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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.

S. Eckhardt et al.

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We thank referee 1 for the comments on our manuscript. In the following, we address the comments point-by-point by repeating the reviewer's text in italic letters, followed by our response in normal letters.

General Comments:

The real-time application of the presented method to ash plumes in VAACs doesn.t appear to be realistic at least at the moment. SO_2 is not a simple substitute for ash. More discussion is necessary.

We are not presenting a method for ash plumes. We just point out that the method could be adapted for the VAAC ash plume problem. In our manuscript we indicate, that SO_2 can serve as a proxy for volcanic ash. However, we added a discussion on the relation between SO_2 and ash.

It is generally acknowledged that ash is the primary volcanic hazard to aviation at cruise altitudes (e.g. Miller and Casadevall, 2000). Explosive eruptions, involving hot, viscous magma, have sufficient thermal energy to bring volcanic debris up to aircraft cruising altitudes or higher. These explosive eruption events are also gas-rich and contain copious amounts of H_2O, CO_2 and SO_2. In a sheared atmosphere the ash and SO_2 in these eruption clouds may travel in different horizontal directions, but the SO_2 is likely to reach higher altitudes, be accompanied by some ash and remain in the atmosphere for a longer time. The motivation for using SO_2 as a tracer for hazardous volcanic clouds derives from two observations: It is critical to have accurate information on the altitude reached by volcanic debris, and SO_2 is often more easily observed by satellite sensors than ash and has a greater atmospheric residence time.

Miller, T. P., and Casadevall, T. J., 2000, Volcanic hazards to aviation, In: H. Sigurdsson (ed), *Encyclopaedia of Volcanoes*, Academic Press, San Diego, CA, 915–930.

The authors need to define what they mean by real-time. If their method needs satellite data for 15-24 hours after the eruption the method is not suitable for plume detection, however, might still be useful for plume tracking on time scales of several days.

What we define by real-time application is creating an operational system which can be used as the event is happening.

While this paper is purely methodological, the method developed could easily be used in real-time. The method does not intend to *detect* volcanic eruption plumes but once a plume is detected and first satellite measurements are available, it could be a useful real-time application to improve aviation hazard warnings.

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For instance, in the case presented here, useful inversions were obtained with SEVIRI data from the first 15 hours after the eruption. The SEVIRI data flow is fast, the model calculations are relatively fast, too, and the inversion only takes a couple of minutes. In a real-time environment, the first inversion results could have been available about 16-18 hours after the eruption. This is a critical phase for aviation. Initially, it is sufficient for aircraft to avoid the immediate vicinity of the volcano. No model predictions are needed at this point anyway. Later, however, as the plume spreads, aviation needs to avoid larger and larger areas in case of a high-altitude volcanic plume, and models are needed to define these areas. Having an estimate of the emission profile after 16-18 hours will facilitate better predictions of the affected areas and altitudes over the following days. This is highly relevant for aviation and could occur in real-time as relevant for aviation. The emission profile can even be improved as time passes and, ideally, the method could be used in a data assimilation mode to extend its usability even beyond a week or so, after which the plumes typically have diluted to an extent that they don.t pose a threat to aviation anymore. In summary, the method could be used in real-time over the full period when volcanic plumes pose a hazard to aviation.

We added following:

After the plume has been detected and first data from satellites are available our method can provide the information needed in order to achieve the actual (and future) position/extend of the plume in real-time. This is done by passing emission profiles to forecast models as soon as possible after an eruption.

The impact of ash and clouds on the presented inversion method needs to be discussed in greater detail. Volcanic plumes in clear sky and ash free conditions are the exception. Can the utilized sensors even retrieve a SO_2 signal if cloud or ash particles are present? Do the SO_2 satellite retrieval include a (ash) cloud screening?

We did not have the opportunity to test our method on other cases, therefore we have no information on the matter how often the conditions would be sufficiently good in

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order to apply the method. This is something to be further investigated. However, as it could be shown that the results are not degraded significantly by using only a few hours of SEVIRI data, it appears likely that even with only some cloud gaps useful estimates of the source profile could be obtained. This necessitates the availability of the cloud mask, of course.

The sensors are capable of retrieving SO_2, also when as is present and dealing with clouds is implemented in the method.

If ash is present the horizontal plume position might still be retrievable from satellite. Does the inversion algorithm work if only the horizontal plume position is known but no information about the column concentrations is available?

