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Climate of the Past
Discussions

Interactive comment on "Decadal-scale progression of Dansgaard-Oeschger warming events" *by* Tobias Erhardt et al.

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In this study the authors use high-resolution ice-core records of aerosols, water isotopes, and layer thickness from Greenland to examine phasing of different aspects of the climate system during Dansgaard-Oeschger events. They use objective techniques to tease out small leads and lags between the noisy records, showing that changes in calcium aerosols and layer thickness lead changes in sodium aerosols and water isotope ratios. They conclude that these lags suggest that changes in sea ice extent did not occur before other changes in the climate system, namely atmospheric circulation. The manuscript is very well written, the analysis is careful, and the discussion is well-argued. The results are quite impressive and should be of wide interest to the community. I have a couple questions and concerns that I hope the authors will address

and then several minor questions.

(somewhat) Major questions: The authors have made a compelling observation through their analysis of leads and lags between the proxy records. They attribute these differences in timing to aspects of the climate system that have different influence on the proxies. My first two questions have to do with whether these leads and lags could arise from other factors. I suspect that these concerns are not too important to the conclusions of this study.

1. How does the signal-to-noise ratio of the record influence fitting the transition model and the identification of starting, mid, and end points? Here I'm thinking of the SNR as quantified by the size of the transitions compared to the variance within the stadials and interstadials. I realize the fitting procedure takes into account the interannual noise and its autocorrelation. But if you have an idealized known ramp function with different levels of background noise, will the model find the same starting, mid, and endpoints?

One could imagine that an increased background noise could lead to the identification of time-shifted transition points depending on the fitting technique. One then risks conflating difference in the timing of signals between records with difference in one's ability to detect signals between records. I cannot tell from the description of the transition model alone how much of an issue this is to this analysis. Doing some simple tests with a couple different ramp-fitting and significant change detection techniques (though ones less sophisticated than the technique used by the authors), I find that different levels of interannual noise can influence the timing of a fitted transition, though not in all circumstances.

My particular worry here is that the substantially higher noise (interannual variability) in the Na records could lead to the identification of a delayed onset or shorter transitions compared to the Ca and other records. My worry is heightened slightly in that the relative timing seems to correspond to the level of background noise (at least visually): the d18O and Na timing are most similar among the proxies and also both appear to have much lower SNR (more noise) compared to the Ca and layer thickness records.

Do the mean lags depend on the amplitude of the DO event or the length of the transition between stadial and interstadial? This could be the case if the ramp-fitting depends on the amplitude of back ground noise. I realize this could be hard to determine since one needs to look at many events at once to see the mean lags. But a scatter plot of lags vs. event magnitude or fitted ramp duration could be informative.

I suspect that this concern is entirely accounted for by the very careful analysis of the marginal posterior densities of the onsets, midpoints, and endpoints for each proxy and the comparison between proxies, that the authors have already performed. It would however be helpful to see the influence, or the demonstration of the absence of influence, of the SNR on the fitting procedure given that the conclusions rest on the difference in timing with respect to Na in particular. I'd find it very useful to see this demonstrated on artificial ramp signals (of varying duration) where the true timings are known explicitly, with varying SNR, and especially with the SNRs relevant to d18O, lambda, Ca, and Na. It seems crucial to know that different SNR alone can not account for the difference in timing identified by the fitting procedure.

2. Can the authors rule out the influence of water isotope diffusion on the difference in timing of the d18O signals and those of Ca and layer thickness? Based on analysis of NGRIP (Gkinis et al 2014) and a similar site in Antarctica (Jones et al 2017, 2018), I'd guess diffusion lengths are on the order of 5-10cm through this interval and so are not insignificant compared to the annual layer thickness. Such diffusion lengths can have meaningful influence on the inter-annual and even decadal variability in the water isotope record (Jones et al 2018). I imagine that correcting the records used here for the potential influence of diffusion, if that would even be sensible, is far beyond the scope of this study. However, it seems entirely reasonable to estimate the influence (if any) of the smoothing implied by diffusion on the timing identified by the transition model fitting procedure. If you take identical idealized ramps, and smooth one with a time-scale reflective of water isotope diffusion lengths, will the fitting procedure identify

the same start, mid, and end points? I suspect these effects, if any, are small, though we are only talking about lags of a few years.

