

Clim. Past Discuss., author comment AC1
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Reply on RC1

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Author 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-AC1>, 2022

Below are our responses to the issues raised by the referee (*italic*).

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.

We thank the referee for these points, and our detailed answers follow the more specific points by the referee below. In short, the latitudinal estimates of Lin et al are not precise enough to base our objective analysis on them. We do not believe it would be a wise choice of methodology to restrict our analysis to a very uncertain subset of eruptions based on a preconceived idea of a physical mechanism for the potential volcanic trigger of DO events, as the referee suggests.

Regarding the stadial onsets, it does not change the results whether only truly abrupt events are considered. There are simply not enough eruptions occurring shortly before these stadial onsets to give significant results, regardless how small a subset of stadial onsets one chooses.

Nevertheless, these points will be discussed in the revised manuscript, and explained in detail 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.

We are happy to mention and discuss the return times for M6 and M7 eruptions obtained in Rougier et al, and will do so in the revised manuscript. Note however that these return times are based on geological evidence of the erupted mass, which is not directly calibrated to the sulfur deposition estimate that is used in our work. To make direct comparisons, one has to rely on individual known eruptions that are in both data sets. This would again be eruptions like Tambora or Samalas that we already compare our data set to (the latter in the revised manuscript), albeit in the context of Sigl et al 2015.

Our data is largely consistent with the return period of these eruptions, which however have large uncertainties both in Sigl et al and Rougier et al, due to the very short time periods investigated. Note here that while Rougier et al do consider data spanning 100 kyr, their estimates for the occurrence rates of eruptions with magnitude $M < 7.5$ are based only on the last 2 kyr.

Referring again to Sigl et al 2015, we already mention in the manuscript that there are indeed many more eruptions with a bipolar signature that are however smaller than what can be detected in the glacial period. This leaves us (Svensson et al 2020) naturally with a data set containing bipolar eruptions of larger magnitude. The smaller, undetected eruptions with a bipolar signature are not relevant to our analysis, since a) they are less likely to have global climatic impact, and b) they would need to be assumed a priori to occur both in the vicinity of DO onsets and elsewhere in time. In other words, our statistical framework does not depend on the knowledge of these events.

The fact that some larger eruptions (e.g. high latitude M7 eruptions) could be missing in our data set is covered and quantified extensively in the manuscript, where we test the robustness of the results against a scenario where more than 50% of the relevant eruptions would be missing.

Finally, note that we strongly prefer to compare our data to Sigl et al, since the Rougier data is incomplete in that it only covers eruptions of known source. From the data in Sigl et al we can see that 12 out of the 25 largest eruptions (as detected in ice cores) of the last 2,500 years are in fact of unknown source.

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

We are happy to hear specific suggestions. For the time being we will try to improve the clarity of the text and try to make it more concise.

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.

We are referring to pretty much all timescales actually, since volcanism has been suggested as a driver of the climate in many different climatic periods. We will write this more precisely, also in relation to the following point.

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.

We will write more specifically what we think is required for a "statistical assessment", as explained in the following: We do not mean that statistical assessments have not been conducted, but that they were challenged by poor data quality/very large temporal uncertainties (several centuries) that made it difficult to actually argue for a causal relationship of eruptions and climate. In previous work one simply did not know whether an eruption occurred before or after an abrupt climate transition. Further, as the reviewer correctly states, relying on deposition estimates from one of the poles potentially introduces many local eruptions of limited global impact into the data set.

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

Ok.

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.

Indeed.

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

Ok.

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.

Ok, will rewrite.

Page 5, L25-30: Can a comparison with the absolute Corrck 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).

As the reviewer states, the absolute ages are not relevant to the analysis, and systematic offsets with respect to absolutely dated archives are not the subject of this study. Since these offsets are much larger than a) the relative age uncertainty of single onsets across cores, and b) the time windows analyzed for the co-occurrence of volcanic eruptions, we do not think that a reference to Corrck et al (2020) is of any help. The precision of our onset estimates cannot be judged because the uncertainties in the Corrck et al ages are 1-3 centuries. Further, we are estimating the onsets of the transitions, and not the midpoints as in Corrck et al.

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.'

Indeed, this is what we mean. Will rephrase.

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.

Sure, we will write this explicitly.

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.

We agree that this is worth discussing in some detail. Still, our statements do not contradict the findings. In the revised manuscript, we will clarify the following points, adding to Section 2.3.