We think it would still produce useful (but of course less accurate) results. A detailed answer to this question would rather be part of a follow-up study dealing with applications under more difficult conditions. The purpose of this paper is to introduce a new method for a case with very good conditions.

The presence of ice as common in many tropical eruption might mask any SO_2 or ash signal.

We are aware that this is a permanent issue of controversial discussion. However in our case, dealing mainly with SO_2 it will have only a small effect.

. it would be interesting to see the effect of moist convection on the inversion

We performed a sensitivity study where we compared the percentage of the particles in the stratosphere averaged over all time-steps for the simulation where the convection was switched on and for one where it was switched off. This should reveal if convection for this case played an important role. We found that 1% more particles were in the troposphere with convection switched on. Therefore, convection (at least in the model but likely also in reality) did not play an important role for this case.

To my knowledge the vertical velocity product in the ECMWF reanalysis data is very

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noisy in the UT/LS region which leads to spurious vertical transport and mixing. A solution to this problem can be the use of ECMWF forecast instead of reanalysis data or the use of a vertical coordinate based on potential temperature.

Vertical motions in the UTLS region can be inaccurate. However, the ECMWF (or similar weather forecast model) data are still the best information we have and they are fully consistent with the horizontal wind information. In principle, other methods could be used (e.g., vertical velocities could be calculated by running radiation codes offline and calculating heating rates) but we doubt that they can provide more accurate information. Potential temperature coordinates might be useful in principle but have problems in regions with potential instability. Furthermore, to our knowledge no operational models used at VAACs or elsewhere use potential temperature coordinates.

page 3764, line 22: define high vertical resolution, poor horizontal sampling

We specified the resolution in the text, such that it reads now:

The CALIOP (Cloud-Aerosol Lidar with Orthogonal Polarization) lidar on board of the CALIPSO (Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations) platform delivers global aerosol information. The vertical resolution, with 60m (Winker et al., 2007) is high, but the horizontal sampling is poor, as the satellite is in an orbit with a 16-day repeated cycle Vertical resolution is 30–300 m. Horizontal resolution is 330 m to 5 km.

page 3764, line 25: define some information on the vertical SO_2 distribution, very coarse (horizontal ?) resolution.

and for IASI as well:

IASI has a horizontal resolution of 12 km (pixel size) and the vertical resolution depends on the information content of the retrieval. For SO_2 it is about 1–2 km.

page 3766, lines 9-10: Why is the 10.8 and 12 km temperature difference (positive or negative?) indicative of clouds?

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See Prata (1989). Prata, A. J., 1989, Infrared radiative transfer calculations for volcanic ash clouds, *Geophys. Res. Lett.*, **16**(11), 1293–1296.

page 3766, lines 15-20: Why does a negative difference indicate ash? Why can a negative difference also be indicative of an overshooting plume? When do ice particle show up? Do they disappear later? What.s their (potential) impact on the retrieval?

See above. You can get a negative difference for an overshooting cloud because the lapse rate changes sign and this affects the brightness temperatures sensed between 10–12 μm. Ice occurs mostly in water-rich, tropical eruptions clouds. There may have been ice in this case, and this could have masked any ash signal. The ice can evaporate. It has little impact on the retrieval of SO_2 using either UV or infrared radiation.

page 3766, lines 21-22: If the plume overshoots the brightness temperature doesn.t provide an accurate height estimate when compared to the background temperature profile. Please discuss.

This depends on the amount of overshooting. The temperature profile of Fig. 2 suggests that an overshooting plume would have reached temperatures greater than 200 K. The accuracy of the brightness temperature as a measure of the cloud top temperature depends on the opacity of the cloud. If the observed brightness temperature was 200 K rather than 190 K then there would be an ambiguity in the height assessment.

page 3768, line 6: If the surface reflectivity needs to be taken into account how reliable are retrievals over land compared to those over water surfaces?

See Krotkov *et al.* (2006) or Yang *et al.* (2007). The latest retrieval scheme calculates surface reflectivity simultaneously. It is not believed that there is any difference in accuracy between retrievals over the land or the ocean. Some effects have been noticed over ice covered surfaces in TOMS retrievals.

page 3769, line 6: What is the motivation for the time averaging?

