Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-94

Anonymous Referee #2

Referee comment on "Methodology to obtain highly resolved SO_2 vertical profiles for representation of volcanic emissions in climate models" by Oscar S. Sandvik et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-94-RC2, 2021

This paper presents a new method deriving vertical profiles of SO_2 representative of volcanic emissions by combining space-borne observations of SO_2 by AIRS/Aqua and of aerosol by CALIOP/Calipso. The profiles are constructed by connecting the CALIOP high resolution aerosol profiles with the AIRS SO_2 columns using trajectory calculations.

The interest of providing SO_2 profiles at the period of a volcanic injection in the stratosphere is definitely clear for the community especially for the initialization of Chemistry-Transport and Climate Models that aim at estimating the chemical and radiative impacts of volcanoes. So I found the idea behind the manuscript of very high scientific return.

I found the manuscript structure pertinent, well-written and going straight to the obtained results (but a bit too much). Although I estimate that this work is really worthy of publication in AMT, there are some methodological elements that still need to be clarified throughout the text.

General comments:

It can be difficult to grasp how the trajectories interconnect both satellite datasets (e.g. how they intersect, typical trajectory lengths). An illustration of a specific case linking CALIOP at a given altitude level and a AIRS swath would be helpful for reader. The relevance of using forward trajectories starting from CALIOP aerosol profiles (aerosol are a product of SO_2 so I much better clearly understand the use of backward trajectories) can be clarified too. Why in the example of the command file the simulation lasts for 6 days? To me (but I may be mistaken!) the authors should find CALIOP and AIRS swaths capturing the volcanic plume every day, allowing them to compute trajectories of a few hours only to connect both satellite observations.

A clearer description of the limitation in the method is missing. For instance, since what would be the impact of the sedimentation of the aerosols in the time length of the trajectories that on the attribution of the retrieved SO_2 plume heights? I guess the impact would be limited but I would suggest the authors to mention it. In case of the presence of ash which has been reported for some eruptions (Kelud, Raikoke) and as derived from CALIOP depolarization/colour ratios, how the method would be affected? Can the CALIOP instrument adequately distinguish between ash and sulfate to properly associate the aerosol detection with the corresponding SO_2 observation away from it? The relevance of the methodology for other reported eruptions could be more discussed.

Specific comments:

Introduction: I found the comment by Xue Wu of high interest and I strongly recommend to add their references about their similar method in the introduction.

P1 line 22: the temperature impact of the Pinatubo aerosols is debated in the community and I suggest to add more recent references such as Canty et al., Atmos. Chem. Phys., 13, 3997–4031, 2013

www.atmos-chem-phys.net/13/3997/2013/.

P3 line 22: The authors should cite also the work of Günther et al. (Atmos. Chem. Phys., 18, 1217–1239, 2018https://doi.org/10.5194/acp-18-1217-2018) who present a synthesis of the emitted SO2 masses and height ranges (see their Table 1).

P4 about AIRS: Do the authors know how this dataset compares with other SO2 datasets such as IASI (Clarisse, L. et al.: Retrieval of sulphur dioxide from the infrared atmospheric sounding interferometer (IASI), Atmos. Meas. Tech., 5, 581–594, https://doi.org/10.5194/amt-5-581-2012, 2012)? If available, I would suggest to add this information in section 2.1.

Figure 1: following the general comment given above, adding the CALIOP paths superimposed to AIRS swaths would be helpful for the reader to see which part of the SO2 plume is closely captured by CALIOP.

Please specify the exact date and/or time range of each swath on Figure 1.

P6 line 11: why the investigation of the proportion of the SO_2 plume located in the troposphere, which is by the way an interesting information, cannot be done automatically? Is it due to errors in following air masses using the trajectories in the troposphere (especially along isentropes)?

P6 line 23: I am fine with the method using depolarization and colour ratios from CALIOP observations to point out the presence of ash and ice but there is a lack of information here. At least please refer to Vernier et al. (2016) who describes the method using depolarization ratio and add the information accordingly.

