

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.

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