3. The most interesting conclusion of this study to my mind is the authors statement that "at face value, this sequence of events suggests that the collapse of North Atlantic sea-ice cover is not the initial trigger for the DO events..." Because of its potential wide interest, this statement deserves some scrutiny. It rests on the authors use of Na as a "qualitative indicator of sea ice cover" in the North Atlantic.

The discussion on Page 5, lines 3-28, highlights the debate over in the interpretation of Na very well. The authors interpretation is laid out on Page 5 lines 20-25. Markle et al 2018 find that most of the millennial variability in Antarctic sea salts can be explained simply by the changes in moisture rainout that are required to explain the water isotope record (these changes also explain most of the changes in Antarctic Ca variability and its relationship to water isotopes in both Antarctica and Greenland (c.f. their Figure 4)). This suggests comparatively small if any changes in sea salt source latitude or strength are needed to explain those Na records (though changes in those things are still possible of course). Is there evidence that this explanation for the sea salts is insufficient in Greenland? Are the observed changes in sea salt for example much larger than what one would expect from temperature dependent rainout alone? It was unclear to me from this discussion that the sea salt source strength (or even mean source latitude) should have a clear relationship to the sea ice edge.

If the main way sea ice influences Greenland Na is through its relationship to the variables driving the rainout effect, then a change in sea ice extent doesn't necessarily mean one should have a coincident change in Greenland Na, particularly at interannual timescales.

For example, one can imagine a scenario in which sea ice extent begins to retreat at exactly the same time as the changes observed in Ca initiate. Coincident increases in temperature and moisture removal would cause a decrease in the amount of Na (and Ca) reaching Greenland (as described by the authors). However, if that change in sea ice extent caused an increase in sea salt source production or a northward migration of mean source latitude (both debatable but reasonable, particularly the latter) this could temporally compensate for the increased removal. This combination of influences could lead to an apparent timing difference in the final Na signal observed in Greenland compared to the actual timing of sea ice changes (an example of the superposition of competing source and rainout factors on polar aerosols is given in the Supplement of Markle et al 2018 c.f. Figures S9). A somewhat similar scenario may be likely for the water isotopes, as changing sea ice extent could drive moisture source effects that could temporarily compensate for the decreased depletion driven by simultaneous changes over the ice sheet (these would be of the correct sign to compensate, though it would be hard to assess the potential size of this effect on the isotopes without analyzing deuterium excess records from the same cores). Quantitatively disentangling these influences may be well outside the scope of this study. But this does at least suggest a limitation to using Na as a qualitative indicator of sea ice, and should suggest some commensurate caution in the conclusions drawn based on that interpretation.

Even in the absence of competing influences, uncertainty in the linearity of the sodium-as-sea-ice-extent interpretation poses challenges. Dose a change in the sea ice edge (or extent) of a given size have the same impact on Na in Greenland if the sea ice edge is at 55 degrees North (just for example) versus if the edge is at 65 degrees North? If relationship between changes Greenland Na and the sea ice absolute position is (sufficiently) nonlinear then the changes in sea ice at the start of DO events may not be as detectable in Greenland Na as changes later in the event. This could lead to apparent lags of the Na signal to Ca, even if the change in sea ice initiates at the same time as the change in whatever drives Ca. I'm not sure I think this is particularly likely, but it is certainly plausible. Further, the impact of sea ice on climate isn't linear. Again a given size change in the sea ice edge when the edge is at 55 degrees North (for example) doesn't have the same impact on the surface radiation budget nor the

buoyancy forcing of the overturning circulation as when the edge is at 65 degrees North. The relative timing of sodium changes in Greenland don't necessarily rule out sea ice changes as a potential "trigger" of DO events, if climatically meaningful sea ice changes can happen without influencing Greenland Na.

