Clim. Past Discuss.,
https://doi.org/10.5194/cp-2019-19-RC2, 2019

Climate of the Past Discussions

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

Anonymous Referee #2

Received and published: 18 April 2019

Review of Lohmann and Ditlevsen 2019

Lohmann and Ditlevsen (2019) evaluate abrupt DO variability in the high-resolution NGRIP d18O record using piece-wise linear fitting. They perform correlation studies of the various model parameters with other climate records.

I find it difficult to judge this paper. On the one hand, the work is carefully done, the analysis statistically and mathematically robust, and the paper is well written. One the other hand, after 31 pages of manuscript the reader has not really learned anything that was not already described in the literature. The authors confirm the 17-yr old result by Schulz (2002), briefly revisit the result by Buizert and Schmittner (2015), and

confirm some of their own conclusions from Lohmann and Ditlevsen (2018) using a new method. One new element (the idea that cooling rates control interstadial duration, a variation on the Schulz argument) is based on the fallacy that correlation proves causation. Moreover, I have some major conceptual problems with their approach, which I will outline below.

In balance, the paper in its current form contributes too little to warrant publication. After major revision (some suggestions below) it may be reconsidered.

(1) In deciding which events are stadials the authors rely completely on (Rasmussen et al., 2014) – R14 hereafter, while this is of course the best starting place, it does mean that the authors inherit all the assumptions and historical numbering conventions that are in that study.

DO cycles come along a wide spectrum of shapes and timings, and deciding what is a stadial (indicated with numbers in R14) vs. a cold sub-event (lettered with b, d, f) in R14) is a very arbitrary choice. This leads to some strange results from Lohmann and Ditlevsen, such as stadials that lasts only 20 years (P18 L24). From a climatic perspective, such short cold periods are more likely to be cold sub-events rather than true stadials.

The cause of the cold sub-events (lettered with b, d, f) is of course not well known, but most likely they represent outburst floods (e.g. from proglacial lakes) that temporarily increase N-Atlantic sea ice cover and cool Greenland, the 8.2 ka event is the posterchild for such events. They occur most frequently during periods of ice sheet decay (MIS4-MIS3 and MIS2-MIS1 transitions) supporting this interpretation. When is a cold period a sub-event and when a true stadial? R14 applies some criterion of baseline separation; this makes GI-24.1b a "cold event" and GS24.2 a "stadial", even though they appear nearly identical and GS24.2 may just be a larger cold event (bigger outburst flood?). In other "stadial" cases (GS 14, GS 23) baseline separation is not achieved and R14 calls them stadials only because they choose to adhere to historical

DO numbering (based on older low-res cores). Many DO events have a clear warming period at their end (7a, 8a, 12a) – could DO 13 be one of those? The difference is arbitrary.

DO 24, 17, 16, 15 each consist of 2 DO events in the Greenland-centric R14, but in for example the Hulu speleothem record and the Iberian margin SST these events are recorded as single (and not double) events. Could these be single events from the perspective of the overturning circulation, but separated events in Greenland due to regional effects (like freshwater)?

These sub-events are always the outliers in the scatter plots (5a, 6a, 6b), making this an interesting point to consider. The authors note that "Buizert and Schmittner (2015) lump each of the interstadials 24, 23, 21, 17, 16, 15 and 2 together into one event, even though they are comprised of two events." But are they two events? If we put 100 paleoclimate researchers in one room, my guess is that less than half would call GS 24.2 a true stadial (minimum requirement to support the claim that DO 24 is comprised of 2 events). The vote may be different for GS 15.2. I don't want to claim here that they are 1 or 2 events, just that this depends completely on arbitrary definitions, and that reasonable people can disagree on the number of "true" DO cycles give that the cold sub-events are ubiquitous and can have a wide range of sizes.

One could turn the question on its head, and argue that, based on the fact that the sub events of DO 24, 23, 21, 17, 16, 15 consistently show up as outliers in scatter plots, and fit the climatic trends when lumped together, they are actually single (and not double) events from the perspective of the global climate system and oceanic overturning circulation.

One way to make the present paper more interesting is to try out different definitions of a stadial (beyond adopting R14), and see what definition may minimize scatter in the plots. What do other climate archives like speleothems suggest these events looked like? Would a duration threshold (e.g. cold period longer than 300 years) be a better

way to define a stadial? There are probably good climatic reasons why DO timescales are linked to global climate markers (as argued by e.g. Schulz 2002 and Buizert and Schmittner 2015), so an approach of finding an objective (multi-proxy?) stadial definition that minimizes scatter is justified in my view.

