Reply to reviewer comment #1

Review of Erhardt et al: Decadal-scale progression of Dansgaard-Oeschger warming events

Erhardt et al. present high-resolution Ca and Na records from the Greenland NGRIP and NEEM ice cores, and combine these with NGRIP annual layer thickness (an estimate of past accumulation) and d18O data. The study evaluates the phasing of the various records during abrupt climate events of the Dansgaard-Oeschger cycle, and finds a clear sequence of events consistent with some previous work. Accumulation rate changes lead, followed by Ca, d18O and finally Na. Taken at face value, this sequence would argue for event initialization at lower latitudes, with the sea ice response associated with DO events coming last.

These records are of great value to the scientific community, and the analysis is meaningful and appears to be rigorously done (request for minor clarification below). This paper is clearly a great contribution to the literature, and I have only some minor suggestions that may improve the clarity and interpretation.

Thank you!

(1) The current manuscript only describes the relative phasing of the onset, midpoint and endpoint of each transition. What is missing is an analysis of the duration of each of the transitions, to put the lead/lag values into perspective. For example, if the transition were to take 100 years, then a 10-yr lead indicates that the climatic components reflected by these records co-evolved; if the transition were to take 5 years only then the same 10-yr lead suggests a decoupling, with the shift in one component (say the jet) completed before the others respond.

I request the authors add one panel to Figures 3 and 4 each that gives the transition duration.

We agree that this is an important point. Because both Figure 3 and 4 show difference relative to Na, we prefer not to add absolute information to these figures. However, to answer the reviewer's comment, we have added the relevant information to the text (P 7, L 11 ff):
"For all of the transitions, the inferred timing differences relative to the onset of the transition in Na are smaller than the duration of the transition itself in each of the proxy records.
That means that none of the proxies exhibit a complete stadial-interstadial transition before the onset of the transition in the sea-salt aerosol concentration."
And later in the discussion (P 12, L 25 ff):
"The fact that for all transitions the inferred timing difference relative to the transition in Na is smaller than the duration of the transition in that parameter indicates that the respective parts of the climate system co-evolved over the transition. That means that the changes in atmospheric circulation at the DO-onset where not completely decoupled from the change in sea-ice cover and Greenland temperature."

Many of us are visually oriented. Would it be possible to show the D-O average transition in Ca, Na, lambda and d18O together in one plot like is done in Fig. 2 for Ca? Either the data or just the fits (if the data are too messy). This would really give a nice visual representation of how the "average" transition occurs.

> Because all of the transitions have different lengths it is not possible to meaningfully stack them. This is the reason why we chose to combine the estimates of the relative onset timings and not the complete transitions. We would like to point out that Figure 2 only shows the fit of a single transition, not a stack.

(2) The paper only analyzes the glacial-to-interglacial (or D-O warming) transitions, and not the interglacial-to-glacial (or D-O cooling) transitions. Have you tried analyzing the latter? I would be very interested to see the phasing for these transitions also. I imagine it is more challenging given the smaller and less abrupt nature of these transitions but I think it would be valuable nevertheless.

> We agree with the reviewer that analyzing the cooling transitions would be very valuable. However, in the presented manuscript, we deliberately chose to focus on the DO onsets as they are much more clearly distinguishable in the records and are possible to describe using the relatively simple model employed here. Furthermore, the fundamental difference between the warming and cooling transitions in their manifestation in the records suggests a different mechanism and thus merits its own separate investigation which we plan to do in the future.

(3) How precise is the depth registration of the various CFA components relative to one another? And relative to the layer thickness and d18O records? I can imagine there may be cm-scale offsets, which could become important given the extremely small time phasings that the authors interpret. Please address this briefly.

> Between the different CFA records, the depth assignment is accurate to within a few mm as they are measured on the same piece of ice and the relative component alignment is measured regularly as described in the cited references. Because the NGRIP CFA records form the data basis for the annual layer-counting, the CFA depth scale is the fundamental depth scale to which the layer thicknesses and dating refers. The relative uncertainty of the depth assignment of the CFA/layer thickness data to the d18O record is fundamentally limited by the accuracy of the subsampling within each 55 cm piece of ice (called a bag). Typically, sample depths are measured with a meter stick and are accurate to within half a centimeter or better.
> We added the relevant information to the manuscript. (P 3 L 29 ff):
> "Between the individual data sets the co-registration uncertainty is limited by the absolute depth assignment of the data sets. This uncertainty is typically on the order of a few millimeter to at most single centimeters and thus on the which translates to a co-registration uncertainty in the sub-annual range."

(4) Appendix A was not completely clear to me, and I think it should be elaborated on in some more detail for the lay reader. Do I understand correctly that the function that is being fit to the data is the ramp function plus an AR(1) noise function, and that the parameters of both are varied in the Monte Carlo sampler?

The language of prior and posterior distributions suggests this is a Bayesian approach – please confirm.

How is the goodness of fit evaluated, and what criterion is used in the MC sampler to accept or reject individual solutions?

It would help greatly if you could add a figure showing how several individual iterations of the fitting process look like.

