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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Volcanic SO_2 fluxes derived from satellite data: a survey using OMI, GOME-2, IASI and MODIS" *by* N. Theys et al.

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In this paper the authors review several techniques used to derive SO2 emission rates (fluxes) from satellite measurements of SO2 columns. The derivation of SO2 fluxes, as opposed to SO2 mass loadings, from satellite data is important as the fluxes can provide more information on the eruptive process at a given time and also on the plume altitude (via the relationship between mass flux and plume altitude). They demonstrate the application of these techniques to volcanic plumes from three recent eruptions (Puyehue-Cordon Caulle, Nyamulagira and Nabro) using UV and IR satellite data from IASI, GOME-2, OMI and MODIS.

Although the paper contains a lot of useful new information, including the derived SO2 fluxes for the eruptions studied, and a comparison between IASI and GOME-2 SO2

data, my main criticism is that the paper is fundamentally a techniques paper. The authors compare results from the different techniques (many of which have been previously described elsewhere), and provide some validation of the methods, but do not use the SO2 fluxes they derive to address any real scientific questions. In the case of one of the eruptions studied (Nabro), with additional work their analysis could be used to further investigate the long-range transport of the SO2 emissions, which remains controversial.

The choice of eruption case studies is somewhat arbitrary, with the Puyehue example being perhaps the least compelling. Although Puyehue was a silicic, ash-rich, explosive eruption (distinct from the other eruptions analyzed), the analysis does not contribute anything particularly unique relative to the other case studies. The Nyamulagira and Nabro eruptions are more logical choices since continuous emissions over a period of days or weeks are more amenable to SO2 flux calculations, and such measurements can be useful for eruption monitoring. However, a shortcoming of the paper is that is only discusses large eruptions, whereas SO2 fluxes are more commonly used for monitoring of 'passive' or non-eruptive volcanic plumes. For this reason I recommend that the authors cite the following, complementary paper (currently in press), on a similar topic, but focused on monitoring lower tropospheric emissions with similar techniques:

Carn, S.A., N.A. Krotkov, K. Yang, and A.J. Krueger (2013), Measuring global volcanic degassing with the Ozone Monitoring Instrument (OMI), Spec. Publ. Geol. Soc. Lon., 380, (in press).

The reviewer can supply a pre-print of this article on request if needed.

The paper suffers from poor organization and is often hard to follow. Methods and results are intermingled in several places (see comments below) and the paper needs reorganizing in a more logical manner. The use of English could also be improved throughout.

In view of the above my overall recommendation is that the paper, after some improvements, would be better considered for publication in Atmospheric Measurement Techniques (AMT), unless the authors can modify it in such a way as to use their results to address a specific scientific question(s).

Specific comments:

31351: the discussion of ‘explosive’ vs ‘effusive’ eruptions here is overly simplistic and needs refining; in fact there is a continuous range of volcanic eruption styles of which these are ‘end-members’. Both effusive and explosive eruptions involve exsolution of volcanic gases due to decompression, and ‘fire fountains’ can be considered a form of explosive activity involving fragmentation of the magma (albeit with larger fragments than those generated during more silicic eruptions). Weak explosive eruptions produce lower column heights than large effusive eruptions. I think the key point is that SO2 is a marker of any ‘magmatic’ eruption and perhaps the major control on column height is the eruptive mass flux (which determines the heat flux).

31352, L16: scrubbing of SO2 can often lead to a complete absence of SO2 emissions at the surface (e.g., at heavily glaciated or tropical volcanoes), and a dominance of H2S or CO2. Hence, including (4) here as an example of the use of SO2 as a marker of important processes is not really valid, since there may be no SO2 to measure. It would be more correct to use this as an example of when SO2 measurements are less useful.

31353, L23: suggest replacing ‘poor detection limit’ with ‘high detection limit’.

31353, L26: note that although the TOVS sensor was first launched in 1978, the technique to retrieve SO2 columns using the data was developed much later (2003).

