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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The vertical distribution of volcanic SO_2 plumes measured by IASI" *by* E. Carboni et al.

Anonymous Referee #1

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General Comments

In this new study Carboni et al. applied an SO2 retrieval scheme for IASI introduced in a paper in 2012 to fourteen minor and major volcanic eruptions in 2008 to 2012. Column density maps illustrate the horizontal distribution and altitude-dependent time series the vertical distribution of the volcanic SO2 emissions. Volcanic plume altitudes and SO2 total mass estimates derived from the retrievals are important pieces of information, in particular for modelling studies. Plume heights are validated with CALIOP satellite measurements and SO2 total mass is validated with data from ground-based Brewer instruments for selected case studies.

The paper presents an interesting topic and is within the scope of ACP. The presenta-

tion is generally clear, but I found that a number of specific questions and issues remains open. I would recommend the paper to be published once the specific comments listed below are carefully addressed. Also, as a general comment, I got the impression that some time should be spent on improving the textual flow of the manuscript and language editing to improve readability.

Specific Comments

short title: Just wondering if the short title of the paper could be replaced by "SO2 vertical distribution of volcanic plumes" to make it more specific?

p24646, l19-23: Here it is mentioned that nadir spectrometer measurements can be used to infer information on SO2 plume altitude, but no details are given. How good does this work? Can you provide references?

p24648, l9-10: Hilton et al. (2012, Fig. 3) show that the IASI nominal and actual NeDT may be as large as 0.3-0.4K in the spectral range from 650-1750/cm. The range of 0.1-0.2K mentioned in your paper may be applicable for the 7.3 and 8.7 micron wavebands of SO2?

p24651, l16-19: It is mentioned that SEVIRI imagery is used to identify volcanic plumes in the CALIOP data. I was wondering what CALIOP and SEVIRI are actually sensitive to? I guess CALIOP is sensitive to sulphate aerosols (rather than SO2)? Please clarify also for SEVIRI.

p24652, l24-28: I have trouble seeing this separation of the plume as there are a number of features visible in the plots. Perhaps this could be made more clear by adding arrows or other markers in the plots?

p24653, l8-12: Here it is mentioned that the Brewer SO2 measurements are sensitive to both anthropogenic SO2 in the lower troposphere as well as overpasses of volcanic plumes. Is your IASI SO2 retrieval scheme also sensitive to the lower troposphere? I was thinking that IR nadir sounders are most sensitive to SO2 in the mid/upper troposphere and lower stratosphere. Assuming that there are some differences in vertical sensitivity of the different measurements (Brewer versus IR nadir sounder), do these pose limits to this comparison?

p24654, l16-19: What is the reference for the 6 m/s average wind speed used to calculate the influence radius? This value looks a bit small. How do the correlations between the Brewer instruments and IASI (Fig. 5) change if the influence radius is increased?

p24655, l23-27: Triangular interpolation is used to fill data gaps, but what happens if IASI measurement tracks are overlapping at high latitudes? How do you consider the different measurement times of the satellite orbits?

p24656, l20-23: How do these SO2 total mass estimates agree with other studies? What are the uncertainties of these estimates? From the data presented in Fig. 7 it might be possible to estimate SO2 lifetimes, which would be very interesting for modellers, I think.

p24657, l12-14: The WMO definition of the tropopause does not depend on the pressure profile. Do you mean you used log-pressure altitudes calculated from ECMWF pressure/sigma levels to estimate the tropopause altitudes rather than considering geopotential heights?

p24658, l4-6: It might be good to recap the meaning of different VEI values at this point. Which plume altitudes are expected/found for a VEI of 1-5? Is stratospheric injection for the different VEI values considered to be likely or not?

p24658, l7-10: At this point it is likely not clear to the reader what you mean by "dynamic effect". The explanation at this point seems a bit short and vague.

p24659, l11-21: This discussion is a bit long, but I took as a key point that it is very important to have good plume altitude information for the SO2 retrievals. Wrong plume altitudes may lead to significant differences in SO2 total mass estimates. Perhaps you could add a sentence at the end of this paragraph to conclude and stress this point, as

it provides strong motivation for this work?

p24662, l25-29: It would be interesting to see how you rate your findings regarding the discussion of the transport of Nabro SO2 emissions in terms of the Asian Monsoon circulation or direct injection. Fig. 11 shows that most SO2 is located below the tropopause, i.e., it might not be a clear case of direct injection into the stratosphere as suggested by Fromm et al.?

