

Clim. Past Discuss., author comment AC4
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Reply on RC4

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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-AC4>, 2022

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

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.

We thank the referee for this suggestion and will include this in the manuscript (Sec. 3.1 and new panel in Fig.2). In essence, there is no DO warming where an eruption occurs within 25 years after the onset, and 2 DO warmings where this is the case within 50 years. Under the null hypothesis we would expect this to happen by chance for 1 event within 25 years and 2 events within 50 years. This is opposed to 5 events with eruptions within 20 years and 7 events with eruptions within 50 years when looking before the onset. Because of this and other reasons already stated in the manuscript, we are fairly confident that the increased frequency of eruptions before the onsets is not an artifact of the better prior matching of the records close to the DO transitions.

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).

It is correct that the problem of misidentified bipolar eruptions is not addressed in the manuscript. This is because a) it is very hard to quantify this in a rigorous way, and b) we nevertheless believe the number of potentially faulty matches is relatively low, as detailed below.

The probability of a bipolar match occurring just by chance is hard to estimate, because the bipolar matching is a quite complicated procedure, relying on information from many different records, and it was not done in the exact same way for all eruptions. Still, our analysis of the data in Lin 2022 may be used to give a rough estimate of how likely it would be to find bipolar matches by chance. Since we have estimates for the magnitudes from Lin 2022, we can see that while as you say there are eruptions identified every 50 years at the individual poles, eruptions with a deposition magnitude corresponding to the bipolar ones in Svensson 2020 only occur roughly every 250 years, as discussed in the paper.

Now for a bipolar match one would actually need at least two consecutive eruptions that coincide in Greenland and Antarctica within a given age tolerance, and which are above this magnitude threshold.

This greatly reduces the likelihood of coinciding eruptions, as follows:

Assuming the simplest of such patterns (2 consecutive eruptions), the first step is to find an eruption in Greenland and Antarctica that lies within the uncertainty of the prior methane or ^{10}Be matching, which is around 100 years around the time of the DO events (see Fig. S16 in Svensson 2020). From the Poisson process model, this gives a probability of a misidentification (i.e. an eruption coinciding in Greenland and Antarctica within the uncertainty window just by chance) of $P_1 = 1 - \exp(-100/250) = 0.33$.

In the second step, another eruption is identified at both poles, and it is checked by layer counting whether the number of years elapsed from the first eruption is almost equal in both poles. Here "almost equal" means "within the relative layer counting uncertainty", which as you say could be estimated by 10% of the counted interval.

Assuming this second eruption is spaced in one of the poles by exactly the expectation value 250 years (of course more rigorously this spacing is a random variable with exponential distribution), the probability of finding the second eruption in the other pole within the window admissible by the counting uncertainty is: $P_2 = 1 - \exp(-0.1 \cdot 250/250) = 1 - \exp(-0.1) = 0.095$.

These probabilities have to be multiplied to give an estimate of the probability of a misidentified doublet: $P = P_1 \cdot P_2 = 0.03$, or 3%. For a total of $N=82$ eruptions (should be strictly less, since the eruptions are identified in patterns and not individually), this would give an expected value of 2.58 misidentified eruptions.

Note that even if one would for some reason doubt our estimated recurrence time of candidate eruptions of 250 years, and use a smaller time instead, the overall probability does not become higher than $P = 1 - \exp(-0.1) = 0.095$ or 9.5%, yielding a maximum expected value of 7.8 misidentifications. This is because, while P_1 will approach 1, P_2 remains unchanged (since the counting uncertainty is proportional to the time in between

eruptions). Note that for patterns longer than 2 eruptions (as was the norm in Svensson 2020) these probabilities will become much smaller.

We don't claim this is in any way a precise estimate of the actual probability of false bipolar matches in our data set. But we just wanted to give some arguments as to why we believe misidentified patterns of eruptions are much less common than one might think at first, and are most likely rare enough so that they will not influence our analysis and results.

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.

Indeed, when looking into single events, like the ones suggested by the referee, one may wonder why a DO event was not triggered earlier by another event. One way to interpret why there are some larger eruptions that do not trigger a DO onset, while smaller eruptions shortly after do, is that the larger eruption already partially destabilized the system, and thus a smaller eruption was sufficient to trigger the transition. Of course, this is completely speculative, but it serves to illustrate that there is no fundamental dilemma here.