> We believe that a thorough introduction to Bayesian statistics and Markov Chain Monte Carlo methods is beyond the scope of this paper.
> Nevertheless, we want to give an intuition on the sampling process here:
> In the sampling process, the sampler randomly walks the whole parameter space, in this case represented by the four parameters describing each transition and the two noise parameters. At each step, the sampler evaluates the likelihood of observing the data given the current set of parameters and accepts or rejects these parameters with a probability proportional to this likelihood. If performed over a large number of samples, this process will yield samples from the posterior distribution which is what we use for our analysis (see for example Gelman et al., 2013)
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> *Gelman, A. et al. (2013), Bayesian data analysis, Chapman and Hall/CRC*

(5) I am confused by the statements below Fig 4 (Starting on P9 L18). If both Ca and lambda lead Na by ~10 years, how come these two are not necessarily synchronous? This is very counter-intuitive; all records are evaluated on the same depth scale, so why wouldn't they be? I think the relative phasing of all these records is the central result of this paper, so it would be important to establish a robust sequence of events. What would be needed to establish synchroneity of Ca and lambda? Would you need to run the analysis again relative to the Ca transition instead of relative to the Na transition? If not too much work, that may be worth doing, given the importance for the interpretation.

I could imagine a 4x4 matrix for NGRIP with the lead/lag of each of the records evaluated relative to the others, and a 2x2 matrix of the same for NEEM.

> We agree that this is indeed counter-intuitive. We slightly rephrased the sentence to make this point clearer (see below) The reason for this is that the uncertainties both for the lags in the individual transitions as well as for the combined evidence is very strongly dependent on the estimates from the layer thickness timeseries. Depending on how these highly correlated uncertainties are projected onto the different axis (lt-Na, Ca-Na, Ca-lt) slightly different distributions arise. For Ca and lt, the timing difference at the onset of the transition shows a lead of Lambda over Ca with 4 (-5/+4) yr that is not incompatible with a zero lag at the 95% probability level.
> A different way of looking at the order of events is to calculate the average probability of the order of the transition onsets. This shows that the transition Lambda and Ca are about equally likely to occur first, whereas Na and d18O are about equally likely to occur last. From this type of analysis, one can also establish the most likely order of onsets during the warming transitions which is lt, Ca, d18O, Na for NGRIP and Ca, Na for NEEM.
> We have added these results in a new Figure focusing on the NGRIP ice core into the revised manuscript (P9, L16 ff and Figure 5).
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> "Note that the density functions shown in Figure 4 cannot be used to infer timing differences between the other parameters. This is a direct result of the estimates being

conditional on the timing of the transition in sodium, leading to large correlations between the lag estimates for the other parameters. That means that even though e.g two probability density functions of the differences relative to the transition in sodium largely overlap, that does not necessarily mean that the timing difference is equal to zero. In the case of the timing difference between the transition onsets of the increase in annual layer thickness and the decrease in Ca^{2+} concentrations the combined lead of the change in annual layer thickness is not larger than zero at the 0.95 probability level with 4(-5, +4) years. To establish the most probable sequence of events at the transition offset we calculate the average order of the onset times, shown for NGRIP in Figure 5. The average positions show that the change in accumulation and Ca^{2+} concentrations about equally likely occur first whereas the transitions in Na^+ and $\delta^{18}O$ about equally likely occur last. The same analysis for the NEEM results confirms this sequence."

(6) All age axes have a "BP" label. Do you use BP 1950 or b2k? This is a contentious point in the ice core community (Wolff, 2007), but BP 1950 is the best choice in my view based on precedent in the literature and convention of the radiometric dating communities. At least specify which is used.

We had only specified this in the caption of Figure 1, but we have now also added this to the text (P4, L1)
"All ages are given relative to 1950."
We have also changed all axis-labels to "before 1950" instead of "BP" to avoid confusion with radiocarbon ages.

Line-by-line comments:

P1 L17-19: The phrase "DO event" is unfortunately ambiguous, with some people equating them to the abrupt warming phases, and others to the interstadials. To avoid this, consider ". . . millennial-scale abrupt climate change, called the DansgaardOeschger cycle (REFS). During abrupt DO warming, . . ."

Thank you for the suggestion. To make the text more consistent we changed the wording throughout the text so to specifically state that we are looking at the onset of the D-O events. However, we deliberately do not use the term "DO cycles" to avoid any notion of cyclicity in these events.

L22: I assume your are still talking about DO warming here? Please specify that the changes described are for the warming phase.

Clarified in the text.

P2 L6: Consider replacing Henry et al. with the recent review by (Lynch-Stieglitz, 2017), to avoid arbitrarily picking one study out of dozens that demonstrate the link to AMOC.

Done.

P2 L23: "Some of . . .". I think you can safely say "All of" (or "most of " to be conservative). I am not aware of any model simulation or theory that does not involve sea ice as either the trigger or amplifier. You simply cannot get that much warming that quickly without sea ice.

We have now changed the sentence and use "Many of…". Most of the experiments are run without an interactive sea-ice model and thus cannot realistically inform on sea-ice feedbacks. We do however agree that most of the recent model studies with coupled models involve the sea ice as triggers or amplifiers.