I want to emphasize this is not a critique of R14 itself, which seeks to provide a consistent nomenclature for events and has succeeded in doing so. The problem arises when R14 is mistaken for an objective and meaningful decision on which events are "true" stadials – which was never the aim of that study. The author's algorithm has the liberty to change the timing of transitions, but not the number of events, and so does not challenge the R14 definitions.

To summarize an excessively long comment, the approach by the authors has a fundamental and tenuous assumptions that is neither acknowledged nor examined. They interpret the R14 beyond its intended use as a climatically meaningful distinction of which cold events are stadials and which are not. While the shape fitting is done with much mathematical rigor, these underlying assumptions will always limit the validity of their conclusions. Trying different definitions for stadials would be an interesting research direction, as well as different proxy archives.

(2) One of the main conclusions of the paper is that cooling rates "control" the interstadial durations (P14 L13, P16 L21 and elsewhere). They are only correlated, which does not prove any kind of causation or control; both could be controlled by a third parameter such as AMOC strength, CO_2 or SH temperature. This correlation was discovered by Schulz (2002), who actually argues for an ice volume control.

The authors further suggest that the "interstadial duration is determined as soon as the rate is established" and that the duration is "determined" a few hundred years after the onset. However, if both cooling rate and duration are controlled by a third parameter (like AMOC strength), this interpretation is strange. In my view, Fig. 5b simply reflects the amount of data needed to determine the slope in a noisy time series, and is not some time scale on which the interstadial duration is somehow “determined” by the coupled climate system.

On a side note, I am uncomfortable with language of transitions being “determined” thousands of years in advance. Interstadial terminations occur because at that time interactions between components of the climate system are favorable for such a transition (including “noise” components). The climate system is not a decision-making entity that plans things centuries to millennia ahead. The word “predicted” seems more appropriate. So please revise.

If the authors want to argue for their mechanism (cooling rate controls duration) they will have to provide a meaningful climatic pathway for such control, which is currently lacking. At the very least they have to clarify all the language suggesting causation.

(3) The discussion section is basically a lengthy summary of the preceding chapters, which is not the function of a discussions section. This will need to be rewritten. There are many caveats and assumptions that need to be addressed, and the work can be placed in a broader paleoclimatic context.

(4) In earlier work, Ditlevsen suggested that the DO transitions are purely noise-driven – others have probably made that suggestion also (Ditlevsen et al., 2007; Ditlevsen et al., 2005). Given that event durations are clearly correlated to global climate parameters, is that still your view?

(5) The referencing of published material is very minimal for a paper of this length on a topic that is so extensively written on. Much is in fact known about the DO cycle (despite the author’s claims to the contrary). Marine sediment data clearly show a link to the Atlantic ocean circulation (see e.g. review by Lynch-Stieglitz, 2017), and climate modeling studies clearly implicate sea ice cover in the North-Atlantic. Many remote teleconnections have been clearly described, and several drivers have been proposed. Also, many more papers have used similar statistical techniques on the DO cycle that should be referenced.

(6) The authors ignore Heinrich events, while it is commonly believed that H-events lengthen the stadials in which they occur by putting freshwater into the North Atlantic. Please discuss this in the stadial duration section.

(7) The paper is very long and could be shortened substantially.

(8) There is no data availability statement.

(9) Are the d18O data corrected for mean ocean d18O? This would of course influence the cooling rates of long interstadials.

(10) The work mostly just confirms earlier work. I would encourage the authors to think about ways to broaden or improve the scope, to reward the reader with something new.

(11) The uncertainties in the fitting parameters are carefully estimated. Could they be listed in table 2? I imagine that for the very short interstadials (<400 yrs) things like the gradual cooling slope are not well constrained.

(12) Why do you use a constant stadial level? Do the stadial levels resemble Antarctica, as suggested by (Barker and Knorr, 2007).

(13) Have you tried including other records? The Ca record has much better signal to noise, allowing for better timing determination. I think marine and speleothem records have much to add to the problem also.

Comments on the text:

P1L7: remove “mechanistic”. This is not attempted, in my view. Climate dynamics are not discussed.

P1L14: “largely unknown”: A lot is known, the gist of which could easily be summarized in a few sentences.

P2L16: “we do not have to rely on any subjective choice of stadial and interstadial onset or levels”. This is a misrepresentation, in my view. The subjective choices were all made by

R14. The algorithm does not have the ability to decide independently how many events there are, and whether individual cold periods are stadials or just cold sub-events.

P2L24: “In contrast. . .state” But the transitions are not purely noise-driven, since the period durations are linked to global climate, no?

P3L13: Do the short cold events influence the fitting?