First, Zielinski et al do not actually show that the increased volcanic activity during the deglaciation is significant, and this would also be difficult for them given the poor data quality at the time. Second, we are specifically mentioning and showing that the frequency is indeed elevated. A more detailed analysis can also be found in Lin et al. 2022 (Fig. 3+4), where it is seen that the increase in eruptive activity seems to mostly concern increases in the magnitude of large eruptions as detected in Greenland.

Nevertheless, restricting ourselves to the bipolar dataset from Svensson et al 2020, our analysis shows that in the bigger scheme of the glacial period the deglacial increase of eruptions does not appear as statistically significant, even though it may well be a genuine feature. Given that it is not significant there is no sound basis for us to include this in our null model. Apart from this, since a) it is only a very short segment of the whole time period, and b) we already test our results against severe undercounting of events, this does not influence our results.

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.

Ok, will try to make it more clear in the abstract and conclusions.

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?

Indeed the boundary conditions are present-day and not realistic for the last glacial. We emphasized the wind stress here and wrote "realistic" because it was one of the changes to the model configuration in Lohmann and Ditlevsen 2021. We will drop the word "realistic".

Note that within this class of models no "realistic" stadial boundary conditions exist unfortunately, since the stadial state of the atmosphere is not well-constrained and very few coupled ocean-atmosphere models can simulate a stadial climate. As a result, the stability of the AMOC is very much unconstrained during the last glacial (and in fact also in present-day).

In our hypothesis we assume that an instability of the AMOC (and corresponding stable states analogous to stadials and interstadials) exists, and achieve this instability by changing the freshwater boundary conditions. We are aware that changes in the wind stress would again change the AMOC stability. We will write this more explicitly in the revised manuscript (Sec. 2.4).

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.

We agree that sea ice and atmospheric components have the potential to change the mechanism. But we find it hard to speculate how e.g. sea ice will affect the dynamics without actually modeling it. This will for example depend on how large the actual sea ice extent is during the different stadials. We will write in the revised manuscript (Sec. 2.4 and Discussion) that our proposed mechanism is contingent on the sea ice and atmospheric response to not destroy the dense water formation in the North Atlantic.

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.

See response further above. The relative timing of events is precise in our study because we are working on records from well synchronized ice cores. But the absolute ages are inaccurate because the dating is based on annual layer counting. Because it is the relative timing of volcanic eruptions and climate transitions that matters for our study, high accuracy is however not crucial.

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

We mean Lin et al, will state explicitly.

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?

We choose the 5 largest events because $2500/5 = 500$ years, and 500 years is the return period of the events in our data set. The magnitude of the 5 largest events of the last 2,500 years matches the magnitudes of the bipolar glacial data set in the sense that most eruptions in bipolar data set fall in the same magnitude category (larger than Tambora). Note that this is only a qualitative comparison to illustrate that the frequency and the estimate magnitudes of the bipolar dataset are consistent with what we would expect based on datasets of more well-known and well-studied eruptions. Talking for instance about the 10 largest events with a return period of 250 years would be less relevant, since we specifically want to make a statement about 1-in-500 year eruptions.

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.

See response further above. Again, the Rougier et al. study is based on geological evidence of known eruptions that is likely to miss out events at an increasing rate the further back in time one goes.

In fact, even in the period of the last 2.5 kyr, the data (of the largest eruptions) analyzed

by Rougier et al is likely sparse. In the 2015 ice core study by Sigl et al, it is found that half of the largest 25 eruptions are of unknown source, and thus unlikely to be found in the data sets of Rougier et al. Accordingly, the estimated return periods of prominent eruptions such as Tambora are significantly larger in Rougier et al compared to what can be inferred from Sigl et al. Thus, we believe the ice core record allows for a more consistent detection of large volcanic eruptions over the investigated period.

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.

We understand the referee's concern that the missed statistical link of terminations and eruptions may be due to the too large number of terminations that are not abrupt and thus not good candidates for a volcanic trigger.

However, first of all, we argue that in principle all events are candidates for a volcanic trigger, regardless of whether they can be predicted approximately from linear trends. In Lohmann/Ditlevsen 2019 Clim Past, we show that the linear oxygen isotope trends at the beginning of the interstadials can be considered a good predictor of the eventual stadial onset. In Lohmann 2019 (GRL) the same is shown more rigorously for the stadials (i.e. interstadial onsets), using dust records. But the predictions of the events are only approximate and thus leave room for a short-term volcanic trigger, regardless of whether the rough timing of the event may be set from the beginning by some other processes. This is discussed in detail in the manuscript, and it is the basis of our interpretation of the results, i.e., why there can be a volcanic trigger of (only) some events even though they are predictable in principle.