Data thinning is a commonly used method in data assimilation in order to reduce the

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correlation between individual data elements. Like most other methods, our method also assumes uncorrelated data, so the time averaging helps to reduce this problem.

page 3770, line 26: I assume it should read 532 nm not 53 nm.

Yes. 532 nm.

page 3771, line 25: Why does FLEXPART only produces output at 100km resolution horizontally? Satellites often have a much smaller pixel size.

This suffices to capture the broad form and the displacement of the cloud and thus serves the purpose. Smaller-scale features are probably produced by smaller-scale atmospheric phenomena which are not well resolved by the underlying 0.5-degree wind fields. Thus, working on a 1 degree scale is a good approach. Sensitivity studies with, e.g., 0.5 degree output may be included in future work to optimize the method.

page 3774, line 5: To what extent does the height sensitivity explain the differences in figure 3?

This is an interesting question, there is a strong temperature dependence (and therefore a height dependence as well) where the retrieval is sensitive to, but this is discussed in details in the papers Prata et al, 2007 (AIRS), Prata and Carn, 2007 (SEVIRI), Yang et al., 2007 (OMI).

page 3775, line 5: Is omega from the ECMWF reanalysis data used for vertical transport? - see also general comment above.

We are not working with reanalysis data but with operational analyses. Vertical motion from the ECMWF model is considered, though not through omega, as neither the ECMWF model nor FLEXPART uses pressure coordinates. The vertical winds used in FLEXPART are calculated from ECMWF spectral data based on the continuity equation.

page 3775, line 25: what is the horizontal area in which the tracer was released? Or is

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the tracer emitted from a point source in horizontal space?

We released the tracer from a vertical line (point in a horizontal plane) source. This is now mentioned in the paper.

page 3776, line 17: Regarding the m observations, are the observations for fixed in time or at multiple times? Are the observations vertical profiles as the source vector x or just column values?

As described on page 3780, li 19 we took hourly data from SEVIRI and one OMI as well as one AIRS observation was used. We added in the text, that the SEVIRI data was averaged from four observations every 15 min.

The observations are total columns retrieved from AIRS,OMI and SEVIRI.

page 3777, line 6: Does .prior values. in this context mean a first guess?

Yes, it does. As the sensitivity experiments show, it can be very coarse or even absent.

page 3780, line 26: Why should the time of the coldest cloud top temperature mark the end of the eruption? I would thing that the time of the coldest cloud top temperature only tells you that the eruption hasn.t finished yet and not that it has finished already. Thus, times after 13:00UT should be considered as release time as well.

Well, we don't say that it is the start or end time. We just say that it is the end of active development.

page 3782, lines13-14: plot AIRS and OMI results in same figure.

Figures 6 and 7 present the results of many different inversions. It is simply not possible to plot all of them together in one figure without compromising clarity. While it would make sense to plot AIRS and OMI results in one Figure, there are also good reasons for the arrangement and combination of results we have chosen for Figures 6 and 7.

page 3782, line 21: A 2K/day heating rate sounds quite large to me if maintained for *a couple of days. If the concept of potential temperature is used one can estimate the maximum lofting that a given heating rate can cause when ignoring all other processes.*

We performed a simple calculation taking the temperatures from the radiosonde at Abha, derived the potential temperature and further calculated the height difference of air with a potential temperature difference of 2K at different base height levels in order to estimate the lapse rate. At 8, 11, 14 and 16 km the lofting would be 1000, 680, 250 and 70 m respectively. These lofting rates are due to the steep increase of potential temperature in the stratosphere quite low (around 70m/day) above 16 km. As the main part of the plume is released at 16 km, this should not have a great effect.

page 3782, line 25: SEVIRI constant and zero don.t seem to differ much. Plotting only one of the two would increase the readability of figure 7.

Yes indeed, they are similar, but this is what we wanted to show!

page 3783, lines 4-5: Does fluctuations of total mass mean fluctuations in the initial total SO_2 as retrieved from the inversion method? If so then this sensitivity test doesn.t make much sense to me. The total initial SO_2 mass should be kept constant during the inversion for physical reasons even if the exact mass is unknown.

As shown in figure 4, the total mass as seen by the different sensors varies in time and between the sensors. We assume, that in reality the mass should stay the same during the first day, therefore we normalize the single observation in order to get a constant mass.

page 3784, lines 17-18: The paragraph is titled comparison with independent data. The FLEXPART inversion used for this comparison should not contain any OMI information at any stage.