P7 line 24: Judging by the difference in vertical resolution between ERA5 (several hundreds of meters) and CALIOP (60 m) in the stratosphere and also horizontally, please specify that the meteorological fields are spatially interpolated to release trajectories from each CALIOP profile.

P7 line 27: I do not really understand what the authors mean here with the 2 standard deviations (in meter and mbar) provided here. Do they correspond to the final vertical resolution of the SO_2 profile?

P8 line 17: Please better define what you mean by a pixel for CALIOP. How do you obtain pixels from a smooth signal (i.e. from fig 4b to 4d)? This is an important step I think.

P9 line 2: "backscatter" instead of "scattering"

P9 line 4: When mentioning "95,000" particles, do you mean in total for one subcloud or for each pixel? What is the time step of the particle release? Every 6 hours?

P10 line 30: the authors do not provide details about the role of the weighting of FLEXPART outputs and how they do it. Please clarify.

P11 line 3: I do not really agree with this statement. E-folding of SO_2 is about 13-17 days (see Haywood et al., 2010; Lurton et al., 2018). It can be mentioned here that 9 days is lower than the reported e-foldings for the Sarychev proving that SO_2 is still present in high quantities over the 9-day time length of the trajectories.

Figure 5: I suggest the background of the AIRS swaths to be coloured in grey rather than dark blue to better highlight SO_2 fields.

Did the authors focus on the 18^{th}? Is it because there is concentrated SO_2? Why trajectories are calculated over 9 days? In figure 5, how many CALIOP profiles have generated each mapping of transported aerosols and matched with AIRS swaths? If I understand well, all trajectories over the 14-22 June period have been used to reconstruct one single AIRS swath but why not focussing on a day-by-day basis, i.e. considering only trajectory calculations from the CALIOP tracks on the same day of the AIRS swaths? This would possibly limit effects of aerosol sedimentation (as a result of growth and coalescence) that can bias the mapping if trajectories are calculated on a too long period.

Figure 6: Not all the labels are visible on fig. 6a because of the colour choice. 11 AIRS

swaths are labelled in fig. 6a but only 9 are shown on the top left list. For figs. 6b, c, d I am wondering if using a log-scale will make all profiles corresponding to the AIRS swaths in fig. 6a more visible.

The authors have chosen the 18-19 June for the application of their method. However, at this stage, the SO_2 plume is already geographically extended. Then, does the method properly capture the more localized and main injection event (i.e. June 15^{th} in Haywood et al. and Lurton et al. studies)? This is of primary importance for robust initialization of models that account for SO_2 chemical cycle producing sulphate particles.

Figure 7: I suggest also to indicate the date of the profiles in the mentioned studies.

P14 lines 12-13: I am not sure that if the model studies indeed missed the highest cloud of SO_2 (i.e. around 15.5 km in fig.7) the consequence on the retrieved aerosol space-time distribution is significant. In Lurton et al. for instance, the agreement between the simulated aerosol profiles and in situ observations are matching pretty well even if the initial SO_2 injection is underestimated by considering the results in fig. 7 (red profile). This may be due to the coarse vertical resolution of global models in the lower stratosphere (~1km) which dilutes (or spreads out) the vertical distribution of the SO_2 profile and limits the impact on the vertical profile of the subsequently produced aerosols. Also, the model results tend to show longer residence times than in the observations. I suggest the authors to mention this possibility. I would suggest to remove the sentence ("Their release…")

P15 lines 27-29: "Our deduced…sulphate particles." I suggest to remove or modify this statement since 1) the SO_2 vertical distribution (which is visible only over the first weeks after the eruption) cannot be directly connected to the one of sulphate particles several months after the eruption as a result of vertical motion (Brewer-Dobson Circulation, sedimentation) sustained by the particles over such a long period and 2) no modelling study has been conducted yet to quantify the effect of the new vertical profile of SO_2 after the publication of the authors'work.