P3 L20: "exact co-registration". How exact? Please specify relative and absolute depth registration of various CFA components.

We added the relevant information to both the CFA data sets as well as to the end of the paragraph:

"…exact co-registration of the aerosol concentration records at the millimeter scale…" (P3, L21)

"Between the individual data sets the co-registration uncertainty is limited by the absolute depth assignment of the data sets.
This uncertainty is typically on the order of a few millimeter for CFA data up to around a centimeter between d18O and CFA data, which translates to a co-registration uncertainty of sub-annual to annual range." (P3, L29 ff)

P3 L23: Do you use the actual single layer annual counts, or the 20yr averages that are publicly available?

The data presented here is based on the single annual layer counts and new 10yr averages are published alongside this paper for the whole 60 ka as well as higher resolution datasets for the individual transitions.

P3 L27: All data ARE shown. . . (the word "data" is plural not singular)

Updated accordingly.

Figure 1+3: Please consider plotting the age axis in reverse (so time goes from left to right). This is what much of the paleoclimate literature is moving towards. That is also what Figure 2 uses.

To make all axis directions consistent, we changed the axis in Figure 2 to match those in the other figures.

P5 L30-31: So are your interpreting the changes in terms of the source strength only? Or does transport to the site dominate? Some would argue for the latter.

The exact interpretation in terms of source and transport changes is described in detail in the discussion section of the paper. (P10 ff)

P5 L33: Do you actually fit an exponential, or do you fit a linear to the log(Ca) time series?

We fit a linear function to the log(Ca) and log(Na) time series, as described in the appendix. We added this also to the main text for clarification: (P6, L2 ff)

"…or exponential transition (i.e. a linear transition fitted to the log-transformed data for all other records) between two constant levels…"

P6L16: "decreases" should be "increases" here, right? (it goes from 2 to 7yr, so increase?)

We prefer decreases in this case as "high resolution" typically referrers to lower sampling intervals and vice versa.

Figure 2: please add units to the y-axis labels. Also, how come the fit is so smooth / rounded? Isn't the fit a linear ramp? Is this because you average over many solutions?

Yes, the smoothness is a feature of the marginal posterior median ramp which is shown here. The individual realizations are linear ramps with sharp kinks. We have now added units to the y-axis labels

P9 L5: Buizert et al. 2015 should be cited as WAIS Divide Project Members 2015.

We have now updated the reference accordingly.

P10 L23-24: Add a reference for this claim.

We have now added Pausata et al .2009 and Merz et al. 2013 as references.

Pausata, F. S. R. et al. (2009), Changes in atmospheric variability in a glacial climate and the impacts on proxy data: a model intercomparison, Climate of the Past, 5489-502, doi:10.5194/cp-5-489-2009.

Merz, N. et al. (2013), Greenland accumulation and its connection to the large-scale atmospheric circulation in ERA-Interim and paleoclimate simulations, Climate of the Past, 92433-2450, doi:10.5194/cp-9-2433-2013.

P10 L24: "lack of covariance" seems like strange phrase here. The records you are talking about are correlated with r > 0.95 probably.

We have now rephrased to "…lack of synchronous changes…"

P10 L32: "events" is confusing here. Are you talking about individual synoptic / precip events? or DO events, better specify more clearly.

We have now specified as "precipitation events".

P11 L15: This idea was suggested by (Seager and Battisti, 2007) and (Wunsch, 2006)

We have now added Wunsch, 2006 as original reference.

P11 L16: I had expected a larger discussion about wet vs. dry deposition. Could the coincidence of lambda and Ca changes be explained that way to some degree?

Ca is, as sea salt, very efficiently scavenged by snowfall events and its deposition is hence governed by the frequency of the precipitation events.

P11 L18: effect should be affect

Corrected, thanks for pointing this out.

P11 L34: This further supportS. . .

Corrected, thanks for pointing this out.

P12 L13: "reduction of the sea ice cover that ultimately coincided with the Greenland warming AND WAS PRESUMABLE A MAJOR DRIVER THEREOF" Again, I think it's hard (impossible?) to get such a large Greenland temp response without a change in sea ice cover.

We completely agree and we do not argue against the fact that the sea-ice change is likely the major driver of the warming in Greenland.

P18 L25: What is the rationale for taking the log of lambda instead of just lambda itself?

We took the log of the annual layer thickness, because it is very well described by a log-normal distribution as shown in Andersen et al. 2006.

Andersen, K. K. et al. (2006), The Greenland Ice Core Chronology 2005, 15-42 ka. Part 1: constructing the time scale, Quaternary Science Reviews, 253246-3257, doi:10.1016/j.quascirev.2006.08.002

P19: specify what all the symbols mean in your maths.

We have now added the missing symbols to the text.

References:

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

Seager, R., Battisti, D.S., 2007. Challenges to our understanding of the general circulation: abrupt climate change. Global Circulation of the Atmosphere, 331-371.

Wolff, E.W., 2007. When is the "present"? Quat. Sci. Rev. 26, 3023-3024.

Wunsch, C., 2006. Abrupt climate change: An alternative view. Quat. Res. 65, 191203.