

Review of Wolff et al. “Frequency of large volcanic eruptions over the past 200,000 years”

Climate of the Past

11 October 2022

The authors present a new analysis of the EPICA Dome C (EDC) sulfate record, and add new sulfur isotope data, to provide a critical assessment of volcanic eruption frequency over the past 200,000 years. Despite the obvious importance of such an analysis to our understanding of the climate forcing implications of medium- and large-scale volcanic eruptions, and any possible climate-volcanic feedbacks, analyses of volcanic frequency over long timescales are sparse. There are good reasons for this, as the authors note in the introduction. The EDC record is well-suited to address the problem, and the authors have a long role in producing and interpreting sulfate data from EDC. The base set of parameters that guide their analysis might be debated, but nonetheless this is a valuable contribution and certainly relevant for *Climate of the Past*. My comments are mainly directed at providing better clarity and insight for the reader, which I hope the authors will consider during revisions.

Line 48: “*Antarctica has rather few local eruptions..*” – that is strongly location-dependent. Probably true for EDC, but certainly not for coastal regions. Please clarify the sentence.

Line 64: “*Antarctic ice cores will record some eruptions which did not reach the stratosphere but are smaller eruptions of more regional origin*” – As written, this seems somewhat inconsistent with the text in line 48. Please provide some clarity here – all Antarctic ice cores, some Antarctic ice cores, and if so which ones, are more or less affected by regional eruptions?

Line 65” “*These can in principle be filtered using sulfur isotope analysis..*” – I agree in principle, but not in practice – at least not yet as far as I understand – we cannot hope to distinguish every eruption with S isotopes. Please add some wording to define the boundaries as they currently stand.

Line 70: “*good depth resolution*” – please be more specific – what constitutes good resolution in this core and time interval of interest?

Lines 91-92: An expanded description of the author’s goals, and contribution of the study, would be helpful here. For instance, are they planning to providing any regional or larger context through comparison with other studies? How do the sulfur isotope factor in, etc.? The two lines of course are accurate, but limited in providing the reader with a broader perspective of what the author’s hope to do here.

Line 93: I think this section should be titled “Existing EDC sulfate data” or something like that, to clearly distinguish from new methods and data that are being used and contributed here in the “methods” section.

Lines 123-127 and Fig. 3: Without additional analyses, interpretation, or discussion, this section and figure don’t add anything to the paper. I would just cite Traversi et al., 2019 and say the sulfate data cannot be confidently interpreted below 200kyr yet.

Line 166: “*This width is justified because diffusion more or less keeps pace with thinning at EDC*” – the phrase “more or less” is a bit unsatisfying, given the stakes of this calculation. I admire the author’s ability to intuitively make that judgement, but some quantification justifying the decision would be helpful. If the following sensitivity studies do the job, then just say so.

Lines 187-188: “*..across 21 volcanic sulfate188 events from Dome C between 10.1 and 96.1 ka.*” – how and why were these events chosen? Are they representative of sulfate peak size, duration, etc. i.e., what was the sampling strategy and what implications might it have for interpretation (if any)?

Line 198: based on comments above, I think this section should more accurately be titled “..on the East Antarctic plateau” or something similar.

Line 229: “*largest recorded eruption in the timeframe that could accommodate the Toba eruption*”- I’m not exactly sure what that means – that the event with a flux of 133 mgm⁻² could be Toba but you’re not sure? Can you please clarify the wording and intent.

Lines 259-260: “*There will certainly also be an associated effect on the efficiency of the Brewer Dobson circulation that transports aerosol to the poles through the stratosphere..*” This needs more explanation. What is the specific mechanism here, and what raises your suspicion that it might be in play if it is not present in models? Not saying that models should guide your thinking completely, but as written it’s not clear what might be going on.

Line 266: Discussion – I am surprised to get the Discussion section without first seeing the S isotope data presented along with some analysis of error, data characteristics, etc. I think this is an organizational problem of the manuscript which should be corrected, before interpretations and comparisons are made in the Discussion section. Basically, a good portion of this paragraph (lines 267-277) should be in the “methods” section.

Lines 267-277: I don’t doubt, based on previous S isotope data and the new data presented here, that most of the sulfate getting to Dome C is stratospheric. But most is not all, and the proportions estimated by the various studies (49 of 64, or 76%, in Gautier; 18 of 21, or 86% here) are different. Is this difference significant? Why or why not? And what implications does that have for interpreting the data solely as a non-regional record (i.e., comments in the introduction)? This is discussed briefly in lines 294-295, but it would help to address it first here I think.

Lines 319-320: True, but given my comment above I would remove reference to this and leave it for a dedicated study.