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Interactive comment on “Tunable diode laser measurements of hydrothermal/volcanic CO₂, and implications for the global CO₂ budget” by M. Pedone et al.

T.A. Mather (Referee)

tamsin.mather@earth.ox.ac.uk

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This paper presents a new application of tunable diode lasers to measure volcanic CO₂ fluxes from SO₂-free emissions (usually low T). This is important because CO₂ fluxes are usually estimated by scaling up with SO₂ emissions and so these fluxes are poorly quantified in terms of the global budget and volcanic activity. This technique seems to offer real potential in terms of determining and understanding CO₂ fluxes and this is an interesting demonstration and application of the technique at 4 different volcanic systems. I am strongly supportive of its publication although there are some areas that I would like to see clarified and explored further as detailed below.

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General comments:

Some more comments on the balance between diffuse and localised soil degassing at each of the locations is needed. One of the potentially interesting things about this technique would seem to be its potential to capture a cross section through whole areas of CO₂ degassing including diffuse degassing (where strong enough to be measurable) and visible localised sources such as fumaroles in the same cross-section. More discussion regarding this be very interesting and add to the impact of the paper. Is there any reason that it could not be used to measure main plumes also? Signal attenuation etc. perhaps.

Would it be possible to include the Matlab script with the paper as a supplementary file? This would facilitate its use by others and increase the impact of this study.

Throughout more specific comparison of the spatial and temporal variation of CO₂ fluxes would be a very welcome addition to the paper and would further emphasise the usefulness of the technique. See specific comments regarding comparing the spatial maps with previous studies and the Vulcano flux through time below.

Somewhere I would like to see the authors to explore how the TDL technique compares in terms of robustness, time to use, user-friendliness, cost etc. for application to a volcanic environment compared to other previously used techniques. These are very important considerations when working at volcanoes as well as the effectiveness of the technique.

Major comments:

P2653, L17: I cannot see the red cells in Fig. 2a

P2653, L 19/20 and P2655, L5: Do the errors in the ICA depend at all on where the high flux values fall? The authors state that they use synthetic data but do not state if it was tested for these sorts of issues. Errors in some kriging techniques are better assessed using a series of realisations with each specific dataset.

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P2654: The shapes of the CO₂ concentration maps generated here should be compared with previous surveys for the 3 systems where previous measurements have been made to as great an extent as possible. This would help understand the balance between diffuse/fumarole degassing in terms of this technique compared to others.

P2655: It would be really interesting to hear more about the time series of flux presented at Vulcano in Figure 6 and how it relates to other developments of this volcanic system.

Section 4.5 is interesting but needs more clarification. For example, Vulcano is included in Table 1 and 2 and Nea Kameni, the Reykjanes volcanic system and Vulcano are all included in Table 3 of Burton et al. 2013 and see comments on Figure 7 below. I think that the argument in Burton et al. (2013) is that we only have a fraction of the strongly degassing 'main' plume fluxes of CO₂ determined. While I agree that saying we have only measured one fifth is probably too low, if the current authors agree that we are likely missing some significant fraction (e.g., PNG, more in Indonesia, the new CAVA data that is cited as Aiuppa et al. submitted etc.) this still has the potential to significantly change their distribution and regression shown in Figure 7. While I think that this section makes useful points a little more on the continuing uncertainties would be appropriate. This section is also quite long and if it were possible to cut it down then it would read better. Please rephrase the last sentence of the conclusions in line with my comments above.

Figure 1a and 2c: It is not very clear where exactly on Nea Kameni this is. How do these locations relate to those shown in the other degassing studies mentioned (Tassi et al., 2013; Parks et al., 2013)? This would aid the comparisons that I suggest above.

Figure 7: It should be possible and would be helpful to indicate the new data points added beyond Burton et al. 2013 here on the Figure. There do not look to be enough data points to encompass the Burton et al. compilation as well as the new measurements they mention? More explanation of the red point in the caption would be helpful.

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I am not an expert on the tunable diode laser technique and so a technical review might also be useful.

Minor comments - P2646, L16: 'concerning' rather than 'onto' - P2647, L27-28 to P2648, L1/2: Clarify English here. - P2648, L26: 'Suggesting INCREASED mantle CO2 contribution' - P2652, L17: 'position upwind OF the fumarolic' - P2653, L5: 'In order to achieve this' rather than 'To this aim' - P2653, L16: 'divided' rather than 'dived' - P2655, L16 'blowing across the top of Hekla at the time of' - P2656, L17: 'during periods of unrest'

Interactive comment on Solid Earth Discuss., 6, 2645, 2014.

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6, C950–C953, 2014

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