

Clim. Past Discuss., referee comment RC1
<https://doi.org/10.5194/cp-2022-1-RC1>, 2022
© Author(s) 2022. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on cp-2022-1

Anonymous Referee #1

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-RC1>, 2022

The manuscript uses a new record of bipolar volcanism derived from Greenland and Antarctic ice cores combined with climate records from the same cores to argue that bipolar eruptions occur more frequently prior to the onset of a DO warming cycle than expected by chance. They do not find a similar relationship prior to the onset of the cooling phase. They suggest that this asymmetric response to volcanic eruptions is down to the effects of cooling on AMOC, and support this with a global ocean model.

The study is welcome, and I believe that the approach of the authors has great potential. However, I found the text very difficult to follow in places, and consequently many of the techniques are unclear. I also have some questions about the statistics. A possible issue might be that it seems to me (apologies if I have missed this – if I have, it needs to be made much clearer) that all identified bipolar eruptions were treated equally rather than separated out by hemisphere. Many studies now show that the latitude of an eruption greatly affects the nature of the response, so this really needs to be considered. Lin et al 2021 has published the estimated latitudinal band of eruptions across the interval 60-9 ka, so this should be easily done. I think that this is particularly problematic in terms of the conclusion that stadial events are not triggered by volcanism; this really should look only at NH eruptions. Also, it seemed to me that all stadial onsets were used in the statistics, regardless of the type, rather than just abrupt stadial onsets. These points are elaborated on in the comments below.

The manuscript states that there was one bipolar eruption about every 500 years. A key paper that needs to be discussed is Rougier et al., 2018 *EPSL*, where they calculate probable return periods for eruptions of different magnitudes. Rougier et al estimate that there was a M6 eruption every 110 years, and an M7 eruption every 1,200 years; both magnitudes would be sufficient to have a bipolar expression (e.g., the M6 1991 Pinatubo eruption was not particularly large, but did result in SH S deposition, Cole-Dai et al., 1999 and others). This should be discussed in the manuscript, as the return period for M6 bipolar eruptions is far shorter than 500 years. The authors do appeal to ice thinning and a higher background of impurities, but even if the eruptions cannot be detected for these reasons, they still presumably happened, and not all of these will be M6 eruptions; some

high latitude M7 eruptions may be missing from the opposite hemisphere if the authors' contention of ice thinning is correct.

The article overall is written somewhat awkwardly and is difficult to follow in places. Overall, the text could be simplified and shortened considerably.

General comments:

P1, L1-2: Vague as written – across what timescales? It is fairly well understood now over the past 2,000 years or so, so please be clear what timescale you are referring to.

P1, L2: The statement regarding a statistical assessment being hampered is incorrect – Baldini et al and Bay et al are both statistical assessments. Perhaps rephrase to include reference to make reference to the particular issue here: that it is difficult to work out the magnitude from S concentrations in one ice core alone, and difficult to correlate individual spikes across Greenland and Antarctica. I think that the 'statistical assessment' is meant to refer to this.

Page 1, 17: 'Greenhouse' does not need to be capitalised.

Page 3, line 18: I believe that this submission is a heavily modified version of a previous unsuccessful submission to *Climate of the Past*. I note that one of the improvements is the inclusion of the Lin et al 2021 dataset of eruption magnitude.

Page 3, line 18: 'volcanogenic sulphur deposition' rather than 'volcanic depositions'

Page 3, line 8-28: this seems like too much summary and interpretation for the introduction. A short 'here we address this' or 'here we do this to show that' is fine, but two long paragraphs with interpretation is too many for the end of the intro.

Page 5, L25-30: Can a comparison with the absolute Corrick et al (2020) dates help say something about accuracy? Noting of course that here the interpretations are based on proxies from the same cores (so absolute timing is less important).

Page 5, L29-30: unclear what is meant here. Perhaps rephrase to (what I think is the intended meaning): 'It is therefore possible to assess the climate repercussions of volcanic eruptions to a decadal-scale.'

Section 2.3: p6, L1-18: This discussion is welcome, and represents a key change from the previous submission. I think that it should be made clear though that many of the other sulphate peaks are probably not just noise, but recording smaller regional eruptions, or even bipolar eruptions that are not conclusively matched to the other hemisphere.

