Atmos. Meas. Tech. Discuss.,
doi:10.5194/amt-2019-13-RC2, 2019

Atmospheric Measurement Techniques Discussions

Interactive comment on "SO_2 Layer Height retrieval from Sentinel-5 Precursor/TROPOMI using FP_ILM" *by* Pascal Hedelt et al.

Anonymous Referee #2

Received and published: 15 May 2019

In this paper the authors present an algorithm to retrieve the altitude (or layer height, LH) of volcanic sulfur dioxide (SO2) clouds in near real-time using ultraviolet (UV) satellite data from the Sentinel-5P/TROPOMI instrument. TROPOMI provides the highest spatial resolution UV observations currently available from space. The injection altitude of SO2 during volcanic activity is the main factor determining the climate impact of volcanic eruptions, and can also be used as a reasonable proxy for volcanic ash cloud altitude, which is required for aviation hazard mitigation. Hence accurate retrievals of SO2 altitude are important and of broad interest to the atmospheric and volcano science community. The main advance described in the paper is the adaptation of an existing 'machine learning' SO2 altitude retrieval algorithm (FP_ILM) to the relatively new TROPOMI instrument. The advantage of the FP_ILM algorithm over most exist-

ing SO2 altitude retrievals is the fast processing speed, which allows it to run in near real-time.

Overall I think the paper could be suitable for publication in AMT after some moderate revisions. The structure of the paper could be improved – currently there are many short paragraphs and not all the information is presented in a logical order, and many figures could be improved (see detailed comments below). I do question why the authors only simulated SO2 layer heights up to a maximum of 20 km? Major volcanic eruptions (with the largest potential climate impacts) can inject SO2 to greater altitudes and hence it would be interesting to see how the FP_ILM algorithm would perform in such a scenario, given that the FP_ILM retrievals of SO2 at 20 km altitude appear least accurate for low SO2 VCDs (e.g., Figure 4 suggests that a VCD > 40 DU is needed for accurate retrieval). On a related note, under very high SO2 loadings in a major eruption the ozone (O3) VCD retrievals may be inaccurate (due to SO2 interference), and I assume this would preclude accurate SO2 LH retrieval (since the O3 VCD is a required input). I also find that the stated SO2 LH accuracy of 'better than 2 km for SO2 VCD > 20 DU' is a little exaggerated, especially for higher SO2 LH, e.g., for an SO2 VCD of 20 DU at 20 km, the SO2 LH appears underestimated by $\sim$5 km in Figure 3.

Another weakness is the validation of the TROPOMI SO2 LH using IASI. Since IASI measurements are not coincident with TROPOMI, only broad conclusions can be drawn from the comparisons. In addition to CALIOP, the authors could explore the use of Microwave Limb Sounder (MLS) SO2 data from the Aura satellite to validate the SO2 LH retrievals.

Specific comments:

P1, L21: there are many different 'flavours' of DOAS algorithm, so writing 'the DOAS algorithm' seems to be a major generalization. Furthermore, there should probably also be a reference to the first multi-spectral Total Ozone Mapping Spectrometer (TOMS)

SO2 retrievals, which used a different approach.

P2, L1: 'fast enough for NRT retrievals' – algorithm speed/computational cost and the timeliness of retrievals are mentioned several times in the paper (e.g., P2, L19-20; P3, L3-5; P3, L17), but there is little quantitative information (I see that there is some information on P6). I would recommend adding a brief discussion to the introduction describing the data latency desired (e.g., for aviation safety and other applications) and estimates of current processing speeds.

P2, L2-4: I think any SO2 algorithm (regardless of whether an AMF is explicitly used) needs to make some assumptions regarding the SO2 vertical distribution (due to the pressure/temperature dependence of SO2 absorption).

P2, L5: accurate AMF calculations could also include parameters such as cloud fraction, surface pressure and surface reflectivity. Also, it is not unique to the 305-335 nm range.

P2, L6: some of paragraph 2 basically restates the previous paragraph; i.e., the SO2 VCD is strongly dependent on the vertical distribution of SO2, as the latter strongly affects the AMF. These paragraphs could be reorganized/combined to clarify the text.

P2, L10: it could be added here (instead of L16) that the usual approach for operational SO2 retrievals (not only from TROPOMI but also other UV sensors) is to assume several different a-priori SO2 vertical distributions and provide VCDs for each. I think it is also important to stress that above $\sim$5 km or so (i.e., in the upper troposphere and above), the vertical SO2 distribution has relatively little impact on the VCD (although the actual altitude is still of interest of course).

