**Atmospheric
Chemistry
and Physics
Discussions**

Interactive comment on "Large scale modeling of the transport, the chemical transformation and the mass budget of the sulfur emitted during the eruption of April 2007 by the Piton de la Fournaise" *by* P. Tulet and N. Villeneuve

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We want to thank the reviewer for all comments, which have helped us improve the paper. During the correction step, we have further examined the entire chain of interpolation of the OMI data base in order to answer to your comment 7. We found an error in our daily SO2 mass calculation. This error only affects TRL level results. Thus we obtained an additional time to reprocess simulations which include corrected TRL

results. In this new version of the manuscript comprises corrections in the mass calculations and new simulations. We have also done our best to improve the English of the text.

1 General remark

This paper reports a combined satellite observation and modeling study of sulfur dioxide (SO2) emissions associated with the April 2007 eruption of Piton de la Fournaise volcano, Réunion. This was a significant eruption and the SO2 emissions were the highest measured during any recent eruption of the volcano. A major problem with the manuscript as it stands is the quality of the English. The paper needs a thorough editing job to correct this, as there are errors in almost every sentence. I do greatly sympathize with non-Anglophone authors on this matter but the language does impact the clarity of the paper in numerous places. I think it would be more efficient and convenient for reviewers if such editing could be done prior to manuscript acceptance by ACPD?

We apologize for the difficulties to read the paper. We are seriously confused. The new version of this manuscript has been read and corrected by a native English speaker.

2 Comment 1

In the introductory section on page 21360, the authors should cite the following paper, which appears highly relevant to this work: Bhugwant et al. (2009), Atmospheric

sulfur dioxide measurements during the 2005 and 2007 eruptions of the Piton de La Fournaise volcano: Implications for human health and environmental changes, Journal of Volcanology and Geothermal Research, 184(1- 2), 208-224. OMI SO2 observations of this eruption (with preliminary SO2 mass estimates) were also posted on this website:
http://so2.umbc.edu/omi/pix/special/2007/piton/piton04.php

We added Bhugwant et al.(2009) as a reference in the text. We also add the reference of Staudacher et al., 2009 who used the same dataset. Finally we also referred two internet address
(http://so2.umbc.edu/omi/pix/special/2007/piton/piton04.php and
http://sacs.aeronomie.be) where SO2 mass estimations have been published on-line during and just after the April 2007 event using OMI, SCIAMACHY and GOME2 sensors.

3 Comment 2

2. On page 21362, "total column" is a better term for the OMI measurements than "integrated profile".
Also, if the authors are using the operational OMSO2 v003 products in their analysis, then the following algorithm paper should be cited (rather than Yang et al. (2009)): Yang, K., N.A. Krotkov, A.J. Krueger, S.A. Carn, P.K. Bhartia, and P.F. Levelt (2007). Retrieval of large volcanic SO2 columns from the Aura Ozone Monitoring Instrument (OMI): comparison and limitations. J. Geophys. Res., 112, D24S43, doi:10.1029/2007JD008825. The authors should also explicitly state whether they are using the operational 'linear fit' algorithm SO2 columns, or the SO2 columns produced

by the 'band residual difference' (BRD) algorithm.

We have change "integrated profile" in "total column" in the text.
For OMI V3 data products, the data user guide indicates that the SO2 columns in the Planetary Boundary Layer (PBL) are produced using the Band Residual Difference (BRD) algorithm [Krotkov et al 2006]. The 3 others estimates of the SO2 column, are produced using the Linear Fit algorithm of Yang et al [2007]. It is the reason why we consider this algorithm. In addition, N. Krotkov confirmed us that this choice was correct. We've added a paragraph to clarify, in the new version of the paper.

4 Comment 3

On page 21363, the discussion of the effects of clouds on the satellite SO2 measurements should be clarified somewhat. Clouds can indeed obscure SO2 located beneath clouds, but if the SO2 is located above a cloud layer then SO2 column amounts can be overestimated due to the increased reflectivity and multiple scattering effects. The statement regarding negative SO2 values is also incorrect. Such values are not usually a "problem with the measurements" (line 14, p 21363) but a natural consequence of random measurement noise in regions of zero SO2. In such cases it is true that negative SO2 columns imply low (or zero) SO2 amounts. Larger negative biases in the OMI SO2 measurements can be associated with deep convective clouds, however.

We are agree with you. The explanations on negative values and their origins are imprecise. The malformed sentence has been deleted. Considering the clouds contribution, our point of view are the following:

- The satellite cannot retrieve the SO2 which is located below the clouds layers.

- When the SO2 plume crosses the cloud, it crosses a wet environment. In this context, due to the wet deposition of SO2 and the chemical transformation into H2SO4, the SO2 life time is short.
  It explains the discontinuity of the SO2 plume, marked by local minima of the burden mass observed by OMI.