P7, L1-3: This conflicts with the findings of Zielinski et al., 1997 JGR, who found that eruption frequency increased during deglaciations, possibly due to crustal stresses. This should be mentioned and reasons for any differences discussed.

Section 2.4: The model simulation is okay to include, and there appear to be adequate caveats in this section regarding how comprehensive it is. It is still meaningful to show that volcanism can trigger oceanic changes in at least one model. But given the limitations of the model (see below) more caveats in the abstract/conclusions are probably warranted.

P7, L27: How realistic is the use of present-day ERA-40 wind stress forcing? I believe that the re-analysis data extends from 1957 through August 2002, during the anthropogenic greenhouse era; surely the winds during stadials would be considerably different?

P7, L33: That the model does not including a sea ice or atmospheric component could be a major issue, because there is a good possibility that sea ice plays a major role in any positive feedback mechanism. This might not be a 'fast' amplifying feedback, but could potentially affect climate through the duration of the event. Again, I have no major issue with the model being included, but it needs to be adequately caveated.

P8, L30: again, when saying "We have estimated the precise times of the DO warming onsets...." it is well worth comparing with the Corrick et al onsets, to briefly compare the accuracy of the derivations here.

Page 9, L16: define which 'this study' means. Is it Lin et al., as mentioned in the last sentence, or this current study?

Page 10, L1-3: Why choose the 5 largest of the last 2,500 years? If the 10 largest were chosen, then the observed frequency would be 250 years instead of 500. What is the justification for choosing the largest 5?

Section 3.2: This section really needs to reference and utilise the information available in Rougier et al., 2018, EPSL, where they provide estimates of the recurrence of eruptions of different magnitudes.

Section 3.6.: I agree that the drops back into stadial events are much less well defined as the onset of rapid warming. However, I disagree with how these are handled in this manuscript. Lohmann and Ditlevsen (2019) identified the ends of interstadials, and it is these data that are used here. However, (as far as I understand) in this present manuscript all these interstadial dates are used, regardless of whether or not it was a sudden transition. It is clear that the trajectory and duration of the warm phase of many DO events is predictable based on linear extrapolation, and therefore the ends of these particular events should not be considered as 'events' in the calculations. Rather, only the sudden drops in some events (such as in DO-20 and 19.2) that deviate from the predicted linear trend could have been caused by volcanism. This could affect the statistics. I would suggest either only including DO events ending with sudden drops, or not considering the ends of DO events at all, and only focussing on their initiation.

P19, L19: The sentence here makes it seem that the authors of this manuscripts are the first to detect a volcanic influence on DO event onset, whereas both Baldini et al and Bay et al also did. I would rephrase. One suggestion is:

"Thus, we conclude that there is a likely influence of large volcanic eruptions on the occurrence of some DO warming transitions, consistent with the results of previous studies (Bay et al., 2004, 2006; Baldini et al., 2015), but do not find evidence for a similar statistical relationship of eruptions preceding the abrupt DO cooling transitions." However – note that the statistics concerning the onset of the cooling phase could be incorrect as outlined above, and therefore the last part of the sentence above may need to be deleted in a revised submission.

Additionally, both Baldini et al and Bay et al looked at hemisphere-specific eruptions, whereas this manuscript does not (apologies if I have missed this – if it is there it needs to be much more clearly stated). Many recent papers covering the more recent past note a different response between NH and SH eruptions, so that this really needs to be considered. For example, Zhuo et al 2021 (Atmos. Chem. Phys.) use two groups of ensemble simulations to show how NH, equatorial, and SH eruptions trigger very different climate responses, including ITCZ migration away from the hemisphere of the eruption in the case of high latitude eruptions. Sun et al 2019 (J. of Clim) argue that NH high latitude eruptions could affect ENSO state, providing another example of why latitude and hemisphere are important.

Specifically, it may be that NH eruptions trigger abrupt stadial onsets, and that because the statistics here consider all eruptions (NH or SH), this link was missed. There are few lines from 23-29 that mention hemispheric asymmetry, but it needs to be clearer if the statistics do take this into account, and, if not, then either it does need to be considered or the section about stadials removed. Lin et al 2021 do divide out the eruptions according to estimated latitudinal band, so perhaps this information could be used.