Apart from this, we find it difficult to assess the significance of eruptions that did not trigger DO events in a meaningful, quantitative way. There are obviously many more "non-triggering" eruptions, so one would have to find an objective criterion of an eruption that "should" have been a trigger. We don't see an obvious suitable statistical framework to devise here, but we are happy to receive suggestions.

Since the philosophy of the work is a robust statistical analysis, we would like to refrain from pointing towards individual eruptions in order to make any claims. Not least because it is highly uncertain whether individual eruptions, such as the ones you mention, are really larger compared to others, due to the large uncertainties in the deposition estimates from Lin 2022 and even more so in the actual climatic impact on the relevant parts of the climate system, which is fully unconstrained.

Regarding the magnitude ranks you mentioned, we show in the manuscript that the estimated deposition of the eruptions before DO events is slightly smaller on average compared to the whole bipolar population, but not significantly so. It may be that you are surprised to only find 3 out of 7 eruptions in the top 45, but assuming that the 7 eruptions are an unbiased random sample from the N=82 eruptions the two most likely outcomes are that you will find 3 or 4 eruptions larger than the 45th largest eruption, since eruption number 45 is very close to the median of the sample. Further, an eventual bias towards too small eruptions is already taken into account when testing for the robustness of our results to undercounting.

Regarding your observation of only one eruption being classified as "Northern Hemisphere":

The data set of Lin et al indeed gives 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 to warrant a specific re-interpretation of our results. We will however discuss it in the revised manuscript (Sec. 3.7 and Discussion), also 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. 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 real 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. But there is a large uncertainty in the deposition estimates of individual eruptions, due to inter-core variability and other factors. 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.

Finally, note that 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.

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

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.

We agree and will add a panel b) where the same is shown for eruptions occurring after the DO warming onsets. This will make it visually very clear that there is an obvious asymmetry in between eruptions occurring before and after the onsets, which indeed suggests a causation from eruptions to DO warmings.

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.

Indeed some minor details have changed since our submission, mostly in the Antarctic deposition estimates. The analysis is now updated according to the published dataset, and

all figures updated.

The results are unchanged, and specifically the very small changes in the deposition estimates had no discernible influence on the upper bound of the eruption occurrence rate.

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.

First, we would like to mention that the model used is a fully fledged ocean-only GCM and not a toy model. In the paper we do acknowledge that it is coarse resolution and uses present-day boundary conditions. We do not claim it is a realistic model that can explain DO events, and the purpose of our manuscript is not to provide a full explanation of how volcanic eruptions would influence DO events. The main purpose is to show that a statistical association exists in between volcanic eruptions and some rapid DO warmings, by means of an analysis of the ice core data that does not depend on any physical hypothesis or modeling. We then provide an admittedly speculative explanation of why the eruptions may preferably trigger DO warmings and not coolings, which is made plausible by model simulations.

The only really important component that the model needs to fulfill (besides being a three-dimensional ocean model with global bathymetry) to show the plausibility of our hypothesis is to have a tipping point of the AMOC, along a regime of bi-stability. For the latter, one needs to have an ensemble of equilibrium simulations, going beyond the usual hysteresis experiments.

There are actually not many models of higher complexity that would fulfill these criteria. Since, as we state in the manuscript, the model does not have an active sea ice or atmospheric component, we cannot assess how sea ice or atmospheric dynamics may alter the plausibility of our mechanism. The mechanism is thus contingent on the sea ice and atmospheric response to not destroy the bi-stability of the AMOC and the dense water formation in the North Atlantic as a volcanic trigger of an AMOC transition. This will need to be tested in fully coupled models that actually have a reasonable representation of DO-type dynamics, and such studies are actually underway. We will make these points more visible in Sec.'s 2.4 and 3.7 of the revised manuscript, and repeat some more crucial information on the model from Lohmann and Ditlevsen 2021.

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.

We agree that the data from Lin et al is important for our robustness tests, but since it is published we prefer to only include authors that directly contributed to this work. We are collaborating with key authors from Lin et al on follow-up studies.