31354, L1: the increased sensitivity is largely due to improvements in spectral and spatial resolution, so these are not independent.

31354, L3: the list of sensors could be updated to include the hyperspectral UV OMPS sensor on the Suomi-NPP satellite.

31354, L9: ‘strongest sources’ is a little ambiguous here, since there is no indication of the detection limit. Also, the authors should clarify that they are referring to daily satellite measurements, since time-averaging of daily data can be used to detect much weaker SO2 sources.

31354, L22: Carn and Bluth, GRL, 2003 should also be cited here. They used TOMS SO2 data to calculate SO2 fluxes from Nyamuragira.

31355, L15: note that OMI is also used in the NASA-NOAA near real-time SO2 monitoring system (http://satepsanone.nesdis.noaa.gov/pub/OMI/OMISO2/index.html).

31357, L28: the origin of the OMI row anomaly is incorrectly described here. It is due to a blockage affecting the nadir viewing port of the sensor, rather than a sensor defect.

31358, L5: the authors should describe the criteria used to decide if the data were ‘useful’ – if data are flagged as affected by the row anomaly, it is unwise to use them for scientific analysis.

31360, L13: base -> basis

31362, L14: . . .consider the SO2 mass contained. . .

31362, L16: it could be clarified here that the dimensions of the ‘box’ are usually determined using a trajectory model or radiosonde wind profile.

31364, L10-13: is this statement regarding ‘reliable SO2 fluxes’ based on actual data (e.g., comparison of satellite flux measurements with independent data from ash-laden plumes; if so a reference should be provided) or is it just conjecture? Please clarify. Furthermore, I would not expect SO2 depletion to be negligible 100s of km from a volcano.

31365, L3-5: it should be noted that coverage from LEO satellites also depends on latitude, so the UV sensors can provide increased temporal resolution at high latitudes.

31365, L12: with multiple satellites and global coverage, coverage of even very large plumes should not be an issue (with the exception of unforeseen data gaps)? Of course, for very large plumes the assumption of constant k (loss rate) becomes more unrealistic.

31365, L22: I don't think there is any reason why time-series mass flux curves should be bell-shaped. In my experience they are usually quite asymmetric, with an initial rapid increase in SO2 flux followed by a slower decline.

31366, L25: in addition to the other papers cited in this paragraph, the following paper should also be cited, since it refers to an eruption of Nyamulagira (as does this paper):

Hughes, E.J., L.C. Sparling, S.A. Carn, and A.J. Krueger (2012). Using horizontal transport characteristics to infer an emission height time-series of volcanic SO2. J. Geophys. Res., 117, D18307, doi:10.1029/2012JD017957.

31369: I am a little confused by the Puyehue-Cordon Caulle example as the first part of the analysis (delta-M method) appears to focus on the initial SO2 cloud that made several circuits of the globe. Calculating an SO2 flux for a drifting plume seems pointless if no additional SO2 is being emitted – and it is not clear if significant (if any) new SO2 emissions from the volcano were detected by IASI after the initial 2-3 days of the eruption? After June 7 the derived SO2 flux in Fig. 2 seems to tend towards zero, as expected. Focusing on the first few days of the eruption using the traverse method analysis (31370, L16) seems more logical.

31369, L22: increases in SO2 mass have often been observed in satellite data following large eruptions – can the authors suggest a reason for this? Signal saturation in the fresh plume and/or emission of H2S (later oxidized to SO2) have been suggested for other eruptions.

31371, L5: the plumes were ash-rich throughout the first few days of the eruption.

31374, L3: how was the effect of the OMI row anomaly mitigated? The data gaps would affect the delta-M method. This is mentioned later (31375, L3) but should be discussed

earlier in section 4.2, along with the use of GOME-2 data. Also, it is not stated which altitude is assumed for the SO2 and/or if the OMI SO2 columns were interpolated to this assumed altitude.

