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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First results of the Piton de la Fournaise STRAP 2015 experiment: multidisciplinary tracking of a volcanic gas and aerosol plume" *by* Pierre Tulet et al.

Anonymous Referee #1

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First results of the Piton de la Fournaise STRAP 2015 experiment: multidisciplinary tracking of a volcanic gas and aerosol plume.

P. Tulet, A. Di Muro, A. Colomb, et al.

This paper present a multidisciplinary approach aiming to track changes during volcanic plume dispersal downwind. The approach combines gas flux and composition measurements, aerosols assessment and volcanic plume dispersal modeling. This combined investigations covered four eruptive periods of Piton de la Fournaise and emphasized existent of a stronger interaction of weak eruptive plume with the island surface, high SO2 and particle concentrations are regularly measured to a distance of 20 km from the source. The study further indicates a predominance of a rather small

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particles in the plume in relation to a strong nucleation of sulfuric acid at around 40 km from the source downwind.

This multidisciplinary approach is a step forward to better constrain the physical and geochemical interactions between volcanic plumes and the troposphere. I would criticize the gas composition and fluxes results which appear to me as the weakest part of this work. But the paper is well written and methods are robust and complementary, thus suitable for publication in ACP after considering the few questions and comments below.

1/ Main remarks

This paper strongly emphasized the gas composition and flux measurements but results presented somewhat fail to fulfill expectations:

a) portable DOAS measurement is reported in the paper (L31 p4, L19 p7, L6 p9) but no corresponding result presented. Why? Figure 7 even present a transect across the plume. What is the corresponding SO2 flux? Table 1 indicates 6 series of DOAS measurements but why no preliminary results presented ?

b) August 2015 eruption is described as following 3 different phases, based on results from DOAS stations (L2-6 p10). But the uncertainties associated to these results are significant (Fig.6). - The eruption phase 1 described as associated to a progressive SO2 flux decreasing trend from 24/08 to 12/09 (L4 p10) is not convincing - this tendency is not clearly decreasing (Fig.6). These gas flux results (Fig.5 and Fig.5) will gain more strength if portable DOAS results are associated. - An “accelerating increase of SO2 flux between 13/09 and 18/10” is somewhat exaggerating. According to Fig.6 and accelerating tendency rather commenced in early October. Figure 6 indicates at least two strong degassing phases: from end august to mid-september and from early October to mid-October. Vigorous intermittent SO2 discharges were recorded between and after these two strong degassing phases.

c) MultiGAS measurements is outline several time in the paper (L32 p8, L33 p10, L1 p11, L6 p11, L13 p11, L31 p17,. . .) and table indicates a total of around 8h of recording from May to October 2015. But curiously only 2 ratios are provided : H2O/CO2 = 50-240 (L12 p11) and CO2/SO2 <0.6 (L13 p 11). - It is well known that H2O and even CO2 are not easily measured in the plume. What is the error of this ratios ? A figure of the plots should be very informative. - Figure 7 gives concentration results which are not exploitable. The behavior of H2O, CO2 and SO2 are totally different which may suggest no common source, that is surprising given that some of the measurement are performed close to the vent. - Should we understand that H2O/CO2 and CO2/SO2 ratios are unchanged over the eruptive period ? That would be very surprising given the dynamic of the eruptive activity. Authors should add more results of multiGAS measurements and check the ratio changes which might describe better the eruption dynamic than the SO2 flux from the stationary DOAS. - L32 p 4 indicates H2S was also measured. But curiously no result mentioned this gas. Is this suggest no H2S in the system ? That would be very surprising.

2/ Minor remarks - L25 p4: accumulation chamber for CO2 soil flux. Is this instrument deployed ? Not referring to in the rest of the paper. Add reference if developed elsewhere. - L21 p6, delete 2 after August. - L21 p6, the date format is e.g., 2 August 2015 whilst L22 p6 the format is e.g., August 24, 2015. Harmonize date format throughout the paper. L21 p6, to the south-southeast ?. . .to the north ? L22 p6, to the west-southwest? What are these direction referred to ? L31-32 the output budget ? Not calculated in the paper, why? Do add reference if done elsewhere. L12 p9, DOAS sessions are acquired with a high rate – what does it mean by high rate ? L25-27 p9, 1870 t/d and 1840 t/d is that same if taking into account the errors. Thus not so sure that highest SO2 emission rate was observed on 20 May – maybe tone down this comparison. L31 p9, May SO2 fluxes are not in fig.6, but fig.5 – do modify the sentence. L35 p9, add reference to the estimated 24-37 m3/s, or give further details if calculated in this work.

L4-5 p10 these phases are not convincing – modify to be coherent at least with Fig.6 L12 p10, change (<300 and 600 ton day-1, respectively) to (<300-600 ton day-1) L31 p10 change regular survey and visible and thermal imagery to visible and thermal imagery surveys... ? L32 mentions permitted to follow the time and space evolution of eruptive dynamics and to constrain the evolution of the degassing source – What are the thermal observation results? what can be said about the eruptive dynamics ? What about the evolution of the degassing source ?

L20 p14, change tranformations to transformations

Figure 7. The multiGAS panel is not contributing anything. Worse it presents incomplete (no H2S) and bad (no correlation) results. Add the SO2 flux on the DOAS panel. The changes in thermal images is not described at all, even in the text? Can you say anything ?

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