Review: In situ measurements of perturbations to stratospheric aerosol and modeled ozone and radiative impacts following the 2021 La Soufrière eruption

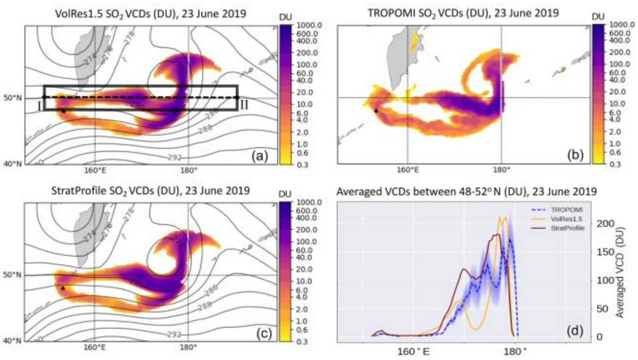
Li et al, 2023.

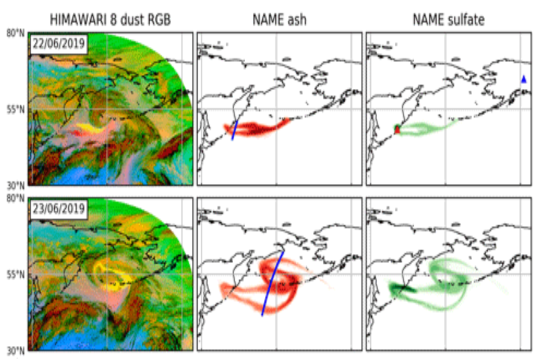
The paper addresses scientific questions that are well within the scope of ACP. The aerosol data collected in the DCOTSS stratospheric aerosol flights with the ER2 combined with balloon borne ascents in unique. The comparison between the SOCOL-AERv2 model and the observations is interesting.

The most prominent conclusion is that the aerosol size distribution in the lower stratosphere consists of more numerous smaller (presumably) sulfuric acid particles, with larger particles overlying them. Sound conclusions are drawn. The presentation in terms of clarity is good, with good figures and captions but a few clarifications and caveats are required. Some of the clarifications are important, and the authors should address them in a revision of the manuscript.

General comments:

1) Without a description of how ozone is impacted by aerosols within the SOCOL model, one cannot assess the model's treatment of any ozone depletion – at least a basic description of e.g. heterogeneous chemistry, treatment of PSCs should be given in section 2.4.
2) Any impacts on ozone from the eruption are likely to occur from a) heterogeneous chemistry, b) aerosol-induced stratospheric heating which can change the poleward transport of ozone. Given that you are using nudged simulations, the dynamically induced response is likely to be suppressed. This caveat should be included.
3) In the modelling, some mention of the approximate number of model layers in the stratosphere should be mentioned together with the model top. From looking at the supplement (e.g. S5) it appears that the resolution in the stratosphere is quite limited with 5-6 layers being represented.
4) Some acknowledgment of the limitations of model resolution (spatial and temporal) should be made. For example, the study of the combined eruption of Raikoke using a global model with a resolution of around 10km and 59model levels (e.g. de Leeuw et al., 2021, https://doi.org/10.5194/acp-21-10851-2021; Osborne et al., 2022; https://doi.org/10.5194/acp-22-2975-2022) does not have to make an injection into such a large area. The detailed evolution and 'filamentary' structure that is referred to in the text in this study is difficult to model with such a crude injection strategy and such a coarse resolution and an appropriate caveat should be made.





Left panel – SO2 (de Leuuw et al., 2021); right panel sulfate and ash (Osborne et al., 2022).

5) There needs to be an acknowledgement that comparison between Fig 2 and Fig 4 is not a like-like comparison as Fig 2 includes the background aerosol while Figure 4 does not. This is mentioned later in the paper, but needs to be acknowledged sooner.
6) In section 3.2 the statement, "As discussed in Section 3.1, the modelled plume agrees well with CALIOP/CALIPSO ……" is rather pushing it. There is one sentence in section 3.1, which does not constitute a discussion. There is no quantitative analysis that supports the 'good agreement' – its just done by eyeballing the two plots. Ideally the background could be removed from multi-year CALIPSO data. Then you'd be much closer to a like-like comparison and could provide quantitative numbers to back up your text.
7) Do the authors think that the rapid change in the observed size distribution with diameter by the POPS (which is a nice bit of lightweight kit) at 300nm diameter is real? I know that there have been some comparisons between the POPS and other instrumentation such as the SMPS which operate quite differently in terms of physical measurements, and there seem to be some differences in the slope of the size distribution that is derived between the two instruments (e.g. Liu et al., 2021; https://doi.org/10.5194/amt-14-6101-2021). There has been some discussion with Handix as to whether the cross over in gain stages of the pre-amplifier/amplifier could have some influence. Whatever, the case, it does seem a notable feature throughout the results that are presented here.

Specific comments:

8) L121 – size distribution – is this radius or diameter? Actually you can find that this is diameter (L198), but diameter should be stated here.
9) The size distributions and effective diameters in Figure S6 & S7 should be the same as those in Figure 6 and 7 to aid comparison.
10) The white lines and numbers in Fig 2 should be boldened.