Answers to review 1

February 25, 2011

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 the comment 7 of the reviewer 2. 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 comment 1

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

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 2

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 in-jected inserted between certain CALIPSO-derived altitudes.

The estimation of the vertical profile of SO2 has not been directly derived from the total mass burden (OMI) and from the vertical level of the aerosol plume (CALIPSO) for several reasons.
(1) The total mass burden observed by OMI, that is integrated over the studied domain, is the result from different integrative processes such as transport, convection, chemical transformation, dry and wet deposition and export out of the domain boundaries. We do not know if there is one method able to inverse all these processes to estimate the emission. For example, a 3D-4D variational technique could done it, but to our knowledge, there is no mesoscale chemical model which include its adjoint.
(2) Unfortunately, due to the cloud cover, the satellite observation by OMI (and callipso) did not give valid data over la Reunion area the April 6. It is a important problem, because it corresponds to the main eruptive period, where the lava flux and the SO2 release is the most important. If we use the total mass burden observed on April 6, the error in the SO2 emission would be approximately 100 %
(3) The strong temporal variability of the SO2 flux on April 6, cannot be deduced from one daily satellite observation.
For these reasons, the estimation of the vertical profile of SO2 are the result of a day by day analysis using different parameters. We have clarified it in the new section 4.3.
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 Runion. (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.

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).

Thank you for this remark and reference.
Eskhardt et al., 2008 have developed a sophisticated inversion method based on flexpart model coupled with ecmwf analysis and observations done with several satellite instruments.
This paper and the methodology are really interesting and adapted in volcanic plumes forecasting. As you said, there are some important limitations in this method. Eskhardt et al., 2008 considers the SO2 as a passive scalar (no chemical oxidation, no deposition,...). One of our major contributions in our paper is to highlight the error in term of SO2 mass budget when the simulation considers the SO2 as a passive scalar. Furthermore, we are no sure that applying this method in our case gives good results for three main reasons:
(1) The strong SO2 flux variations during the April 6, which had generate strong SO2 gradient in the plume. This cannot be observed with only one measurement a day.
(2) The accuracy of satellite quantification is not completely reliable. This is due to perturbations by clouds (largely present in this tropical region). Also, the Geometrical properties of the footprint size (which varies with distance from the nadir) may also influence the accuracy of the measurements.
(3) The SO2 concentrations in the plume are also largely modulated by precipitations that scavenged SO2 (and increases the transformation into H2SO4).
We added in the text this reference and explain our choice to use a different way.

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 emis-

sion on 9 April.

As we explained before, we did not use the total mass burden, to estimate the vertical column of SO2 in the same day.
For estimations we used the mass burden observed by OMI localized over the vent, associated with the plume shape modeled in the vicinity of La Reunion. In Table 1, the daily mass burden over La Reunion is about 3 DU on April 9 at 11 UTC. We use this integrative value for the vertical profile of SO2 in the model. After April 7 the SO2 retrieval by OMI is low compare with the main eruptive period (i.e. 5-7 April). As a consequences, the estimation of the SO2 emission days after the April 7, do not contribute significantly in the total emission budget.

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.

This is true that OMI has a reduced sensitivity near the surface. We added a sentence to take caution with OMI data during the two first days when plume is located in the trade winds layer.
It is important to note that the period when the plume is in lower troposphere and the period when SO2 emissions are low.
We are not sure to understand completely the second part of your remark. We only use the total mass burden from April 7 (Fig 8), in order to estimate the emission on April 6. There is no 3D atmospheric models able to reproduces perfectly all the dynamical transport and chemical processes encountered during this event. The period when the model is close to the total mass observed by OMI (fig 8), corresponds to the period of emission increases (until April 7), when the plume is relatively young.