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Interactive comment on "Satellite-derived SO_2 flux time-series and magmatic processes during the 2015 Calbuco eruptions" *by* Federica Pardini et al.

Anonymous Referee #1

Received and published: 23 August 2017

This paper proposes a novel approach for maximising the amount of information that can be extracted from satellite retrievals of volcanic SO2, by combining satellite images with forwards and reverse trajectory modelling to determine plume height and SO2 flux time-series. The paper is well-written and overall the method is clearly explained. I think this will make a valuable contribution to how we monitor SO2 release during explosive eruptions, and will be useful to the assessment of associated climatic impacts such as ozone depletion. However, there are a number of minor points that require addressing before publication, which I outline below in the general comments and line by line comments. I recommend that this manuscript be accepted with minor revisions.

General comments:

1. Does the paper address relevant scientific questions within the scope of SE? The

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Discussion paper

topic focus of the paper lies well within the scope of SE, as it explores new techniques for the detection of volatile emissions from volcanoes and the implications for subsurface magmatic processes.

2. Does the paper present novel concepts, ideas, tools, or data? The methodology presented in the paper is novel, and offers a useful approach for maximising the data yield from satellite images of SO2 detection. It offers a significant advance to the subject that I have not seen before in the literature.

3. Are substantial conclusions reached? The conclusions are physically reasonable, and well-justified. Although the conclusion itself not particularly novel (as the presence of pre-eruptive vapour phase is now a well-established concept), the method by which this conclusion was reached is of substantial value.

4. Are the scientific methods and assumptions valid and clearly outlined? The methods and assumptions related to the trajectory modelling and numerical calculations are very clearly explained. However, discussion of the sources of uncertainty related to the petrological technique are lacking; the overall discussion of the petrological methodology is unsatisfactorily brief. For example, are any post-entrapment crystallisation corrections applied to MI compositions? Uncertainties related to the acquisition of the initial SO2 satellite images is also lacking.

5. Are the results sufficient to support the interpretations and conclusions? The results are convincing, and are clearly in line with those of other independent studies. Although this is unlikely to change the overall conclusion, I would like to see an expanded discussion of the petrological analyses (e.g., the overall variability in both MI and glass sulfur concentrations, relationship to major element compositions, potential for post-entrapment crystallisation of MIs, potential to volatile losses from compromised MIs etc.) which would provide stronger support to the quantification of the 'excess' sulfur.

6. Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)? The description

of the modelling and numerical calculations were very clear, and well-illustrated. However, this is largely on a qualitative (at best semi-quantitative) level and the analysis could not be fully reproduced from this description. The authors might consider publishing their algorithm as a supplement to the paper, if indeed their intention is for this to be a useful tool to the wider community.

7. Do the authors give proper credit to related work and clearly indicate their own new/original contribution? Yes

8. Does the title clearly reflect the contents of the paper? Yes, the title is relevant to the subject matter.

9. Does the abstract provide a concise and complete summary? The abstract is well-written and provides a good summary of the paper. However, it is too long and could be condensed more effectively to have more of an impact: a brief summary abstract is more informative to the reader than a lengthy detailed abstract that repeats sections of the introduction and conclusions.

10. Is the overall presentation well structured and clear? Is the language fluent and precise? Are mathematical formulae, symbols, abbreviations, and units correctly defined and used? Yes, the paper is well-structured and well-written overall and the organisation of ideas flows well. There are a couple of typo errors in the units, which I have highlighted on the line by line comments below.

11. Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated? The figures are very good. I particularly like how the data are presented in figure 8. Panels a,b,c,d need to be labelled in figure 6.

I feel that the petrological methods should be described up front in the methods section, rather than being in the supplementary information. The petrological data are central to the discussion of the magmatic processes involved, and so should be set up in the main paper.

12. Are the number and quality of references appropriate? Mostly yes. There are a few additional papers that are relevant and I would suggest considering, particularly those relating to the fidelity of melts inclusions as a volatile record, for example: Wallace, P.J. and Edmonds, M., 2011. The sulfur budget in magmas: evidence from melt inclusions, submarine glasses, and volcanic gas emissions. Reviews in Mineralogy and Geochemistry, 73(1), pp.215-246.

