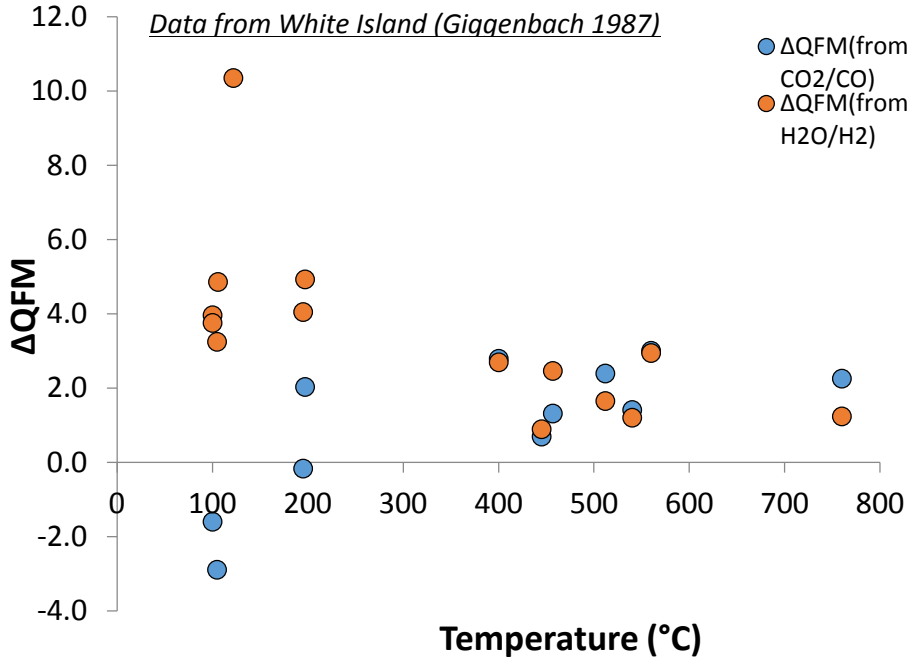
Reply to: Anonymous Referee #1
On the manuscript: “Characterisation of the magmatic signature in gas emissions from Turrialba volcano, Costa Rica”

1) Estimate of the oxygen fugacity

We again disagree with reviewer 1. The figure we provided in the reply to reviewer 1’s comment (and reproduced here for convenience) illustrated how the CO_2/CO ratio is able to retain information on the magma redox state for a large temperature range. We’ll state this once more, data from Giggenbach 1987 show that gasses collected from 800 to 400°C (and almost down to 200°C) all record a similar oxidation state (expressed as deviation from the QFM buffer in the figure below). In this temperature range the CO_2/CO ratio can therefore be used to estimate the high temperature magmatic equilibrium.



As further discussions is likely to be sterile we will simply point out some of the mistakes in reviewer 1’s comments:

“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 parallel slope in Fig. 9 (Giggenbach 1987) is quite “apparent” indeed and deviation from this slope happens at temperature below 200°C as in the above figure. So gas collected at 400 and 800°C do record the same oxidation state (using the CO_2/CO ratio). Since the CO_2/CO ratio doesn’t follow the SO_2/H_2S gas buffer there no reason to assume it is controlled by it.

“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.”

There is no such assumption. The figure reports the calculated oxygen fugacity (expressed as deviation from QFM) as a function of the gas temperature using the CO_2/CO ratio measured. The equilibrium temperature is unknown and gives rise to the discussion in the text.

“Furthermore, this figure contradicts the original discussion where the oxygen fugacity is estimated with estimated magma temperatures but not with the outlet temperature”

Quite to the opposite, the figure illustrate the original discussion and extends it; as the equilibrium temperature is unknown the system remains underdetermined, the best we can do is to illustrate the range of possibilities.

2) Comparison with the previous data

“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”

Conde et al. (2014) had a long path integrating gas not only from two vents but also from fumaroles. Our measurements, only looking at the 2012 vent cannot be appropriately compared. The discussion of the spatial heterogeneity and temporal variability presented in Conde (2014) is described there already and doesn’t need repeating. Comparing the results of our “mixed plume” composition with the one they reported is sufficient given the focus of our paper.

“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.”

ADDED: New column in table 2 showing the error on all ratio based on the standard error of the regression analysis.

I suggest more quantitative comparison with the Conde's data with more moderate expressions even with the cautions on their large errors.

The figure provided by referee #4 showing a strong correlation between the Novac data and wind speed a Turrialba supports our decision not to use the dataset of Conde et al (2013) in the discussion. Conde et al (2013) is referred in the revised text.

3) Current state of the degassing

“Target period of the discussion is not clear”

The period under discussion is illustrated quite clearly in the figure accompanying the discussion (former Fig.6 now Fig.7).

“The progressive drying during 2002-2008 was discussed by Vaselli et al. (2010). The present paper should focus on the changes afterward”

Disagreed, the discussion brings the current results in the context of previous studies to characterise the continued change in activity. See comment from reviewer 2: “Such results are of key interest to track the transitions from hydrothermal to magmatic activity at awakening volcanoes…”

"…however, it is not clear if the author suggest that the progressive drying still continued after 2008."

It is clear that we suggest this from the text and from the last figure (Fig. 8, former Fig. 7) depicting the interpreted evolution since Campion et al., 2012's investigation.

"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"

As discussed, these ratios are similar to the ones measured by Vaselli et al (2010) during the intense fumarolic activity in 2008. The changes since 2008 are physical with several small eruptions and large quantities of SO_2 emitted.

Other comments

We again choose to keep table 3 as it provides a comparison with measurements made at other arc volcanoes and was requested by an earlier review.