31375, L11: could the OMI-GOME-2 differences also be due to the temporal offset between the morning (GOME-2) and afternoon (OMI) satellite overpasses? It is not stated if the same SO2 loss rate was assumed for OMI and GOME-2. Could differences in the assumed SO2 altitude (OMI vs. GOME-2 retrievals) also be an issue?

31375, L20: does this error analysis refer to OMI or GOME-2 or both?

31375, section 4.3: no references are given here for the details of the Nabro eruption – citations are needed.

31376, L9-11: I suggest omitting the categorization of the eruptions – Puyehue was not classically Plinian (it also produced a rhyolitic lava flow), nor was Nyamulagira 'pure effusive' (since lava fountains were involved). Although details of the eruption remain unclear, the Nabro eruption was clearly not purely effusive either. Note also that the 1981-82 Nyamulagira 'effusive' eruption produced a SO2 plume that reached the tropopause (Krueger et al., 1996), so this is not exceptional.

31376, L13: the UV satellite images alone cannot reveal the 'multi-layered' nature of SO2 plumes; they are not altitude-resolved.

31376, L18-25: perhaps it would make sense (and save some space) for comparison purposes to present all the inversion settings used for all the eruptions studied in a table?

31377, L6: what is the source of the plume altitude information (15-18 km)?

31377, L8: please explain 'fine tuning'. Also, an SO2 e-folding time of 2 days at the surface seems long – is there a reference for this?

31377, L25: 'limited sensitivity to SO2 in the lower troposphere' – this seems contradictory to an earlier statement (L19) that GOME-2 has sensitivity down to the surface.

31378, L22: again, the authors need to state the source of the plume altitude cited here (15-18 km).

31379, L7-14: in this case, why was the 6 h time step used at all? Was the inversion also performed with a 12 h time step? These issues with the technique should be highlighted earlier (section 3.4).

31379, L17: 'misfit effects' and other sources of uncertainty in the inversion technique should be described in section 3.4.

31380, L5-6: details of the eruption (20 km long lava flow) are given here with no citation of the source. In fact, satellite evidence suggests that the extent of the Nabro lava flow was already significant prior to June 17, so this statement is incorrect.

31380, L7: this information on GOME-2 operations should also appear earlier, before the results.

31380, L24: MODIS measurements are introduced here, but the use of MODIS should be described prior to the 'results' section. I recommend that the sensors used in the analysis of each eruption should also be summarized in a table (along with inversion settings and other key information).

31381, L6: 'volcanic water vapor particles' – does this refer to gas, liquid, solid (ice) or all three? Also, it seems unlikely that water vapor of volcanic origin can be distinguished from ambient atmospheric water vapor.

31381, L19: in their discussion of the initial Nabro eruption plume, the authors need to cite work by Bourassa et al., Science, 2012 (and subsequent technical comments) on the transport of the Nabro volcanic cloud. This is cited later (31382, L27) but appears almost as an afterthought, whereas previous work should always be cited first. Bourassa et al. (2012) invoke upward transport in the Asian Monsoon circulation to explain the large stratospheric impact of the eruption, and the peculiarities of the transport

mechanism could partly explain the poor results of the inversion. Indeed, the authors should explore whether their results shed any light on the transport mechanism (which remains controversial).

31382, L3: 'passive' may be a poor choice of word here – in volcanological terms this refers to a non-eruptive plume. By 'passive' do the authors mean a plume transported solely by the ambient wind field, and not by other processes? Could the term 'weak plume' be used instead?

31383, L10: 'non-nil' = 'non-zero'?

31383, section 4.3.3: since the authors state here that the inversion technique is only applicable if the IASI and GOME-2 SO_2 columns are consistent, then this entire section should logically appear prior to application of the inversion technique (to Nabro in particular), in order to validate its use (i.e., before section 4.3.1).

Figures: several of the figures (e.g., Fig. 11) would require enlargement (relative to the review copy) as the text is hard to read.

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