- Overestimation of SO2 due to the reflectance: this is true, but we did not found any strong burden mass above clouds in the OMI data.

So we re-wrote the sentence as "the SO2 retrieved in cloudy areas is not considered here as a physical value". We have also added a sentence to take into account your remark about cloud albedo.

5 Comment 4

Line 2-3, p. 21364: "Maurice Island" = Mauritius? Also 'alizes' is (I think) the French term for "trade winds" – the latter should be used for clarity.

Thanks, these terms have been corrected in the new version of the paper.

6 Comment 5

The CALIPSO section (3.2) beginning on page 21364 needs some embellishment.

The authors need to show at least one example of a CALIPSO lidar profile showing the volcanic plume from Piton de la Fournaise. Finding volcanic features in the CALIPSO data can be non-trivial and I would like to see what criteria are being used to distinguish the volcanic aerosol from other aerosol and clouds. I had a quick look at some CALIPSO profiles for this eruption and it was not clear what features the authors were using to plot the data in Figure 3.

We added a specific figure to illustrate CALIPSO.
We also added a paragraph which clearly explains what level of data products, we use. On NASA web page, series of processed data are available. The altitude of aerosols of the observed plume are directly deduced from "532 nm Total Attenuated Backscattered" images and "Aerosol Subtype" products.

7 Comment 6

The method used to estimate the daily SO2 emission (section 4.3) could be described more clearly. I think that the daily OMI SO2 columns over the eruption site have been converted into a vertical profile of SO2 mixing ratio, which is then used in the model simulations? But the authors report the contrary. Is this an error?

This is true for all days except April6. For all days except the April 6, we use: (i) the mass burden observed by OMI over La Reunion; (ii) the form of the plume given by the model, and the aerosol plume levels given by CALIPSO, to estimate the vertical boundaries of the SO2 plume.
The April 6, the important variability of the SO2 emission and the presence of clouds over La Reunion, require us to use other parameters. (i) The concentration of SO2 introduced in the simulation on April 6, is deduced in oder to reproduce the total mass

burden observed by OMI on April 7.(ii) Also comparisons of plumes between the simulation and the OMI observations on April 7 (local maxima and shape) are use to update the vertical boundaries of the SO2 column over La Réunion. (iii) The temporal evolution of the SO2 column are deduced from the evolution of seismicity which is partially correlated with the lava flow.

These estimations of the SO2 vertical profile have required a large number of simulation test before reproducing the plume as well as the total mass observed during the main eruptive period.

The section have been clarified in the new version.

8   Comment 7

Section 6.1 (mass budget), p. 21370 (also Figure 7). This section needs some clarification. Were the negative OMI SO2 pixel values included in the integrated mass calculation? Also, under normal circumstances the OMI lower tropospheric (TRL) SO2 retrievals should produce larger SO2 amounts than the mid-tropospheric (TRM) retrievals, but the authors report the contrary. Is this an error?

First, how we quantify the SO2 in the OMI dataset :
An interpolation of the OMI data have been made on a regular grid of 24 km x 13 km, over the studied domain. We exclude a buffer of + 0.7 DU for TRL level , and +0.3 DU for TRM level. These values correspond to the background noises (OMI user data user guide). Then we don't consider in our measurement negatives values. These explanations have been introduced in the new version of the paper.

Second, to answer to the question about TRL vs TRM:
Thanks to your comment, we have spotted a major error in our data extraction subrou-

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tine. The column supposed to be the TRL one was in reality the STL (Stratospheric CMA). This explain the problem you noticed.

We have completely corrected the chain of treatment of the OMI data, and we found the TRL values noticeably stronger than before. Considering that the correction on the data, have modified our estimation of SO2 release; all the simulations have been reprocessed, and updated the Figures included in manuscript.

9   Comment 9

9. p. 21372, line 6-7: again, see comment #3 above regarding the effects of clouds on the satellite measurements.

We have rephrased the sentence as: "First, clouds prevent the correct observation of SO$_2$: in cloudy regions, the mass observed by OMI is globally underestimated in agreement with Carn et al., [2008]."

10   Comment 10

p. 21373 (section 6.2): the authors need to provide some error bars on their SO2 mass estimates. Given the various uncertainties involved (e.g., plume altitude) it is unacceptable to give a total SO2 emission of "156.7 kt" without some indication of the error margin.

We agree that values with four significant digits are too precise in regards to all uncertainties of input data and model errors. But here we only give precise values for

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the analytic computation by the model. Nevertheless, in the new version, we do not give digit after the decimal point.

It is not realistic to integrate all the possible source of errors and give an error margins. We think, that the correct SO2 estimate is probably between TRM and TRL. One focus of the paper, is to try to refine this estimate.

Thus, we indicate in the text that values extracted from the simulations need to be considered as an order of magnitude.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C14127/2011/acpd-10-C14127-2011-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 10, 21357, 2010.

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