P2, L14: it is not only the number of photons but also the UV wavelengths interacting with the SO2 layer that are influenced by the SO2 layer height.

P2, L16: TROPOMI is first mentioned here, but some key information is provided later on L29 – some reorganization is needed.

P2, L21: Extensive -> Extended.

P2, L22: it is not clear what is meant by 'strong volcanic eruptions'. Eruptions can be relatively weak and still produce high SO2 column amounts, and vice versa.

P2, L26-27: is there a reference to support the statement that IR SO2 height retrievals are more accurate than UV retrievals? I'm not sure that either approach has been extensively validated.

P3, L3: It would be useful to know how often the algorithm needs to be 'trained'. Is re-training necessary if the TROPOMI data quality changes, or for other reasons?

P3, L6: please also cite the first paper on the FP_ILM SO2 LH algorithm here. It might also be useful to briefly summarize the 'improvements' to the algorithm here too.

P3, L24: 'plume profile'.

P4, L10: eight parameters were used to simulate the spectra, but a larger number (10) of PCs is needed to retrieve the layer height. This seems to contradict the authors assertion (P4, L9) that 'fewer parameters' are used to characterize the dataset after the PCA. Some more explanation/clarification may be needed here.

P4, L12: Figure 6 is the first Figure referenced here – in which case the Figures should be reordered.

P4, L16: This paragraph (and also the following one) is probably difficult to follow for anyone not acquainted with neural networks or machine learning. Several new terms are introduced without elaboration (loss function, weight vectors, hidden layers). I recommend that the authors provide more details on the procedure.

P5, L5: shouldn't the O3 VCD also be listed as a direct dependency?

P6, L2: from Figure 4, it appears that the accuracy of SO2 LH retrieval does not significantly improve with increasing SNR for high altitude SO2 LH (20 km). Can the authors explain this?

P6, L23: The IASI data are not the only source of independent SO2 LH data. The Microwave Limb Sounder (MLS) on the NASA/Aura satellite can provide some information on SO2 LH, albeit with limited spatial coverage and vertical resolution. The afternoon MLS overpass is nearly coincident with TROPOMI, and MLS did detect at least some of the eruptions discussed in the paper. I wonder if the authors considered using the MLS data to validate their SO2 LH retrievals?

P6, L26: it would be useful to have at least a few more details on the Ambae volcanic activity (and actually for all the eruptions discussed in the paper), e.g., from the Smithsonian Institution Global Volcanism Program reports.

P6, L30: it is unclear why the plume is 'aged'? See general comment below regarding the SO2 LH map (only pixels with robust LH retrievals should be shown). The two plumes discussed in the text are swamped by areas of blue (low SO2 layer heights which I presume are incorrect due to the generally low SO2 VCDs) and hence hard to see.

P7, L6: need to stress that this is also from Ambae.

P7, L8: acid rain is not usually an issue for stratospheric SO2.

P7, L15: a brief description and reference for the CALIOP instrument is needed (also provide the full name of the sensor).

P7, L16: it should be noted that it is not necessarily the case that SO2 and ash/aerosols are collocated, as gas and ash can separate in volcanic clouds (e.g., as ash falls out to lower altitudes).

Figures:

Figures 3 and 4 are quite similar and could perhaps be combined as one figure.

Figure 5: this figure is a bit cluttered and could perhaps be improved by removing some of the data from the plot, e.g., using just the higher SO2 VCDs.

Figures 7-15: General comment on the SO2 map figures: I would recommend 'zooming in' as much as possible on the SO2 plumes to show the detailed structure (especially Sinabung). Also, I think the SO2 LH plots should only show those TROPOMI pixels with robust SO2 LH retrievals (i.e., SO2 VCD > 10-20 DU or so), since otherwise most of the plots are showing invalid data.

Figure 8: I'm not sure that it is necessary to show both the IASI-A and IASI-B SO2 LH data. Since neither are coincident with TROPOMI, just show the overpass that is closest in time and/or which has the best coverage of the volcanic plume.

Figure 11: the CALIPSO satellite track corresponding to the lidar data in Figure 12 should be shown on the maps.

Figure 12: this figure is also not very clear. I recommend 'zooming in' to the volcanic plume to show the data more clearly, and only plotting the red symbols (SO2 VCD > 20 DU). It is also not clear which features represent the Sinabung volcanic eruption cloud and which are meteorological clouds; this could be highlighted on the plot.

Figure 13: the left-hand panel does not seem to show much if any useful IASI data. Given that there are CALIOP data for this case, perhaps the IASI data are not needed and this figure could be removed.

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