P4L29: Instead, we use . . . basin-hopping. This means very little to much of your readership. Please elaborate or leave out.

P5L8: “climate features” should be “d18O features”

P5L12: “mechanistic” I don’t think this is the right word. Climate dynamics are barely discussed.

P7L1: but you use 90% confidence, correct? How many false positives for 0.9?

P7 section 3: why is DO 1 omitted?

Fig 2: can you comment on DO 23.1? You routine picks its termination 3 ka before R14 does.

Section 4.1.1: Is there any conceivable mechanism by which cooling rates determine interstadial duration? Correlation and causation are falsely equated here.

P13 L13: What is lambda? Wasn’t the cooling rate called s_2? Also specify whether you’re talking about the gradual or fast cooling rate

P14L3: fixed cooling rate. . . You mean all interstadials would have the same cooling rate? We know this to be untrue. I do not understand this hypothetical scenario.

P14L8: “strong control”. Correlation is not causation

P14L22: log-normal: is this meaningful since you only fit the tail of the log-normal

distribution?

P15L10: “determined” change wording. The climate system does not plan ahead millennia. If you want to argue for such a mechanism you should at least provide a dynamical pathway, even if speculative.

P16 L19: Not necessarily. Both duration and cooling rate are controlled by heat transport and interactions within the climate system, and appear to correlate to a third parameter like CO2, ice volume, or similar. Within a few hundred years you can detect the cooling trend within the noisy time series, and because of the correlation you can predict the interstadial duration with reasonable accuracy at that time. Nothing is “determined” a few hundred years into the event.

P16L21: rewrite.

P18L16: see comments above. Whether these are one or two events depends on ones definition of a stadial. There is no widely accepted definition, and R14 provides nomenclature only and is not the final authority on this matter. Other archives should also weigh in on this question, since Greenland may reflect regional effects.

P18L24: a 20yr stadial is probably not a stadial in most people’s definition. A time threshold (250 yrs?) may be of use in defining stadials. 250 yrs provides a rough timescale of Atlantic overturning (volume divided by rate), and makes some intuitive sense for that reason.

P18L27: the data ARE consistent with. . . (data are plural).

Fig 7a: all outliers are in MIS5. Is that relevant?

P19L6: common forcing makes most sense, right? Weak AMOC means low temps and long stadials?

Discussion: This is just a lengthy summary. Please discuss the strengths and shortcomings of your method, and place it in a broader context.

P24L5: “cooling rates clearly determine” rewrite

References:

Barker, S., Knorr, G., 2007. Antarctic climate signature in the Greenland ice core record. Proc. Natl. Acad. Sci. U. S. A. 104, 17278-17282.

Buizert, C., Schmittner, A., 2015. Southern Ocean control of glacial AMOC stability and Dansgaard-Oeschger interstadial duration. Paleoceanography 30, 2015PA002795.

Ditlevsen, P.D., Andersen, K.K., Svensson, A., 2007. The DO-climate events are probably noise induced: statistical investigation of the claimed 1470 years cycle. Clim. Past. 3, 129-134.

Ditlevsen, P.D., Kristensen, M.S., Andersen, K.K., 2005. The recurrence time of Dansgaard-Oeschger events and limits on the possible periodic component. J. Clim. 18, 2594-2603. Lohmann, J., Ditlevsen, P.D., 2018. Random and externally controlled occurrences of Dansgaard–Oeschger events. Clim. Past 14, 609-617.

Lynch-Stieglitz, J., 2017. The Atlantic Meridional Overturning Circulation and Abrupt Climate Change. Annual Review of Marine Science 9, 83-104.

Rasmussen, S.O., Bigler, M., Blockley, S.P., Blunier, T., Buchardt, S.L., Clausen, H.B., Cvijanovic, I., Dahl-Jensen, D., Johnsen, S.J., Fischer, H., Gkinis, V., Guillevic, M., Hoek, W.Z., Lowe, J.J., Pedro, J.B., Popp, T., Seierstad, I.K., Steffensen, J.P., Svensson, A.M., Vallelonga, P., Vinther, B.M., Walker, M.J.C., Wheatley, J.J., Winstrup, M., 2014. A stratigraphic framework for abrupt climatic changes during the Last Glacial period based on three synchronized Greenland ice-core records: refining and extending the INTIMATE event stratigraphy. Quat. Sci. Rev. 106, 14-28.

Schulz, M., 2002. The tempo of climate change during Dansgaard-Oeschger interstadials and its potential to affect the manifestation of the 1470-year climate cycle. Geophys. Res. Lett. 29, 2–1-2–4.

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