

Dear authors

Thank you for the reply. The replies and the additional description clarify some points. However, the following three major comments are not fully clarified yet and further revision is suggested;

- 1) Misleading estimate of the oxygen fugacity,
- 2) Incomplete comparison with the previous data,
- 3) Discussion of the current state is not well supported by the data.

The first comment is essential and the second and third comments are rather to improve the manuscript.

1) Misleading estimate of the oxygen fugacity

Both the original discussion and the reply are in error. As Fig. 9 of Giggenbach 1987 shows, CO/CO₂ ratios commonly decrease with the outlet temperature and do not keep the information of the magmatic condition. The decrease is due to reaction to follow the equilibrium under lowering temperature condition. The CO/CO₂ decrease less perfectly follows the gas buffer (SO₂/H₂S ratio controlled) than the H₂/H₂O ratio, as the CO/CO₂ ratios respond more slowly and reflect equilibrium conditions at greater temperature and pressure than that from H₂/H₂O ratios, but not of the magma unless a very quick cooling, such as at the lava lake surface, quench the reaction just after the magma degassing.

The parallel variation of CO/CO₂ ratio with the solid buffers on Fig. 9 (Giggenbach 1987) is simply apparent. The CO/CO₂ decrease slower than that controlled by the gas buffer resulted in a similar slope with that controlled by the solid buffer. The authors provided a new figure of ΔQFM -Temp to show the solid buffer control of the CO/CO₂ ratios. However, this figure is based on an unrealistic assumption that the gases are in equilibrium with rocks at the outlet temperature even at temperature as low as 400°C. Furthermore, this figure contradicts the original discussion where the oxygen fugacity is estimated with estimated magma temperatures but not with the outlet temperature.

2) Incomplete comparison with the previous data,

1-1) Conde et al. (2014)

“and the five-times difference of the CO/SO₂ ratio at 2012 vent” □ We do not report the CO/SO₂ ratio at the 2012 vent, neither do Conde et al 2014. We do report the CO/SO₂ ratio for the 2010 vent while Conde et al., 2014 do not.

Sorry for the confusion. I should say "the CO₂/SO₂ ratios at the 2012 vent".

Although the FT-IR paths are different, Conde et al. (2014) reported the ratios that should be compared. They also reported the temporal variation and the spatial heterogeneity of the ratios. These results and their discussions should be also mentioned to discuss the representative CO₂/SO₂ ratio.

“large CO₂/SO₂ ratio of the 2012 vent is attributed to the large error of the FT-IR measurement, such a conclusion cannot be justified without a quantitative error evaluation”
The CO₂/SO₂ ratio we report for the 2012 vent does carry a large error (self-evident from the regression statistics $R^2 = 0.29$) and which we already discuss at length. The 95% confidence intervals are calculated and shown on each scatter plot.

The 95% confidence intervals do not tell us a possible error range of the ratio.

Please show the error range (such as ± 1 sigma) of the ratio (e.g., 7.7 ± 2). Please show the range not only for the CO₂/SO₂ of the 2012 vent but also for other ratios and for the 2010 vent for comparison. The H₂O/SO₂ data scatter similarly with the CO₂/SO₂. If the three-times larger CO₂/SO₂ ratio is due to the error, the similar H₂O/SO₂ ratios for the 2012 and the 2010 can be simply by chance and quantitative discussion is not appropriate.

ADDED: “Conde et al.,(2013) reported SO₂ fluxes intermittently for the period 2008 to 2012 using stations from the Novac network. The high uncertainty on their reported dataset (with daily variations in SO₂ flux spanning up to three orders of magnitude and standard deviations similar or larger than the reported values) makes it unideal however for comparison purposes.”

I understand that the Conde's fluxes are less precise. However, the expression of the added text is quite strong and sounds concluding that their data are useless without justification. The description of their errors is too strong and not precise.

The large variation and errors are not associated with all the data. The error of the SO₂ flux of 840±120 t/d in 2013 seems to be similar to your error of 350±50 t/d. The Campion's fluxes do not show the large discrepancy with the Conde's flux variation diagrams (Figs. 3 and 4; Conde et al., 2013). I suggest more quantitative comparison with the Conde's data with more moderate expressions even with the cautions on their large errors.

3) Current state of the degassing

We're talking about a long term change, initiated in 2002 with the first detection of SO₂ (Vaselli et al., 2010) and subsequent increase in fumarolic activity followed by several small eruptions opening the 2010 and 2012 vents.

Target period of the discussion is not clear. The progressive drying during 2002-2008 was discussed by Vaselli et al. (2010). The present paper should focus on the changes afterward, however, it is not clear if the author suggest that the progressive drying still continued after 2008. The present study shows the similar S/CO₂ and HCl/CO₂ ratios with those in 2008, indicating a similar condition with limited hydrothermal contribution but not indicating a progressive change after 2008. The increase in the fumarolic activity may correspond to the progressive change, but it can also be the results of the eruptions and does not readily indicate the progressive drying.

Other comments

Table 3 and relating discussion are suggested to be removed, as they are not relevant to the major subject of this manuscript.

Disagreed, this table and the related discussion where suggested by an earlier review and do help understanding the data presented.

Aim of this paragraph is not clear. What is the meaning of the similar H₂O/CO₂ and SO₂/CO₂ ratios to those of Masaya and the similar SO₂/HCl ratio to Etna and Surtsey? If it is aimed to characterize the Turrialba gas composition among the arc volcanic gases, the data should be compared more systematically with a compiled data and the related discussion (e.g., Shinohara, 2013 JVGR; Fischer, 2008 Geochem.J.).