Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-480-RC3, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Evidence for the predictability of changes in the stratospheric aerosol size following volcanic eruptions of diverse magnitudes using space-based instruments" *by* Larry W. Thomason et al.

Anonymous Referee #3

Received and published: 20 July 2020

General comments

The paper describes the relationship between the perturbation in the aerosol extinction coefficient and the extinction ratio or particle size in the early months following small to midsize volcanic eruptions. The authors acknowledge the limitation of the analysis, which were restricted to early months following volcanic eruptions while tracking the main layer, mainly because of the presence of significant amount of ash. I believe that the analysis presented here are incomplete and the paper would benefit from expanding the analysis to a longer period (not only the early months) and wider range

of altitudes, rather than the peak of the aerosol layer. In addition, the paper needs clear and defined objectives (see my comment below).

Specific comments

Page 3, L64-66: 'space-based missions are mostly limited to single wavelength measurements associated with instruments such as . . .' The statement is only valid for the datasets used in GloSSAC climatology. CALIPO routinely provides aerosol measurements at two wavelength 532 and 1064 nm, while OSIRIS can provide measurements at 750 and 1530 nm, which were used to derive stratospheric aerosol particle size information (Rieger et al., 2014). In addition, MAESTRO (McElroy et al., 2007), GOMOS (Vanhellemont, et al., 2016), and SCIAMACHY (Taha et al., 2010; Malinina et al., 2019) can also provide aerosol measurements at multiple wavelengths. While the quality of the measurements is debatable, its existence is not.

Page 3, L73-74: 'It is also, until the start of the SAGE III mission, a period where the long-term stratospheric record is less robust due to the lack of global multiwavelength measurements of aerosol extinction coefficient.' Again, the Authors are describing the GloSSAC dataset rather than the global stratospheric records. See previous point.

Page 3, L76-81: 'Thus, the original aim of this work was to understand how volcanic events manifest themselves in SAGE II/III observations with the goal of 1) inferring the uncertainty in single wavelength space-based data sets that use a fixed aerosol size distribution as a part of their retrieval algorithm such as the OSIRIS and CALIOP and 2) infer how well the wavelength dependence can be estimated for these single wavelength measurements.' The authors failed to address both objectives and I don't see how the paper's findings, in its current form, can be of any use to these instruments because of the limited analysis shown her. I suggest either expanding the scope of the work to address those objectives or revising it to more realistic objectives.

Table 1: 'Volcanic eruptions and smoke events that significantly impact stratospheric aerosol levels in the Version 2.0 of the GloSSAC data set' Figure 1 shows very low

aerosol and no impact of any of the volcanoes listed between 1998-2004, which implies that none of these eruptions reached the stratosphere. I suggest revising Table 1 by removing all volcanic eruptions that is not seen by GloSSAC. In addition, add the eruption altitude, similar to Table 2.

Figure 3: Unlike the rest of the analysis shown in this paper, the figure is for GloSSAC zonal mean aerosol stratospheric optical depth and ratio rather than extinction coefficient and ratio for the peak aerosol layer. I suspect that the extinction ratio is inaccurate, given that SAGE II measurements were missing following the early months of the eruption, and the dataset was mostly reliant on single wavelength Lidar measurements. For the sake of consistency, I suggest using SAGE II measurements of the aerosol extinction and extinction ratio, similar to Figures 4-7.

Figure 4a: Can you use different color for the extinction values at 20.5 km?

Figure 4b: Can you plot all data shown in Figure 4a while using different color for extinction ratio at 20.5 km?

Figures 4, 6, and 7: The Days label is confusing. Can you use month/year or something similar?

Figure 5: change 'Same data as shown in Figure 3' to Same data as shown in Figure 4a or 4b. In addition, can you specify if the data shown are for 20.5 km or all altitudes? If so, can you use different color for 20.5 km.

Page 6, L189 'the maximum extinction coefficient at 525 nm does not necessarily occur at the same altitude or time as the maximum in 1020 nm extinction coefficient'. This is all the more reason to track the plume at different altitudes/zones rather than the maximum extinction value.

Figure 7: I suggest over plotting points symbol to show the number of points used in each figure, adding a second vertical line for Ambae and Ulawun denoting the second eruption date, and using the same x-axis scale for all figures.

Figure 9 and page 7, L220 'It is clear here that the maximum in the extinction ratio lies below the main peak in extinction coefficient in the tropics and, notably stretches to higher southern latitudes and the maximum values actually occurs near 30° S despite more inhomogeneous conditions at this latitude than in the tropics' and page 8 L240: 'Both eruptions show increased aerosol extinction coefficient ratios away from the main aerosol peak suggesting, at least in part, behavior more consistent with most eruptions.' This is interesting observation that raises more concerns about the analysis shown in Figures 6 and 7. Is it possible that tracking the maximum extinction value is not the best approach as it might bias the outcome, especially where the aerosol extinction is very large? Very large extinction values can be caused by the presence of Ash particles, or it is an artifact in SAGE measurement when the volcanic plume is localized and spatially inhomogeneous. In addition, the result results shown here can be easily biased by SAGE limited coverage. Perhaps repeating Figure 7 using zonal means at different altitudes and extending period of the analysis can produce a more consistent relationship between the aerosol extinction and extinction ratio perturbations.

Page 8, L246: 'peak extinction level as essentially all the data follows the mean relationship in Figure 7g.' The sentence is unclear. This correct for Ulawn, however, the paragraph is discussing Raikoke eruption. Please revise the sentence.

Page 8, lin3 253: 'It is also possible that a pyrocumulus event, that occurred in Alberta, Canada just prior to the Raikoke eruption, plays a role in the evolution of extinction following this event.'. In addition to Alberta, there was a second PyroCb event in Siberia, Russia in July 2019, that reached the stratosphere and was also seen by SAGE III/ISS (https://directory.eoportal.org/web/eoportal/satellite-missions/i/iss-sage-3, Figure 7). It is more likely that the smoke aerosol interfered with the Raikoke analysis. If the two different aerosol layers were separated (which is most likely), then repeating the analysis at different altitudes instead of tracking the peak extinction can explain the behavior seen in Figure 7h.

Section 4: The first paragraph leading to the aerosol perturbation model relies on unsupported speculations (by the authors own admission), and it can benefit from the addition of few references that support those assumptions.

I also find the whole discussion regarding the model calculation and figure 11 confusing and I can't relate the figure to what was presented in the previous section. The aerosol extinction ratio perturbations for Ambae are almost double those for del Ruiz eruption and figure 11b in particular shows large extinction ratios (>5) not seen by any of the cases shown in this paper. Close inspection of both figures indicate that the extinction ratio perturbation is very sensitive to the baseline ratio, and if Manam or Ulawn baseline values were used, the extinction ratio values would've been even higher.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-480, 2020.