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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

Anonymous Referee #2

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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

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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?

Other remarks:

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

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.

3. On page 21363, the discussion of the effects of clouds on the satellite SO2 measure-

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ments 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.

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.

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.

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?

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?

8. p. 21371-21372: there is a discussion here regarding the more rapid fall-off in

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SO2 burden measured by OMI compared to the model simulations. In addition to the factors mentioned by the authors, this is also an expected consequence of SO2 dispersion over time. SO2 column amounts will decrease below the OMI detection limits, particularly at the fringes of the volcanic cloud, and hence less SO2 will be measured from space, even though SO2 is still present. The model does not suffer from this finite sensitivity.

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

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.

11. Figure 2: the color bar needs labeling.

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