

# ***Interactive comment on “Objective extraction and analysis of statistical features of Dansgaard-Oeschger events” by Johannes Lohmann and Peter D. Ditlevsen***

## **Anonymous Referee #1**

Received and published: 16 April 2019

Review of paper “Objective extraction and analysis of statistical features of Dansgaard-Oeschger events” by J. Lohmann and P. Ditlevsen

### Summary:

This paper provides a very thorough statistical analysis of ice core records evidencing DO events in the last glacial interval (mostly the NGRIP d18O record, but some comparisons with the GRIP record are performed), as well potential external forcing factors such as among others global ice volume (as inferred from benthic d18O) and atmospheric CO<sub>2</sub> from Antarctic ice cores. The focus is on first-order variability, and the proposed piecewise-linear fit is a suitable method to extract features such as stadial

[Printer-friendly version](#)

[Discussion paper](#)



and interstadial durations, as well as warming and cooling rates. The study provides a comprehensive summary of statistical features associated with the DO cycles, that should prove useful for benchmarking past future modelling studies attempting to explain the DO cycles in terms of physical mechanisms. In this sense, this manuscript provides a very valuable contribution to the literature of DO variability.

The paper is written very well, the authors' reasoning is clear and easy to follow, and the statistical analyses are performed very carefully, with appropriate account of uncertainties of different kinds. I thus suggest publication as soon as the following, minor comments are addressed:

Comments:

1. In the abstract and at several more occasions, it is stated that the goal of this paper is to obtain a mechanistic understanding of the DO cycles. This is not true in my opinion, since only statistical features are reported. These features can be used for benchmarking modelling studies testing different mechanisms, but this is not done in this paper.
2. Similarly, I would suggest to remove the word "causal" from the abstract and remainder of the manuscript: Only statistical similarities are tested, and no conditioning is performed to infer conditional dependencies. Also, no dynamical models are used, which could provide some hints at actual causality.
3. I don't think that previous work on the DO cycles is sufficiently recognized by the authors. For example, in the introduction it is stated that there are no established theories of the DO cycles. This is not true, there's a multitude of competing hypotheses, which can be broadly divided into two classes, namely those focussing on AMOC changes induced by freshwater forcing (the works of Ganopolski and colleagues and Timmermann in particular) and those focussing on sea ice changes (Li et al., J. Clim. 2010, Dokken et al., Paleococ. 2013, Petersen et al., Paleococ. , 2013, Boers et al., PNAS 2018, Sadatzki et al., Sci. Adv. 2019) In the last few years, DO-like oscillations have

[Printer-friendly version](#)

[Discussion paper](#)



been reproduced even in comprehensive models (Peltier & Vettoretti GRL 2014, Vettoretti & Peltier GRL 2013, Zhang et al., Nature 2014, Klockmann et al., 2019). A brief paragraph giving credit to these models seems in order.

4. Moreover, it is one of the key results of the study that global ice volume (inferred from benthic  $\delta^{18}\text{O}$ ) or temperature have a strong influence on the interstadial durations; this observation has, however, been made previously: Mitsui and Crucifix (Clim Dyn 2017) show from a statistical point of view that including this forcing is supported by the data, and Boers et al. (PNAS 2018) use it explicitly to infer the interstadial cooling rate during interstadials.

5. The ultimate goal of this study is to provide the statistical basis for discriminating between different mechanisms to explain the DO events, but this comparison of different mechanisms is not performed. Do the statistical features you extract give some hints at which of the prominent hypotheses listed above (point 3) are more likely? It would be nice to include at least a discussion on this at the end, as it is somewhat promised in the beginning.

---

Interactive comment on Clim. Past Discuss., <https://doi.org/10.5194/cp-2019-19>, 2019.

Printer-friendly version

Discussion paper