p24664, l25-27: Stating that IASI is "consistent" with CALIPSO and the Brewers instruments is good, but I think you should try to be more precise. How good/uncertain are the plume altitudes and total mass estimates from your retrieval scheme?

p24665, l7-11: The conclusion that your paper demonstrated that the VEI "is a poor index of the potential height to which volcanic SO2 is injected" is not evident to me. Perhaps you could add a table or a scatter plot showing the VEI and plume heights for the different eruptions to demonstrate that there is no good correlation?

p24665, l12-15: Is it to be expected that many volcanic SO2 plumes reach a level near the tropopause? What would be the physical mechanism for this?

Fig. 3: It seems the CALIPSO measurement tracks used for comparison were not well chosen as they have only limited overlap with the SO2 plume (as shown by IASI)? Perhaps CALIPSO tracks located more to the west would have been better? What was the rationale for your choice?

Fig. 4: Some data points from the SO2 retrieval seem to have very large uncertainties in plume altitude (up to +/- 8 km). I guess these large uncertainties are related some kind of retrieval problem? I wanted to suggest to remove these points from Fig. 4 (as well as Figs. 1-3) as they do not seem to tell us a lot? The comparison with CALIOP seems to be meaningful only if the SO2 retrieval delivers a plume altitude with reasonable accuracy (e.g. with an uncertainty less than +/- 2 km or similar).

Fig. 6: Does gray color indicate that the retrieval failed because of an SO2 column

density larger than 100 DU? It seems there are a number of data gaps (white color) in the SO2 column density and plume height maps near 70°W and 45°N, which are filled by zero rather than interpolation from neighbouring pixels? Zooming in on the plume could help to check this.

Fig. 7: This figure might be a bit confusing as the emissions of some volcanic eruptions are overlapping in time (e.g. Grimsvötn, Puyehue, and Nabro in May and June 2011), but are plotted as separate events here. Full time ranges including actual days are given for some eruptions in the plot key (which seems helpful), but are missing for others. What defines the time span of data points shown for each volcano in this plot? For instance, the time span for the Puyehue is much longer than for the Nabro (whereas the total SO2 mass is much lower for the Puyehue than for the Nabro)?

Figs. 8-11: The SO2 column density maps are all limited to a maximum value of 5 DU, which seems pretty low compared to actual maximum values that occurred in the case studies. Different color bar ranges for each plot or a log-scale with fixed range for all plots may help to retain this information. The plots of the vertical distribution present the total mass of SO2 (in Tg). However, the total mass in each box will depend on the vertical and temporal binning. The box sizes (in particular the vertical binning) should be mentioned in the caption. Alternatively, SO2 density (Tg/km/day) rather than total mass could be shown.

Fig. 10: The tropopause data for the Puyehue case study (bottom row) shows very large fluctuations and has data gaps. Is this considered to be realistic? Are there any problems with the estimation of the WMO tropopause height in this case study? (Very large fluctuation of the tropopause height is also visible for the Eyjafjallajokull case study in Fig. 8.)

Technical Corrections

p24644, l5: "Instrument" -> "Interferometer"

p24646, l8: "observe into" -> "observe in" (?)

p24648, l3: "2007" -> "October 2006"

p24648, l21: "1 C" -> "1C"

p24649, l19: "is it" -> "it is"

p24653, l13: "Metop" -> "METOP" (also in other places of the paper)

p24655, l7: remove "instrument" (?)

p24655, l22: "into a 0.125" -> "into 0.125"

p24656, l17: "other, it" -> "other. It"

p24658, l11: "of a multilayer" -> "of multilayer"

p24662, l29: "Caliop" -> "CALIOP"

p24665, l4: "lidar or limb Michelson... (MIPAS) measurements" -> "lidar or limb measurements (e.g., MIPAS)" (?)

Figs. 1-4: The plot titles of the CALIPSO plots (e.g., "CAL_LID_L1-ValStage1-V3-01.2010...") could be replaced by more readable style.

Section 6.4.1 should be Section 6.5, I guess?

References

Hilton, F., R. Armante, T. August et al., Hyperspectral earth observation from IASI: Five years of accomplishments. Bulletin of the American Meteorological Society, 93, 347–370, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24643, 2015.