To be sure, there are innumerable, potentially ad hoc, explanations for the data, and it is not the authors responsibility to come up with and then weigh the merits of all such explanations. However the assertion that the lag in sodium seen in Greenland necessitates a lag in changes in sea ice extent with respect to shifts in atmospheric circulation, and that this in turn rules out sea ice changes as the trigger for DO events, is somewhat bold. That this "provides an essential benchmark for climate models" is bolder still. Both of these statements to my mind somewhat outpace the robustness in the interpretation of Na as a qualitative indicator of sea ice extent. I think some slight tempering of the conclusions is merited here and perhaps some discussion of the limitations to the interpretation, or much more compelling evidence is needed that a change in sea ice extent must be seen in the Greenland Na records regardless of other processes. To be clear, I do think the difference in timing of the aerosol species identified here is a compelling target for modeling. And I do think the climatic interpretation offered by the authors is plausible, and one to be taken seriously. It is just not clear to me that these data alone place that strong of constraints on the timing of sea ice changes.

Minor points: Page 1, line 8: The authors say "from one of the cores".... Which one?

Page 1 line 17: I think that the clause "In the course of the last glacial period" should be moved after the 2nd comma in this line (after "warming episodes,"). This is a possibly silly language thing but: the ice core records reveal that the events were in the last glacial period. It wasn't during the last glacial period that the ice core records revealed these events (that was in the 1980s!).

Page 1 line 20: This is a language choice the authors should feel free to ignore but

"..going along with an almost doubling..." sounds strange to my ear, though I can't tell why. "...coincident with a near-doubling..." sounds better to me.

Page 2 line 3: I'd move the "Also" at the start of the sentence to after "Northern Hemisphere".

Page 2 Lines 20-35: This is a very nice description of several related though distinct ideas. Thanks!

Page 3 lines 15-20: Are the Ca and Na records corrected for the sea-salt vs non sea-salt components of each (e.g. using average Na/Ca mass ratios in average crust and sea salt, as is common in Antarctic records)? I ask because having looked into this once briefly, it seemed like the corrections often used in Antarctica lead to nonsensical results in Greenland, which was disturbing, and I wasn't sure why.

Page 6, Line 9: I think there is a missing "of" between "interpretation" and "phase".

Page 6, lines 20-21: This is interesting! Nice.

Page 6, lines 27: "on" should be "one" I think.

Page 6, lines 29: If you don't make the assumption that the DO-events have the same imprint in both cores, how would that effect any of your conclusions?

Page 10, lines 25-30: Might be worth citing Fudge et al 2016 here, they show a strong divergence of accumulation rate and d18O in ice cores at millennial time scales.

Page 11, lines 3-5: This is nice, convincing analysis.

Page 11, lines 11: I think this should be "...a coinciding..." Not "...an coinciding..."

Page 11, lines 21-22: How big are the additional changes in source need compared to the total variability across a DO event?

Page 11, lines 24-35: This is really nice discussion.

Refs cited: Markle, Bradley R., et al. "Concomitant variability in high-latitude aerosols,

water isotopes and the hydrologic cycle." Nature Geoscience (2018): 1.

Jones, Tyler R., et al. "Southern Hemisphere climate variability forced by Northern Hemisphere ice-sheet topography." Nature 554.7692 (2018): 351.

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Fudge, T. J., et al. "Variable relationship between accumulation and temperature in West Antarctica for the past 31,000 years." Geophysical Research Letters 43.8 (2016): 3795-3803.

Gkinis, Vasileios, et al. "Water isotope diffusion rates from the NorthGRIP ice core for the last 16,000 years–Glaciological and paleoclimatic implications." Earth and Planetary Science Letters 405 (2014): 132-141.

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