

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

Laura Crick et al.

Author comment on "New insights into the ≈ 74 ka Toba eruption from sulfur isotopes of polar ice cores" by Laura Crick et al., Clim. Past Discuss.,
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General comment:

The study presents the sulfur isotopes of the potential 74 ka Toba eruption sulfate spikes in the Antarctic EDML and EDC ice cores (would be good to mention ice cores in abstract) adding new and valuable information to the ongoing Toba saga. The study is carefully carried out, figures are good and illustrative, the study is well referenced and the message is clear. Nice work, I have only a few minor comments.

We thank the reviewer for their invaluable insight, and we are grateful for the time spent to consider our manuscript in detail.

Reply to specific comments:

Specific comments:

- *124 onwards: What is the approx. time resolution of the obtained samples? I guess this info can be extracted from the supplementary info, but it would be good to mention in the main text as well.*

Sampling resolution varies across the individual peaks depending on their sulfate concentration. In general background samples represent 4–8 years of time whereas samples across the highest concentration regions of the sulfate peaks are 1–2 years. Due to diffusion these peaks have broaden and will cover more time than the initial deposition event. This clarification is included in the revised text at L127–130.

- *154: '..., this integration also corrects for thinning.' It is not entirely clear to me how the integration, that I assume refers to the sulfate peak area, implies a thinning correction? Doesn't this correction need to be done separately after the peak integration? In any case, it would be good to know which thinning models you are applying for the thinning correction (with some reference), and also it would be helpful to know the magnitude of the thinning correction for each core, as this could be quite significant at least for EDML?*

The following text has been added to the Methods section for clarification (L162–165): "... the total deposition is calculated by integrating over the flux from a given peak to the ice core. The flux is in turn the product of the concentration in a slice of ice and the snow

accumulation rate. However, as the input data is by depths rather than ages, we multiply by the reciprocal of the annual layer thickness at the depth of the slice. As this annual layer thickness is derived from the age model (Veres et al., 2013), the flux is corrected for thinning during the calculation.”

- 380: *You may also compare to the results of (Corrick et al., 2020) for absolute ages.*

We have amended the text include comparison to Corrick et al., (2020) (L495–499):

“Corrick et al., (2020) provide a comprehensive compilation of 63 published speleothem records, providing dates of $71,594 \pm 230$ years and $75,583 \pm 248$ years for GI-19.2 and GI-20c respectively. In comparison, using the AICC2012 age model we place these transitions at 72,142 years and 75,876 years with the GICC05modelext giving values of 72,340 years and 76,440 years (Rasmussen et al., 2014). Further interrogation of these records to determine onset of GS-20 would further improve our estimates for the ages of T1 and T2.”

- 400: *'This would remove...' -> 'This would suggest Toba to be unlikely as a trigger of ...' or similar.*

This alteration is included in the revised text (L504 –505).

- 402: *Which candidate gives 3 times the Salamas 1257 CE stratospheric sulfur loading?*

Using the sulfate concentration data for the Antarctic B32 core, we have recalculated the estimates for sulfur loading due to the Toba candidates (L302–321). We estimate a sulfur loading from T2 of 238 Tg S, 4 times greater than that of Samalas reported by Toohey and Sigl, (2017) of 59.4 Tg S (L481–483).

Figure 5: The repeated measurements eg for Salamas have different isotopic amplitudes and are probably obtained for different sample sizes? Would it be possible to show the temporal sample resolution (and maybe the sampled ice core) in the same Figure? In principle the 'true' amplitude of the sulfur isotopes could be extrapolated to infinitesimal sample size? There will still be diffusion in the ice that cannot easily be accounted for, of course.

We have amended Figure 5 to reflect the number of samples for each eruption, the ice core used in each study is detailed in the supplementary Table S2. Indeed, the magnitude of S-MIF measured will depend upon the sampling resolution, to investigate this further we have averaged the sample data from Burke et al., (2019) to represent a reduction in sample resolution, the results of which we present in supplementary Figure S9. We use Figure 5 to demonstrate the various studies utilising S-MIF over the past two decades and how our data for volcanic sulfate significantly further back through time compares to Common Era events.

Figure 7: Strictly speaking the Buizert et al, 2015, publication has nothing to do with the release of the NGRIP isotope profile. A better reference may be (North Greenland Ice Core Project members, 2004).

Thank you for highlighting this discrepancy, we have amended the references in the main text.

References

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