Atmos. Chem. Phys. Discuss., 8, S2048–S2053, 2008
www.atmos-chem-phys-discuss.net/8/S2048/2008/

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 #1

Received and published: 27 April 2008

General comments:

The manuscript by S. Eckhardt et al. presents an interesting topic and is overall well written. I recommend the publication of the manuscript after addressing my general and specific comments below.

The real-time application of the presented method to ash plumes in VAACs doesn't appear to be realistic at least at the moment. SO2 is not a simple substitute for ash. More discussion is necessary.

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.

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 SO2 signal if cloud or ash particles are present? Do the SO2 satellite retrieval include a (ash) cloud screening? 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? The presence of ice as common in many tropical eruption might mask any SO2 or ash signal.

With respect to the model FLEXPART the impact of vertical transport on the inversion result should be discussed. A parameterization of moist convection is included, however, its effect needs to be estimated based sub-grid information that is not available. Given the location of Jebel at Tair and the atmospheric condition during the eruption moist convection is probably not a dominant factor in the inversion, nevertheless, it would be interesting to see the effect of moist convection on the inversion. To my knowledge the vertical velocity product in the ECMWF reanalysis data is very 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.

The presentation of figures 6 to 8 needs to be improved - see details below.

Specific comments:

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

page 3764, line 25: define some information on the vertical SO2 distribution, very coarse (horizontal ?) resolution

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

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?

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.

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?

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

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

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

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

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

page 3775, line 25: what is the horizontal area in which the tracer was released? Or is the tracer emitted from a point source in horizontal space?

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?

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

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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.

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

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.

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.

page 3783, lines 4-5: Does fluctuations of total mass mean fluctuations in the initial total SO2 as retrieved from the inversion method? If so then this sensitivity test doesn't make much sense to me. The total initial SO2 mass should be kept constant during the inversion for physical reasons even if the exact mass is unknown.

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.

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

page 3784, lines 19-20: Are the OMI SO2 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).

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

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

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

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

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.

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.

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.

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 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.

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.

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

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helpful.

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

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