Regarding the stadial onsets, we do agree that it is a priori more obvious to look for a potential trigger in the "well-defined" or abrupt onsets. And indeed one might argue that there are quite a few stadial onsets that are simply not well-defined enough to even look for a potential trigger, at least when considering the oxygen isotopes alone (as in Lohmann/Ditlevsen 2019). It is not trivial how to best define objectively which events are abrupt and which are not. A procedure is proposed in Lohmann/Ditlevsen 2019, which yields that 10 out of the 19 terminations regarded in the present paper are not abrupt.

Regardless of whether the referee agrees with all that was said previously, the fact that we do not see a significant relationship of terminations and eruptions is not because we

are using too many terminations that are no good candidates for a volcanic trigger:

As can be seen in Fig. S5, there is really only one interstadial where a volcanic eruption happens within 100 years prior to the interstadial termination.

So this is clearly not significant, even if the number of admissible events is reduced to half or even less.

In fact, if we were to only use the events that were identified as abrupt in Lohmann/Ditlevsen 2019, there would be no termination where the closest preceding eruption occurs with 170 years prior. Thus, we will not be able to find any subset of DO events where a significant link of terminations and eruptions exists, no matter how much cherry-picking we allow.

We do not claim that this is a proof for the absence of such a link. Better data may show otherwise. Besides unidentified bipolar eruptions, the most obvious candidate for a "missed" statistical link of interstadial terminations and eruptions is the large uncertainty in the timing of the terminations, as discussed in the main text. Specifically, some of the interstadial terminations may be identified too early with our method. We'll discuss all of this explicitly in the revised manuscript (Sec. 3.6).

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.

We will rephrase to make it less ambiguous. There are both agreements and disagreements to the two previous studies, which we will state more explicitly. Regarding the stadial onsets, see the previous point.

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.

We agree that latitude is important for the climate response and will include the Zhuo et al 2021 and Sun et al 2019 references in the revised paper to further emphasize this point (Sec. 3.7 and Discussion), expanding what is already discussed and investigated with our model simulations.

Indeed, Lin et al give a binary classification (below 40degN or Southern Hemisphere, LLSH; versus Northern Hemisphere High Latitude, NHHL) of the eruption latitude based on the relative Greenland-Antarctic deposition. We do not believe, however, that this data is informative and reliable enough to incorporate it into our statistical analysis, or even use it as the main data of the analysis.

We will however discuss it in the revised manuscript (Sec. 3.7 and Discussion), in relation to our proposed mechanism of the volcanic trigger, giving the following details:

There are 5 (LLSH) vs 2 (NHHL) eruptions within 50 years before a DO onset, and 4 (LLSH) vs 1 (NHHL) eruptions within 20 years. This is out of 34 (LLSH) vs 48 (NHHL) total bipolar eruptions.

While the sample size of eruptions before DO onsets is very small, it still indicates that eruptions classified as LLSH are more likely to cause a DO warming. We understand this could be taken as evidence that our hypothesis of a North Atlantic (NA) cooling to trigger the DO onsets is not so likely.

However, the climatic impact and its latitudinal footprint is still very much unconstrained, since the classification of Lin et al 2022 only tells us whether there has been relatively more deposition of sulfur in Greenland versus Antarctica.

The first obvious impediment is the large uncertainty in the deposition estimates of individual eruptions, due to inter-core variability and other factors. But even if this uncertainty was zero, there is still a massive uncertainty in the climatic impact, because we do not know where most of the sulfur aerosols actually were transported (troposphere or stratosphere) and how long they were present in the atmosphere. So there is the possibility that an eruption with a large sulfur deposition in Greenland compared to

Antarctica still produced less NA cooling than a tropical eruption classified as LLSH.

Further, the latitudinal estimate "LLSH" includes NH eruptions up to 40N. So based on this classification we do not have a clear separation of eruptions with and without a NA climate impact.

Note that even tropical eruptions tend to have a hemispherically asymmetric cooling towards the NH, as discussed in the manuscript.

All in all, the latitude classification by Lin et al 2022 does not warrant us to focus the analysis on either latitudinal subset. Even more so because there is no other evidence for our NA cooling hypothesis that would warrant us to have the hypothesis inform the data analysis.

We will, however, include a detailed discussion on this in the manuscript (adding to Sec. 3.7 and the Discussion), so the reader can decide to what degree this is in conflict with our physical hypothesis.