

Clim. Past Discuss., community comment CC2
<https://doi.org/10.5194/cp-2021-100-CC2>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on cp-2021-100

Jihong Cole-Dai

Community comment on "Magnitude, frequency and climate forcing of global volcanism during the last glacial period as seen in Greenland and Antarctic ice cores (60–9 ka)" by Jiamei Lin et al., *Clim. Past Discuss.*, <https://doi.org/10.5194/cp-2021-100-CC2>, 2021

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.

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.

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.

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

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

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.

Line 220. Is there such an assumption?

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

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

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.