

# Review of Moussalam et al., Characterisation of the magmatic signature in gas emissions from Turrialba volcano, Costa Rica

## General Comment and Recommendation

The manuscripts present recent measurements of the composition and flux of gases emitted by Turrialba Volcano (Costa Rica). These data are interesting because they complete and update a few previous studies that have documented important changes in these parameters and have all recommended to carry on with the geochemical monitoring of this volcano. Although the data partially overlap with another recent study (Conde et al., 2014) reporting measurements over roughly the same data period, I feel there is not much overlap since the focus of the studies is really different (CO<sub>2</sub> output and instrumental comparison for Conde et al., chemical volcanology and volcano monitoring for this study). The discussion that supports one of the main conclusions of the paper (the further drying out of the hydrothermal system) seems solid. However the SO<sub>2</sub> flux data might be significantly underestimated and lead to erroneous conclusion about the origin of the May 2013 event. I urge the author to improve the description of the methodology (see specific comment), possibly revise it (as it is currently described it is not possible to judge), and reexamine the related conclusion. In summary, the manuscript is an interesting contribution that deserves to be published in Solid Earth pending some moderate to major revisions.

## Specific Comments

Gonzalez et al (2014) is not in the reference list

**DOAS retrievals:** The DOAS retrieval is not described with enough precision. The least should be to describe the wavelength window for the DOAS fit. This is important since it can have important implications on how the radiative transfer issues may affect the retrievals.

**SO<sub>2</sub> flux results:** I'm really surprised by these low fluxes. Are you sure these are real? The measurement geometry that you report (horizontal scanning of a very dense plume) is arguably the worst in term of the accuracy of the result, because of the radiative transfer issues (e.g. Kern et al 2010, 2012). Given the distance, and the high SO<sub>2</sub> slant column amounts reported in your FTIR measurements, your flux could easily underestimate the "real" value by a factor of 2 to 3, even in absence of clouds. Without the necessary information on the spectral window used for the DOAS retrievals (see my previous comment) the readers cannot judge accuracy of these value. You should try to perform DOAS retrievals at longer wavelength (315-330 nm) or 370-380 nm, as suggested by Bobrowski et al., (2010) for very concentrated plume? In March of this year, I did some SO<sub>2</sub> flux measurements at Turrialba from the finca La Central (2.1 km from the crater) with a UV camera. Prior to being corrected for atmospheric scattering, the results were around 4 kg/s. And after applying an appropriate correction, the new value was about 7 kg/s. So please be careful and check the retrievals

**Gas composition discussion discussion:** I agree that the most likely cause of the discrepancy between the C/S ratio of the 2010 and 2012 vent is the high retrieval error on the FTIR measurements of the latter. The intercept concentration of CO<sub>2</sub> is rather low compared to the 400ppm of a standard atmosphere. What are the results if you force both regression lines to pass through this value?

**Discussion-Current stage of degassing:** What are the factors that may cause the reduction of permeability in a high temperature volcano-hydrothermal system ? Self-sealing of the fractures by hydrothermal deposits? My intuition is that it should be a slow process. So I would consider the possibility that the apparent decrease of SO<sub>2</sub> flux in the 1 week time lapse between the measurements of (Conde et al., 2014) and yours may be an retrievals issue (see my previous comments)

Table 1: some species (H<sub>2</sub>O, CO<sub>2</sub> and CO) appear several times (once in the “target species” list and twice in the “other species included”). Is this a typo or does it mean you fit them both in the “volcanic gas” layer and in the “atmospheric” layer? to take into account the change in shape of their absorption spectra with temperature?

Figure 2d is too dark to be readable

Please specify in the legend of Figure 4 that these are Multigas measurements (it is not so obvious)

Fig 6: How can you report values of C/S ratio *prior to* the date of first detection of SO<sub>2</sub> in fumaroles? Please revise the literature and be consistent.

I would include an additional synthetic figure showing the set-up conditions of the DOAS, a scan through the plume and a correlogram used to derive the wind speed.