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Interactive comment on "Improving automated global detection of volcanic SO_2 plumes using the Ozone Monitoring Instrument (OMI)" *by* V. J. B. Flower et al.

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I would suggest to slightly change the title of the paper, since it gives the reader the feeling that a detection method to detect volcanic SO2 plumes on a global scale is presented in this paper. However, the method presented is only suitable in the vicinity of volcanoes to detect eruptive events and not e.g. to monitor SO2 plumes in general, as other works do (e.g. Brenot 2014). Only at the very end of the paper in Sect. 5 this is clarified. As a suggestion I would simply replace the word 'plumes' by 'eruptions' in the title and also clarify this in the abstract. Response: Thank you for the suggestion, the title has been updated. The paper has also been updated to better define the procedure developed with potential methods for more widespread implementation and

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Discussion paper

accounting for the limitations mentioned here. Change: Page 1 Line 1-2

On which assumption is eq 1 based on? Was it found by testing? Can it be improved and does the choice e.g. of the denominator have an impact on the analysis? I can image that during explosive events or after dispersion of the plume the M1 region contains much more signal than the M2 region, severely affecting the value of M3 Response: Equation 1 was found by testing. The equation was found to be effective in the diverse eruptions incorporated however it could be refined particularly where a regional analysis was performed. The extension of this work to in depth regional analysis and the capability of the technique to be tweaked to more effectively define volcanic eruptions have been incorporated into the paper. The potential applications of the developed methodology on a wider scale through the implementation of a multi segment analysis region which would be more adept at capturing drifting plumes has also been added to the paper. Change: Page 12 Line 25-30

The authors have chosen TRL OMSO2 data for their analysis. Choosing the TRM or STL product would provide a dataset with much less noise. Since the difference in the different products should only be a different scaling factor (or AMF) , in my opinion choosing e.g. the STL product would decrease the amount of false-positive detections. This is one reason, why Brenot 2014 has also chosen this plume height product (as described in Sect. 1.1). The authors themselves state in Sect 4.4 that a product with lower noise should result in lower detection limits - so I was wondering, why this hasn't been done, or at least, analyzed what the effect would be. Response: While the TRM or STL algorithms would decrease false positives they would also severely limit the capabilities of this technique with respect to smaller eruptions with lower injection altitudes. Plumes with these characteristics are currently the least resolved plumes with existing techniques which this method aims to remedy therefore the use of either of these retrievals would result in no improvement in current plume identification capabilities. Change: Page 4 Line 12-17

Before the modelling procedures are described in great detail in 2.4, I am missing some

basic introduction beforehand why this model has been chosen. The way in in which the paper is currently organized and written, the reader gets an overview of the data collection method and the plume quantification, which is very good and detailed, but then section 2.4 immediately starts off with the description of the software package and the logistic regression model. It took me a while until I understood why this is done :::hence some words before that this technique is chosen, and first a training has to be conducted and then the model can be applied to other datasets is needed in my opinion at the beginning of 2.4. Currently, up to section 2.3 the paper reads as if only a simple threshold criterion is used to detect volcanic events, after the correction of the background noise using the M3 model. Response: Thank you for this suggestion. An additional section has been added to the beginning of the Methodology section outlining the techniques to be investigated. Change: Page 3 Line 6-17

There are some points not (or only shortly) discussed in the paper, which could be potentially interesting and should be further analyzed from my point of view: - Does the effect of instrument degradation (and hence choice of the training dataset) has an effect on the analysis and training of the model? I.e. what would happen if I train the model only with data of a specific time frame? Response: The degradation of the instrument is likely to influence the training of the model by limiting the number of eruptions identified. The spatial resolution of the remaining data (due to measurements only being obtained from half of the measurement swath) could influence the results. This is supported by the fact that the variation in viewing angle has been seen within time series analyses of OMI data (Flower et al., 2016; doi: 10.1016/j.rse.2016.05.022). Therefore, the corrections for off–nadir views likely add some uncertainty and could influence the results. This effect is likely to be minimal and would be accounted for if other measurements with similar observational characteristics (off-nadir views) were also included in the training of the model (as was the case in this work).

- When the model is trained using only a single volcano, the detection limit should decrease. Would this be an option for the monitoring of selected volcanoes? Response:

If you have a detailed breakdown of discrete eruptions in a specific location then data could be collected correlating to these events. These (along with a control group) could be used to both refine the background correction equation (either by alteration of the background/active correction or by altering the location of the 'background' region) and result in a lower detection limit at a specific location. Details relating to the potential use of the developed method in this way has been incorporated throughout the paper. Change: Page 12 Line 30 - Page 13 Line 2

- What is the impact of cloud screening? During the selection of volcanic events for the training dataset, were there only eruptions chosen with no cloud cover? Is there a limit on the cloud cover or cloud pressure limit for this method in order to detect degassing events? Response: A cloud fraction limit of ∼20% of the scene was implemented in the initial data collection stage of this analysis in order to attribute the effects identified to volcanic events. This limit was a personal preference based upon experience with the data and could be altered depending upon the user and specific requirements of both the size of eruptions analysed and the local meteorological characteristics. Additional information has been added to the cloud cover section of the paper. Change: Page 6 Line 10-11

- What is the effect of using a different SO2 product (e.g. TRM or STL) ? See my point above Response: This comment has been addressed by the addition of information to the paper in relation to the question above Change: Page 4 Line 12-17

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