Climate of the Past Discussions

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# Comment on cp-2022-1

Anonymous Referee #3

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-RC3, 2022

**Review of "Ice core evidence for major volcanic eruptions at the onset of Dansgaard-Oeschger warming events" by Johannes Lohmann and Anders Svensson**

Lohmann and Svensson (LS22) investigate whether volcanic activity may play a role in initiating abrupt Dansgaard-Oeschger climate variability. They suggest that the probability of finding a large bipolar volcanic event 20 or 50 years prior to DO onset exceeds the null hypothesis (Poisson distribution) for abrupt DO warming events (but not DO cooling). They conclude that volcanism may play a role in initiating DO events. They provide simple ocean model simulations to lend support to their hypothesis.

Overall the study is interesting, and the paper is well written. However, the methodology suffers from some severe flaws that may be hard to overcome for the authors. The validity of the statistical methods relies on two key points:

- The bipolar volcanic catalogue by Svensson et al. (2020) is complete, and
- The choice of Null hypothesis (1 eruption per 500 years) is correct

I am not convinced that the authors have demonstrated either.

(1) the authors rely on the bipolar events identified by Svensson et al. (2020) using annual layer counting. The goal of that study was not to make an unbiased and complete catalogue of all bipolar events, but rather to to investigate the phasing of bipolar climate change at great precision. Svensson et al (2020) write: "The bipolar layer counting is not continuous but is focused on periods of abrupt climate variability or high volcanic activity."

This implies that the Svensson et al. (2020) catalogue is biased toward periods of abrupt climate change. This is critically important. LS22 demonstrate convincingly that there are more events in the vicinity of the abrupt events, however does this reflect the DO triggering mechanism proposed, or simply a bias in the volcanic catalogue towards periods of abrupt climate change that were investigated in more detail (as suggested by Svensson 2020)?

It seems critical to me that the bipolar layer counting has to be done continuously across the full interval of interest with no regard to the presence of abrupt transitions, and not just the periods of abrupt climate change.

Anders Svensson is an author on both studies, and I am interested to hear his perspective on this issue.

(2) the choice of the null hypothesis is critical here. The authors base their choice on the assumption that the Svensson 2020 bipolar catalogue is complete, which yields 1 large bipolar event per 500 years.

(2a) However, the quote from Svensson (2020) implies that the catalogue is not complete, but instead focused on periods of abrupt climate change or large volcanic acitivty. The recurrence time of such events should thus be shorter.

(2b) It is clear that the Svensson catalogue has more events during the deglaciation (12-16 ka) than during the glacial (24-60 ka). Performing a simple Student t-test on the recurrence time distribution of these two intervals will show this I believe. This makes intuitive sense given the larger annual layer thickness in the former interval. What would happen if the deglacial recurrence time was used as the basis of the Null hypothesis? There are also many bipolar links identified in the Holocene (e.g., AICC2012). What is the recurrence time of those?

(2c) The choice of large, 1-in-500 year bipolar events as the sole trigger for events is somewhat arbitrary. The model simulations suggest the system is most sensitive to NH forcing. So would it not make sense to use the largest NH eruptions instead?

(3) The discussion of literature is not very balanced, and heavily favors self-citation over important prior work by others.

(4) Work by Sigl et al. (2015) suggests that the largest volcanic eruptions influence

climate by up to 10 years. Why do you use a 50 year threshold instead? Is there a basis for this number?

Line comments

P1 L16: remove "past".

P2 L5-10: this seems contradictory. The models now show unforced oscillations, so wouldn't this obviate the need for triggers / drivers?

P2 L12: Though this depends really on the subjective choice of what one decides to call a stadial or interstadial. The events that follow the traditional numbering are all over 1000 years.

P2 L15-L22: Please refrain from self-citation in favor of a balanced review of the literature. For example, a long literature exits on the dependence of the DO timing characteristics on background climate conditions that predates the work by the author himself.

P3 L10: Is there a published bipolar ice core chronology? Or just the volcanic ties?

P4 L3: Are the

P6 L10-15: "Thus, most eruptions in the dataset of Svensson et al. (2020) fall into the category of 1-in-500 year events in terms of their magnitude". This is unclear to me. Do the 82 bipolar events you identify overlap with the 69 events by Lin 2021?

Further down it seems this is addressed in Section 3.2. Please consolidate this information into one place to avoid confusion. Although section 3.2 refers back to 2.3, so it is not clear how the analysis was done.

P6 L16: I strongly disagree with this conclusion. The Svensson (2020) paper itself clearly states that it is incomplete: "The bipolar layer counting is not continuous but is focused on periods of abrupt climate variability or high volcanic activity."

P6L 22: what do these p values signify? What threshold do you use?

P6 L32: It seems clear that the deglaciation ( 12-17ka) and glacial (24-60 ka) have different volcanic statistics. It is true one expects 2 false positives at 90% confidence, but that ignores the fact that BOTH of these are consecutive, and BOTH are in the youngest segment with thicker annual layers and thus higher detection probability for volcanic layers.

A t-test of waiting time distributions on either side of the data gap would probably suggest these are different.

Figure 3: Do you use the estimates from Lin 2021 for the bipolar volcanoes? In Fig 3c, what is the aerosol loading based on?

P11 L8: I don't understand the logic. Antarctica also has several large local volcanoes in close proximity (on the continent). Also, these NH eruptions probably had more impact on the AMOC that you care about.

P11 L10: Out of these 84 largest eruptions, how many correspond to bipolar events?

P13 L20: Personally I think at 50 years it would already be unlikely. Sigl et al. 2015 shows a ~ 10 yr impact on climate of the largest eruptions.

Section 3.5: Figure 5b is hard to interpret. Fig. 5a is much more intuitive. Can you also plot the result when doubling the events there?

Section 3.7: It is not surprising that your model is more sensitive to NH cooling. Does this not imply that you should be evaluating the proximity of DO event onsets to large NH volcanoes?

P19 L5: My main concern is the incompleteness, and the fact that the focus of the identification of bipolar events focused on periods of abrupt climate change.

P20 L11: These kind of statements are somewhat tentative, as this depends a lot on the weather conditions that distributed the volcanic deposits to both polar regions, and depositional processes and redistribution on the ice sheet surface.

P20 L12: “arguably more accurate”? I would say certainly more accurate!

P20 L19: I believe this is in violation of the data policy; all data should be made available. I will let the editor weigh in.