Climate of the Past Discussions

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Review of Lohmann and Svenssonmment

Anonymous Referee #4

Referee comment on "Ice core evidence for major volcanic eruptions at the onset of Dansgaard–Oeschger warming events" by Johannes Lohmann and Anders Svensson, Clim. Past Discuss., https://doi.org/10.5194/cp-2022-1-RC4, 2022

Lohmann and Svensson present a statistical analysis which indicates that bipolar eruptions are more like to precede Dansgaard-Oeschger warmings than chance would suggest. There does not appear to be an increase in volcanic activity preceding DO coolings. They use a simple climate model to suggest that bipolar eruptions in the NH are able to induce DO warming due to cooling in the North Atlantic. The manuscript is well written and organized. The ideas are interesting and highlight the importance of a bipolar volcanic record to interrogate abrupt climate change.

The manuscript is an interesting analysis that works to better understand the abrupt DO events. The conclusions rely upon the bipolar matches of Svensson et al. (2020) and the magnitude and location of the eruptions based on Lin et al. (2022). I have two primary topics that I think could use elaboration in the manuscript:

1) Potential biases in the timing of identified bipolar matches. The bipolar matches in Svensson et al. (2020) represent a considerable effort, but it’s worth remembering how uncertain the matches are; there are few definitive linkages between volcanic events - such as tephra or even sulfate isotopes that indicate stratospheric eruptions. Which is not to say they should not be used in the manner in this manuscript, but considerable caution should be applied. Because the matched volcanic events rely upon pattern matching sulfate peaks with a similar number of annual layers between, the identification of matches may be biased to when the timescales are already well synchronized – the abrupt DO events. It would be good to include a discussion of how many volcanic events fall in the 50 years after a DO-warming.

Another issue for this work is whether the identified volcanic matches are accurate. As the recent GICC revision (GICC21 for the past 3.5ka, Sinnl et al., 2022) shows, mis-identification is not a trivial problem. The paper addresses well whether bipolar eruptions are underestimated overall; but it does not address the number of misidentifications. This seems like the largest uncertainty to me. There is a volcanic eruption identified every 50 years, most of which do not reach both poles. So there are likely to be a significant number of instances where there was an eruption in both the NH and SH within a few years of each other that could be mis-identified as a bipolar eruption (accuracy of annual layer interpretation in the glacial for both GICC05 and WD2014 is probably close to 10% on shorter intervals, which greatly increases the number of events that could be considered coincident).

2) Magnitude of identified events at the DO warmings. I looked up the 7 volcanic events at the DO warmings in Table 2 of Lin et al. 2022. I was surprised that only 3 of these were in the top 45 largest magnitude, and only one was a Northern Hemisphere eruption. But maybe most surprising that two of warming with volcanic events had much larger volcanic events that preceded them by decades to a century (14761 was the 21st largest and 38366 was the 39th largest). In both cases, the stadials were already long and stable, which raises the question of why the larger events did not trigger a DO warming? I also looked quickly at the largest event (55383) which occurs during a time of "flickering", suggesting the climate was susceptible to external forcing. Yet it did not produce a DO warming (possibly a cooling?). The manuscript would benefit from providing more context on the magnitude of the identified volcanic forcing and how it compares to other volcanic forcing that preceded, but did not trigger, a DO warming.

I am late getting this review in, so I have not reviewed the statistically analysis extensively. But I did not notice any objective limitations. As described above, my primary concern is the possible subjective errors in the matching to establish the bipolar volcanic events.

Minor comments:

- Figure 1. I like this figure.

- Figure 2. I would like this figure to include the volcanic events that occur after the DO warming. This could illustrate the volcanic events are more like to precede the climate transition than to follow it, which would support the inference of causality.
- It seems like some of the information taken from Lin et al. needs updating, likely due to changes in the review process for that manuscript.
- The climate model used seems too simplistic. I get that it is a toy model to show plausibility, but more justification for why the model has the important components to address this issue would be helpful. There are only 3 references in section 2.4. Maybe the benchmarking of the model occurs in Lohmann and Ditlevsen, 2021, but if so, some of the relevant content should be repeated.
- And a final note on authorship. Given the reliance on the data set of Lin et al., which was only recently accepted for publication (the Ides of March if I remember right), it seems like adding authors would be warranted.