

Response to the comment from Jihong Cole-Dai.

This paper by Lin and colleagues extends the records of explosive volcanism constructed systematically from polar ice cores from the Holocene into the last glacial period. This is much valuable and needed research on volcanic records and the volcanic impact on climate.

[We thank you for the constructive comments.](#)

To reconstruct volcanic records from ice cores for the glacial periods, the authors faced several daunting challenges not encountered when compiling such records for the Holocene or shorter periods. First, detection of volcanic signals in ice cores depends critically on quantifying the non-volcanic background, and the background is much more variable during the last glacial period than during the Holocene; the larger variability is the result of both climatic variations (stadials vs. interstadials) and the presence of significant non-marine biogenic sulfate. Second, the Greenland and Antarctica cores in this study were analyzed with various sampling and analytical methods, resulting in datasets of various quality. Third, due to significant layer thinning with very old ice, temporal resolution of chemical analysis is reduced in glacial ice and, as a result, further complicates the detection and quantification of volcanic signals. Fourth, significant layer thinning at much older ages makes quantitative estimation of volcanic deposition difficult. In my opinion, the authors succeeded quite well in tackling these thorny issues and came up with a remarkable record of large volcanic eruptions for the last 60,000 years. It is worth noting that, due to the highly variable background sulfate levels during the glacial part of the period covered in this study, only extremely large eruptions are detected and quantified. Nonetheless, a record of extremely large eruptions is very valuable when it comes to assessing the climate impact of explosive volcanism, for we know from other studies that very large eruptions exert the most significant impact on climate.

The conclusions of this study are significant, not only because it is the first time that a systematic study of volcanism during the last 60 ka yields a robust record, but also it demonstrates that ice cores are capable of providing valuable information regarding volcanism and its climatic impact on time scales of millennia and longer, supplementing and/or enhancing knowledge from geological records.

I would like to offer a comment on a technical aspect. I find that in several places in this paper, the authors use terms such as “measurement techniques” and “measurement methods” for how ice core analysis yields sulfate data and how different “techniques” or “methods” yield data of various quality. Regarding the chemical measurement of sulfate, there are only two techniques: ion chromatography (IC) and mass spectrometry (ICP-MS for sulfur). The quality of data from these techniques is the same or similar, for any ice core samples. Where data quality may vary is when different sampling methods are used: discrete, continuous melting with online IC or ICP-MS measurement, or continuous melting followed by off-line IC measurement. Often, these sampling methods determine measurement resolution or temporal resolution. For example, when sulfate was measured using IC on discrete samples, the resolution is lower than that when the measurement was using FIC on samples from a melter, due to the higher sampling resolution of the melter. The quality of sulfate data is the same, as sulfate in both cases were measured with ion chromatography.

[We thank you for those insights. However, in this study we apply the NGRIP sulfate record that is obtained using a continuous absorption method that is different from the IC and ICP-MS techniques. The method is introduced in Röthlisberger et al. 2000 and applied by Bigler et al. \(2002\) to measure sulfate and detect volcanic signals. We now include the references in the manuscript.](#)

In addition, I ask the authors to consider the following comments on specific passages in the paper.

Line 68. Small deposition at low accumulation sites could be also, or even mostly, due to reduced wet deposition.

[This has been changed. In the manuscript it now says: Another reason for the lateral sulfate deposition variability is the amount and patchiness of snowfall, which may locally enhance the sulfate deposition for high snowfall areas compared to low snowfall areas for a volcanic event. Moreover, there may be more absent sulfate deposition events caused by post depositional processes on the snow surface, such as wind erosion \(Gautier et al., 2016\).’.](#)

Line 88-91. Are you saying that uncertainty in thinning-rate estimates also contributes to uncertainty/variability of volcanic deposition?

Yes, there are uncertainties on the applied, modelled thinning functions that are not well quantified, and in the deeper ice the thinning is very significant potentially leading to a large error of the estimated sulfate deposition.

Line 113. In my opinion, the main problem with tephra ID is not that tephra does not deposit with sulfate simultaneously. The difference in timing of tephra and sulfate deposition is usually small, less than one year. The main problem is that it is impossible to perform continuous search and analysis of tephra in any deep/long ice core with the current analytical (or technological) tools. Additionally, there are no objective standards on matching the tephra in ice core to ash from a particular eruption.

We have changed the text as follows: 'However, tephra layers are not always associated with sulfate peaks and most volcanic sulfate signals have no tephra associated with them.'

Line 150. The word "analytical" should be inserted in "different methods".

This has been changed.

Line 166-169. MSA correction is needed only when sulfur, not sulfate, was measured. In fact, the correction should not or cannot be applied to sulfate data.

We agree. This was a mistake and the MSA correction is only mentioned for the WDC record.

Line 220. Is there such an assumption?

No, we just want to mention that the meteorological conditions are different between the last glacial period and the Holocene period, so that some of the knowledge we have from the Holocene period may not be valid in the last glacial.

Line 294. "sulfate deposition strongly influenced by sulfate record type (IC, FIC, CFA)"? See my comment on a technical aspect.

See our earlier reply.

Lines 301-304. Deposition variation is due to different sampling methods, not measurement methods or techniques. See my comment on a technical aspect.

Again, in this case the measurement methods are different. Fig.S3 shows the volcanic sulfate deposition as obtained by different analytical techniques. More explanations are provided above.

Line 350. "Two bipolar volcanic eruptions are known from tephra to be Icelandic origin: Could signals in Antarctica ice cores be from contemporaneous eruptions in SH? I notice that the authors use the term "bipolar volcanic eruptions" for contemporaneous signals in both Greenland and Antarctica ice cores. I think it is acceptable to use this term, with the caveat that such contemporaneous signals may be left by simultaneous eruptions in both hemispheres, rather than a single eruption.

Yes, there is of course a risk that some of the bipolar synchronization events are not bipolar, but rather 'false' coincidences of eruptions occurring almost simultaneous in the two Hemispheres. We make the assumption for the bipolar volcanic eruptions that they are one and the same event based on our well synchronized volcanic sulfate signals from Greenland and Antarctic ice cores. However, based on the sulfate concentration alone we cannot exclude contemporaneous SH volcanoes, and in view of the rather low volcanic frequency the likelihood of co-occurring NH and SH volcanoes is very small. We are addressing this issue in Lines 131-134: 'It has recently become possible to test if sulfate has indeed reached the stratosphere, which is a prerequisite for being globally distributed, as the sulfate undergoes characteristic isotope fractionation in the stratosphere (Burke et al., 2019; Gautier et al., 2018; Crick et al., 2021; Baroni et al., 2008), but these analyses are still scarce for the Glacial.'

Reference

Röthlisberger, R., Bigler, M., Hutterli, M., Sommer, S., Stauffer, B., Junghans, H. G., and Wagenbach, D.: Technique for continuous high-resolution analysis of trace substances in firn and ice cores, *Environmental Science & Technology*, 34, 338-342, 2000.

Bigler, M., D. Wagenbach, H. Fischer, J. Kipfstuhl, H. Millar, S. Sommer & B. Stauffer. 2002. Sulphate record from a northeast Greenland ice core over the last 1200 years based on continuous flow analysis. In *Annals of Glaciology*, Vol 35, ed. E. W. Wolff, 250-256.