

***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 #1

Received and published: 11 November 2010

This paper gives a detailed analysis of the April 2007 eruption of Piton de la Fournaise. The authors use a combination of satellite remote sensing techniques and a mesoscale chemistry model. The results are interesting and the paper will be a useful reference in the history of Piton de la Fournaise, but also in the study of other similar eruptions. I also think the figures are very nicely done. I have two main comments:

1/ The first comment is of a technical nature, but I put it first as it is important. Currently

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the paper is very hard to read due to numerous grammar and spelling mistakes. The first sentence of the abstract is quite representative "...entered in its bigger eruption registered at least one century". This simply doesn't make sense (although of course with effort one can understand what the authors are trying to say). I will not try and list all the errors (almost every sentence has one mistake), but as it is, this paper needs a complete and thorough revision by a native speaker prior to publication.

2/ The estimation of the day by day SO₂ release (section 4.3) needs to be much better explained. As I understand it now, the total mass burden is estimated from OMI on a day to day basis. This is then converted into an average emission rate of SO₂ injected inserted between certain CALIPSO-derived altitudes. This is obviously a basic approach (the authors should compare their approach with <http://www.atmos-chem-phys.org/8/3881/2008/acp-8-3881-2008.html>), with many limitations. Like it does not take into account the lifetime of SO₂ (when OMI observes the plume, some SO₂ will already be deposited/converted). Also, there is the problem of observing an aged plume. E.g. on 9 April, OMI observes an aged plume, and this cannot be used to estimate the emission on 9 April. Also the sensitivity of OMI to boundary layer SO₂ is an issue. As far as I know, OMI has a reduced sensitivity near the surface. Linked with this section is also the discussion in section 6.1. Since the observations have been used to determine the emission rates, there should be a perfect agreement in Fig 7, no? I suggest expanding section 4.3 and 6.1, explaining better the methodology and its caveats.

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

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