Andres, R.J., Rose, W.I., Kyle, P.R., DeSilva, S., Francis, P., Gardeweg, M. and Roa, H.M., 1991. Excessive sulfur dioxide emissions from Chilean volcanoes. Journal of Volcanology and Geothermal Research, 46(3-4), pp.323-329.

Wallace, P.J., 2005. Volatiles in subduction zone magmas: concentrations and fluxes based on melt inclusion and volcanic gas data. Journal of Volcanology and Geothermal Research, 140(1), pp.217-240.

Danyushevsky, L.V., McNeill, A.W. and Sobolev, A.V., 2002. Experimental and petrological studies of melt inclusions in phenocrysts from mantle-derived magmas: an overview of techniques, advantages and complications. Chemical Geology, 183(1), pp.5-24.

13. Is the amount and quality of supplementary material appropriate? Yes the SI is fine and provides useful extra detail. However, the authors should consider including the description of the petrological methods up front in the main methods section of the paper.

Line by line comments:

P2 Line 11: replace ‘which’ with ‘that’

P2 Line 16: replace Westrich et al., 1992 with Westrich and Gerlach, 1992 (also consider citing some additional references here)

P2 Line 32: Should ‘box method’ be ‘delta method’ on this line?

P3 Line 20: Consider adding some additional explanation to this statement such as '. . .allow us to infer the presence of excess SO2 at depth before the eruption, when combined with petrological and deposit volume constraints.'

P4 line 4: 'appeared to be more violent' – in what way? Considering adding additional description. P4 line 13, 14: Why were such deposit densities used by the cited studies? (1000 kg m-3 vs. 2500 kg m-3)

P4 line 14: units typo – replace '2450 km m-3' with '2450 kg m-3'

P6 line 12-14: The justification for using the 2.5 km plume is not clear to me, please add some additional explanation or re-phrase. How is overestimation of the SO2 plume a good thing?

P7 line 3, 4: 'This is due to several uncertainties given by wind data, trajectory calculation and SO2 spatial distribution' – give more specific details of what exactly these uncertainty sources are, and how significant.

P8 Line 19: should Figure 5(d) be Figure 5(b)? (there are only two panels in figure 5. . .)

P9 line 18, 19: Uncertainties should be given along with calculated MER values

P9 Line 21: should 'first or the four layers' be 'first of the four layers'?

P9 Line 23: should 'despite the two authors agree' be 'despite this, the two authors agree'

P9 Line 23: What is the basis for the disagreement between the origin of layer 2? Why is it ambiguous? How much does this affect you conclusion if layer 2 is attributed instead to eruption 1?

P9 line 32: 'well-correlated' – looking at figure 8 I would say this is a slight overstatement. I agree there is a correlation, but there is still quite a bit of scatter in the data.

P10 line 23: 'bubbles migrated to the top of the...'

P10 line 29: methodology should not be in the SI (see earlier comments)

Also, just a consideration - potentially pyrite is not the optimal standard for S in this case. I anticipate that the Calbuco magma is quite oxidising, such that much of the S will be in the S6+ phase. Use of pyrite (S2-) standard may well be underestimating the total dissolved S in the glass, as the peak position of S varies quite significantly between the two valence states. Barite may have been a more appropriate standard.

P10 line 32: Have you considered that MI hosted in plagioclase may not represent that initial S concentration in the melt? Late crystallisation of plagioclase may yield MIs of slightly more evolved composition.

Have you performed any post-entrapment crystallisation corrections on your MI compositions?

Some discussion of the major element systematics of your MI and matrix glass data would help to shed light on these points.

P11 line 3: Do the errors on your petrological S yields include the errors attached to the deposit volumes?

P11 line 4: 'it is'

Table 1: Matrix glass S values are close to the limit of detection, I am surprised to see such low uncertainties on these measurements.

'0.8 is a coefficient accounting for 20 vol% of crystallisation' – this is thrown in here as a footnote without any mention or explanation in the text. Please clarify this in the main paper.

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