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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of the vertical profile of sulfur dioxide injection into theatmosphere by a volcanic eruption using satellite column measurements and inverse transport modeling" *by* S. Eckhardt et al.

Anonymous Referee #2

Received and published: 24 April 2008

I apologize to the authors and editors for posting this review after the end of the discussion phase.

This is an interesting ms which describes a novel technique for inferring the vertical profile of SO2 in volcanic eruption plumes from an inversion of satellite measurements of spatial SO2 distributions in volcanic clouds. It is a nice demonstration of how multiple, multi-spectral satellite datasets can be combined with trajectory models to provide critical information on the altitude of volcanic emissions and to validate the observations and model predictions. The example chosen is an eruption of Jebel at Tair volcano

(Yemen) in October 2007, which occurred in very dry and cloud-free conditions.

I recommend publication of the ms in ACP following some revisions. I have a number of specific comments on the ms that are detailed below. I am not an expert in trajectory modeling and so I am unable to provide a detailed critique of the modeling approach (or the equations given), but it seems entirely reasonable given my limited knowledge. One controversial aspect is the authors' definition of the tropopause altitude — this needs to be explained more clearly, as in my view it is arguable whether the volcanic cloud did reach the stratosphere. Also, the dry atmosphere and cloud-free conditions during this eruption were ideal for the satellite measurements, but many eruptions occur in the humid tropics and/or in cloudy conditions. A comment by the authors on the general applicability of their technique would be welcome. I would also recommend a thorough check of the English and grammar — the writing is generally good but lapses in a few places.

The authors stress in several places that the inversion technique could be applied in "real-time" to mitigate volcanic cloud hazards to aviation, by providing the necessary source term for trajectory models. However, although the computations are rapid the technique relies on satellite measurements of SO2 that may not be available until several hours (or more) after the eruption (except for SEVIRI data, but the ms suggests that several hours worth of SEVIRI data are also required for the inversion). "Real-time" for the aviation industry means within 5 minutes of an eruption, so I think this application of the method could be played down. I do agree that the method would improve the accuracy of long-range forecasts of volcanic cloud transport and provide improved constraints on the vertical extent of the volcanic cloud, both important for hazard mitigation and aircraft rerouting.

Specific comments:

Abstract: indicate that Jebel at Tair is in Yemen.
Abstract, line 17: not all the aerosol observed by CALIPSO was stratospheric (also

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see later comment on Figures).

Abstract (and throughout the ms): I think "a priori" should be in italics to distinguish it from the surrounding text.

P3763, line 7: suggest replacing "mineral" with "silicate".

P3763, line 10: non-explosive volcanic eruptions can actually send plumes to upper tropospheric altitudes, e.g., the 1984 eruption of Mauna Loa (Hawaii) reached 10-11 km. Passive or quiescent volcanic degassing occurs at lower altitudes.

P3764: the sensor acronyms (SEVIRI, OMI etc) should be defined at their first occurrence in the ms.

P3765, line 3: "..proxy for volcanic ash"

P3765, line 10: insert "(Yemen)" after Jebel at Tair.

P3765, section 2: the volcano seems to be somewhat displaced from the actual mouth of the Red Sea.

P3765, line 19: is "Yemeniti" an English word?

P3765, line 27: UT and local time are mixed up here.

P3766, line 26: please briefly explain what the WMO standard definition of the tropopause is. Using the temperature profiles provided in Fig. 2, one would probably place it higher, at 17 km. This is important as it determines whether the cloud did indeed penetrate the stratosphere or not. It would also be useful to provide (in Fig. 2) the wind profiles (if available) from the radiosonde sounding at Abha (Saudi Arabia), for comparison with the ECMWF profile.

P3767, line 10: at the end of this paragraph please specify the time range used to represent the "main explosive eruption".

P3768, line 1: "A-Train" may require a little more explanation.

P3768, line 14: V003 OMI SO2 data are now available from the NASA DAAC. If V002 data were used for the analysis, this should be stated.

P3768, line 25: capitalize "IFOV"?

P3769: the interference of water vapor on the IR SO2 retrievals is discussed here. I wondered if there is any information on the expected vertical water vapor distribution

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in the ECMWF data that could be used to assess the precision of the IR retrievals?
P3770, line 26: should be 532 nm.
P3771, line 6: the eruption was of Soufriere Hills volcano, on Montserrat.
P3772: Here there is a discussion of the differences in SO2 vertical columns retrieved by OMI, SEVIRI and AIRS. Presumably this is at least partly due to the variable sensitivity of the instruments to the altitude of the SO2, but the discussion of height sensitivity appears later (section 3.6). I think that the discussion of height sensitivity should occur before the measurement comparison in section 3.5. Also, the SO2 altitude used in the AIRS and SEVIRI retrievals should be clearly stated — it doesn't seem to be at present.
P3772, line 18: deposition (wet or dry) is unlikely to be significant for a cloud at this altitude on a timescale of a few days.
P3774, line 19: check that the correct OMI averaging kernel from Yang et al. (2007) has been used for this case, with SO2 distributed between 15-20 km (Umkehr layer 3). Note that this does not correspond to a "clear atmosphere" as stated in the ms but to an atmosphere perturbed by SO2.
P3775, line 14: again, dry deposition seems irrelevant for a volcanic cloud in the UTLS, but I suppose there is no harm in including it in the model. But how much SO2 is removed by this process in the model?
P3776, line 15: here and elsewhere the authors use "prior" where it appears that they mean "a priori" — please check and replace if necessary.
P3776, line 23: "squared deviations between model and observations."?
P3777, line 11: "done by minimizing.."
P3777, line 14: "misfit between model and observations.."?
P3778, line 15: "all three satellite data sets.." (delete "the")
P3779, line 14: replace "satellite" with "sensor".
P3779, line 20: delete "they".
P3781, line 11: replace "during the hours" with "from 10-24 hours after the eruption."
P3781, line 13: there is also a prominent peak at 9 km (larger than the 5 km peak). I

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am not sure of the significance of the minor peaks in the inversion profile below 15 km, as they are dependent on the wind profile and hence on the accuracy of the ECMWF model winds.
P3781, line 14: move the “10%” to the next line, before “above the cold point”
P3781, line 20: please clarify what indicates the layers of enhanced stability in the temperature profile.
P3784, line 14: replace “a too slow” with “insufficiently rapid”?
P3787, line 2-3: a comment here by the authors suggests that “some more text” was due to be added. Perhaps a discussion of the lofting of sulfate aerosol to explain the location of the aerosol detected by CALIPSO at the top of the simulated volcanic plume.
P3787, line 15: I think only OMI observed the SO2 plume for more than a week.
P3788, line 15: not all the aerosol observed by CALIPSO was stratospheric — it was mostly upper tropospheric.
P3789, line 5: as I mention above, I think the “real-time” application is a stretch, at least using the aviation industry definition of real-time. The main constraint is the availability of the required satellite observations.

Fig. 2: give the location (lat, lon) of the ECMWF profile. The criteria used to define the tropopause need clarification.
Fig. 4: it’s a little difficult to distinguish the blue and black data points — suggest using higher contrast colors.
Fig. 5: check that the correct curve is plotted for OMI.
Figs 9-11: check the magnitude of the CALIOP attenuated backscatter values as they seem too high (>0.2) - CALIOP data I have seen have much lower values of attenuated backscatter (0.0001-0.001) for volcanic aerosol layers. Why does the tropopause altitude plotted in Figs. 9 and 11 show a sudden plunge at a certain latitude?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 3761, 2008.