we only need to fix the timing of known DO events within the admissible period of observations and do not need to take care of its actual span. There is no interest in using sulphur spikes as predictors for approaching DO events, and hence, there are no “false positives”. On the other hand, one could also be interested in the reverse research question of the predictability of DO events based on volcanic eruptions. In the present case, the latter question does not make much sense to me scientifically (DO events obviously require a certain preconditioning of the climate system, and volcanic activity acts mainly to determine

the actual timing of the event to happen, according to the research hypothesis of the authors). However, this question would also be partially ill-posed due to the imperfect time coverage of the identified sulfur events and the “subjective” choice of the exact time period considered for such an analysis.

I do not discuss this aspect here in such great detail because I see any obvious error in the analysis presented. However, (i) it might be worth clarifying those points in the manuscript – also in relation to the concerns of the other reviewer - and (ii) it appears to me important to be precise about what kind of research question the presented analysis is able to address, and which not. This clarification also helps motivating why the analysis setting used in the present work is appropriate. If, for example, DO events would not always be separated by much longer time intervals than the tolerance window used for the analysis (or if they were not sufficiently rare), this would render the assumption of a binomial distribution for studying coupling between two presumed Poisson processes an invalid basis for a test statistic. This aspect has also been discussed in detail by Donges et al. (2016).

Regarding the latter assumptions beyond the binomial distribution of event co-occurrences, I am however wondering about another aspect. The authors have well addressed the waiting time distributions between sulfur anomalies and reported convincing evidence for an absence of significant deviations from an exponential distribution, supporting the hypothesis that the volcanic eruption episodes can be well approximated as an uncorrelated temporal point process (Poisson process). However, in order for the employed analysis to make full sense, it would also be required that the DO events follow a Poisson process as well, which is clearly not the case due to the known shape of the waiting time distribution peaking at a 1.5 ka time scale. Strictly speaking, if one of the two type of events violates the assumption of uncorrelated events in such prominent way, the confidence bounds derived from the theoretical reasoning in Section 2.3 may not apply anymore. To clarify this aspect, I recommend a simple numerical check based on random event sequences leaving the waiting time distributions of both types of events invariant. A possible alternative would be repeating the analysis as performed in the manuscript, but successively shifting the series of volcanic events relative to the DO series and counting how often the empirical co-occurrence frequency between both types of events is reached at time shifts for which one would not expect any statistical link between both to apply.

Other comments:

1. Can strong sulphur anomalies in bipolar ice cores really be attributed to single strong eruptions? Or would it also make sense to check for close successions of events, each of which providing a small “kick” to the system towards the point of instability? Regarding Late Holocene climate variability (e.g., the transition between Medieval Climate Anomaly and Little Ice Age), it had been reported that in addition to solar variability, an enhanced period of volcanic activity might have had a crucial impact for this millennial scale transition to occur (e.g. via bistability of the North Atlantic subpolar gyre, cf. Schleussner et al. 2015). This is of course not strictly related to the problem studied in the present work, but I wonder if one might draw upon some analogies here.

2. It might be important to study more systematically the effect of different magnitude thresholds to sulfur spikes in bipolar ice cores (and, hence, the density of identified volcanic events) on the results of the present study.

3. In terms of stacking different Greenland ice cores, I am wondering about the relevance of interpolation suppressing high-frequency variability. At which time scale is high-frequency variability actually uncorrelated among the different cores, and what would this imply for the rationale of high-frequency variability just being considered as noise? I suppose this is mainly related to the aspect that DO events have a strong (slow) ocean component while being forced here by high-frequency (intra-annual to inter-annual) atmospheric variations carrying the volcanic signal. It might be helpful if the authors could elaborate a bit further on this aspect in Section 2.1.

4. In Section 2.2, the definition of the “stadial mean” used as a benchmark for iden-

tifying the DO event onset appears a bit opaque to me. For sure, both stadial mean and variance may depend markedly on the actual reference period taken into account. Related to this, as well as to the previous comment: What do the authors define as “noise level”?

5. I suppose that “nearest eruption” in Fig. 2 etc. always refers to the nearest preceding eruption. This should be clarified in the caption. Is it actually possible to uniquely identify this term, given remaining statistical uncertainty regarding the timing of DO event onset?

6. Can you elaborate a bit more on the plausibility of possible mechanisms of volcanic activity triggering DO events at decadal or even multidecadal lags?

7. In my opinion, the manuscript title might deserve a bit more precision - the manuscript topic is quite a bit narrower than the title suggests.

Technical comments:

- p.4, l.1: “In the remainder. . .”

- p.4, l.19: “except for GISP 2”

- p.7, l.13: “eruptions”

- p.9, Fig. 3, caption, l.6: remove “gives indicates the” (same also in Suppl. Mat., Fig. S4)

- Suppl. Mat., Fig. S3, caption, last line: “hypotheses”

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