Dear Authors,

I am now in possession of two reviews of your revised manuscript. While the first referee is satisfied with the changes to the original submission, the second is more critical and requests additional modifications. In line with their comments, I find that a few relatively minor but necessary changes are still required before the paper can be considered for publication.

Specifically, you may wish to consider the following suggestions:

1. Changing the paper title to emphasize the vertical distribution of SO2 rather than the resolution of the dataset, making it more consistent with the actual content and discussion in the paper. It would also be relevant to mention which eruption is under investigation (the June 2009 Sarychev eruption).

We agree that the title was a bit misleading. A major finding in our manuscript is the importance of compiling SO2 data with instruments that have high-vertical resolution and provide altitude information with high accuracy. Even though the CALIOP data has been available for ~15 years, we are likely the first group to simulate the eruptions as thin SO2 layers separated by km in altitude, resulting in a vertical profile with more modes than one. CALIOP, with its high accuracy in vertical profile estimates, is to our knowledge, the only instrument that could provide data of this kind. Our study is <u>not only</u> an investigation of the importance of the vertical distribution of the SO2. Such a sensitivity study could have been performed by many other groups using assumed SO2 profiles. A major importance of our study is that we use the first SO2 dataset that has been compiled with high accuracy vertical information and show that the model can perform more realistic estimates of the aerosol load when run with this data.

We have tried to stress this in our manuscript as well as in the new manuscript title: "Impact of SO2 injection profiles on simulated volcanic forcing for the Sarychev 2009 eruptions - investigating the importance of using high vertical resolution methods when compiling SO2 data".

We are aware that this title is longer than the original title. However, the simulations themselves are not the main achievement in this work. As stated above, a major importance of our study is that the simulations were performed with SO2 data that were compiled with high vertical accuracy and precision.

2. Adopting the suggestion (by Ref. 1 of the original manuscript) to show the injected SO2 profile in Figure 1 on the model vertical grid for June 15-16, as represented for S21-1D and M16. Including the location of the model levels would also be helpful for readers.

We have created a figure depicting the SO2 profiles during the first 6 days after the eruption (15-20 of June) to the supplementary. The figure includes the location of the model levels. We have also added a short paragraph discussing this figure in the results section.

3. Adjusting the statements discussing the need for high resolution to make them more consistent with the actual findings of the paper, which highlight the sensitivity to the vertical profile of SO2 (and the partitioning between troposphere and stratosphere) rather than the importance of fine

vertical resolution data. I am thinking, for instance, of the last sentence of the abstract or line 300 in the conclusion.

We agree. We have gone through the manuscript and changed the text to highlight that the vertical placement of the SO2 rather than the resolution of the dataset is the critical factor for the results. We have changed both the sentences suggested by the editor.

Please also address the specific and general comments by the referees.

We have answered all of the reviewers' comments.

Many thanks for the careful revision of the paper. I just have one tiny remark. Please add to Fig. 8 where you picked the radii. Are the plotted radii the maximum values or values at a certain altitude?

Thanks for this remark. The effective radius is the weighted geometrical mean for the entire stratosphere. This is stated in the caption for Figure 8.

After reviewing the authors' responses, I appreciate their efforts to address each question raised in the reviewers' comments and the community feedback, as well as the minor modifications made in response to the reviewers. However, I still have a lingering concern regarding the main focus of the study. Initially, I had hoped to see a demonstration of how the authors' SO2 emissions could be shown to be more accurate than those in M16, potentially due to improved resolution of both the volcanic SO2 plume height and mass. This would, ideally, result in better simulation outcomes compared to those based on M16. Unfortunately, this crucial point seems to remain unaddressed in the revised manuscript.

I acknowledge that the authors have undertaken simulations using their own developed volcanic SO2 emissions (S21-1D and S21-3D) instead of relying on the original volcanic SO2 inventory (M16), which is commendable and bold. However, the contribution of this work to our current understanding of volcanic impact simulation remains somewhat unclear to me.