For the inversion we used data only from the first 24 hours. As we compare the results only with data after this period, we consider these data as independent. Notice in

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particular that there are 24 hours between individual overpasses, so the degree of temporal correlation is very low. The data used for the inversion and used for the validation should thus be completely independent. Also notice that the results when not using OMI data for the inversion were very similar.

page 3784, lines 15-16: Are there any clouds present in the ECMWF data to confirm this claim?

Yes, ECMWF analyses show clouds in this region, we added this in the paper.

page 3784, lines 19-20: Are the OMI SO_2 column values near the detection limit? Paragraph 6.3: Since the authors compare admittedly apple and oranges in this paragraph what is gained by this comparison that has already been shown in the previous paragraph (if OMI is truly used as independent dataset as I suggested).

Yes we compare different substances measured, but the aerosols seen by CALIPSO are formed from the SO_2, which is explained in the paragraph. The comparison is important because we consider this time the height information of the plume, instead of the columns as in the comparison with OMI.

page 3786, line 3: How many hours after the eruption?

The CALIPSO data is captured at 22:00 on October 2nd, as the eruption took place on September 30th noon this is 58 hours after the eruption . we added it in the text.

page 3787, line 3: SOME MORE TEXT...?

We forgot to remove this remark. Thanks

page 3788, line 15: The aerosol seen by CALIPSO is stratospheric only for October 8th.

We changed stratospheric to upper troposphere/ lower stratosphere region, see also answer to reviewer 2.

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page 3788, line 24: The accuracy is not so great for lower altitudes.

We changed to: "...the emission altitudes of volcanic eruptions that reach the middle to upper troposphere and higher, can be estimated with great accuracy..."..

page 3789, lines 3-4: Even if the FLEXPART only needs a few seconds the inversion requires 15 to 20 hours worth of satellite data following the eruption making it less ideal for real-time applications. In addition, VAACs are mainly interested in ash, however, the authors explicitly choose a test case without ash signature.

See discussion below

page 3789, line 10: Even if the presented method could be applied to ash plumes somehow it doesn.t appear to be safe to fly below a predicted plume given the poor accuracy of the inversion for low altitude.

A detailed discussion of the accuracy with respect to VAAC applications is out of the scope of this paper. This is not a VAAC paper—we just point out that the method has a potential for VAAC applications, but of course this would require a detailed study of its own. However, it is up to the airline to determine where it flies. We can only say that we believe the plume resides at a certain altitude to some degree of accuracy. In the case of the Soufriere Hills, Montserrat eruption of 2006, all airlines chose to fly under the plume and did so safely and without incident. Some information helps.

page 3789, lines 12-18: I doubt that ash plume applications are as easy as suggested. Gravitational settling critically depends on particle size, shape and surface properties that (at least currently) cannot be derived from satellites. Ash signatures are often masked by ice signatures. Column ash values are difficult to retrieve since they require assumption about the particle size distribution.

See above.

page 3790, line 4: Even with a radiative transfer scheme the precise knowledge of other absorbers and scatterers in the atmosphere would still be crucial for the quality

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of the retrievals.

That isn't true. For SEVIRI and MODIS it would be good to know the water vapour amount, but for all sensors CO_2 and nearly all other trace gases (except for O_3 and the UV sensors) there is virtually no impact on the quality of the retrievals.

page 3790, line 6: Errors in the underlying wind fields or their prediction could be addressed by performing FLEXPART simulations based on ECMWF forecast data instead of reanalysis data.

As already said, our simulations are based operational analyses and not reanalyses. Forecast data are not a good tool to assess uncertainties in the analyses of meteorological fields as differences will mainly reflect the forecast error which is an amplification of the errors in the underlying analysis used as initial condition, which is both in its spatial structure and position and in its magnitude really different from the analysis error.

figure 6 and 7: Too many lines are overlapping, lines can hardly be distinguished from each other. I would recommend to split them into several plots for clarity.

Yes we admit there are many lines on these figures, but as they show similar patterns we think it is ok to leave them.

figure 8: This figure is too small, color shading and isolines are very difficult to compare quantitatively. It would be useful to have different plots for OMI and FLEXPART column values using identical shading. The continental outlines in red are more confusing then helpful

The main aim of this figure is to show the geographical match between OMI and the FLEXPART simulation. We admit that the figure is difficult to read in this size, we will ask for a increased size in the final publication.

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