While M16 is largely derived from satellite observations and may have a coarse resolution, it is generally not inaccurate when compared to observations. Although S21-1D and S21-3D offer high vertical resolution, the data must be interpolated to model levels, as pointed out by Reviewer #1. Additionally, as noted by other reviewers, S21-1D and S21-3D, particularly S21-3D, appear to overestimate SO2 mass and altitudes, leading to excessively high aerosol burdens and altitudes in the simulations. Consequently, the results from these simulations do not exhibit significant improvement over those based on M16. While S21-1D and S21-3D have been discussed in Sandvik et al. (2021), this does not inherently qualify them to enhance volcanic impact simulation performance. Even Sandvik et al. acknowledged the elevated and larger estimates compared to other studies. It seems that S21-1D and S21-3D may require further refinement before their application can be expanded.

M16 assumes uniform vertical distributions of SO2 after volcanic eruptions, which is a simplification that does not align with the observed distributions of aerosol after many volcanic eruptions.

Since SO2 sensors lack the high vertical information/resolution needed to retrieve precise SO2 vertical distributions, we use the formed aerosol particles as evidence for the vertical location and distribution of the injected SO2. CALIOP revealed that almost half of the aerosol formed from SO2 after Sarychev reached above the 380 K-isentrope, of which a fraction was transported to the tropics within the shallow Brewer-Dobson branch (e.g. Figure 8b in Friberg et al. 2018). Hence, half of the SO2 was injected above the 380 K-ienstrope. This is not the only example when M16 misrepresents the vertical distribution of the SO2. For example, the Kasatochi eruptions in Aug 2008 injected SO2 to varying altitudes forming two main aerosol layers separated by ~5 km in altitude (Figure 2 in Andersson et al., 2015; Friberg et al., 2018). M16 treats Kasatochi as a uniform layer spanning 10-18 km altitude, whereas CALIOP shows that the aerosol was positioned in two layers centered at ~10 and 16 km in the extratropics (the vertical position is better described by isentropes), with most of the aerosol located close to the tropopause. Hence, M16 does not capture the complex vertical distribution of Sarychev 2009 and Kasatochi 2008.

Regarding the manuscript's title, it may inadvertently be misleading. The emphasis on "high resolution" could suggest more than what the current findings substantiate, and the title might not accurately reflect the core results of the study, potentially leading to skepticism.

We have changed the title of the manuscript to align with this comment.

The methodology used to derive S21-1D and S21-3D (Sandvik, 2021, AMT) might benefit from additional consideration. The approach of constructing profiles by tracking volcanic aerosol signals from CALIOP above the tropopause might contribute to missing data at lower altitudes and the tendency to place SO2 injections in the stratosphere.

This would cause bias if using SO2 data that captures SO2 in the entire atmospheric column. We therefore used an UTLS SO2 product produced by Fred Prata and used CALIOP backscattering data to tell the vertical SO2 profile. Hence, low altitude SO2 data does not impact the SO2 profile. CALIOP provided precise altitude resolved data at high resolution.

These comments are offered with the hope of enhancing the scientific significance of the manuscript to live up to the standard of ACP. I commend the authors for their work and encourage them to continue refining their approach.

PS. I noticed that in the replies, the author answered that the results could be compared with CALIOP aerosol data because CALIOP is independent of their data. However, it's important to note that the initial SO2 profile was derived by tracing the CALIOP aerosol data backward. This raises a question about the extent to which the CALIOP data can be considered independent in this context.

CALIOP was used to estimate the altitude of the SO2 measured by AIRS, while the mass estimate where produced by AIRS data. Hence, CALIOP was not used to determine the amount of SO2 put into the model. The SO2 is put into the model as a gas phase compound. The model then simulates aerosol formation, transport of both SO2 and aerosol, the evolution of the size distribution of aerosols in relation to the injected SO2. The simulated aerosol extinction coefficients computed months after the eruption depend on the injected SO2 profiles but also on many other parameters. Hence it is realistic to evaluate the model performance against CALIOP measurements during the months after the eruptions. We are interested in simulating the time evolution of the aerosol formation and transport in the stratosphere. Using CALIOP to retrieve the vertical SO2 profile generates initial SO2 profiles, but the subsequent aerosol formation and atmospheric dynamics is independent of CALIOP. Hence, we argue that CALIOP is well-suited for the comparison used in our manuscript.