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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Validation of satellite SO_2 observations in northern Finland during the Icelandic Holuhraun fissure eruption" *by* I. Ialongo et al.

Anonymous Referee #1

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This paper provides a comparison of results from different SO_2 total column products of the OMI and OMPS satellite instruments as well as for ground-based Brewer direct sun measurements at Sodankylä (Finland) during the Holuhraun fissure eruption (Iceland) starting in September 2014. Additionally, SO_2 surface concentrations from four air quality stations in Northern Finland are presented, supporting that the volcanic plume was located at low altitudes over Finland.
The paper is well written. However, while the authors suggest to show "validation" results, I would rather consistently use the word "comparison". It is mainly shown that the satellite as well as the ground-based measurements support the presence

of the volcanic plume in the lower part of the atmosphere and that (at least for some observations) the variability of the SO_2 columns is similar. While a comprehensive "validation" of operational OMI/OMPS SO_2 products and ground-based data generally should be strongly endorsed (the literature on this topic is quite sparse), a main focus of the paper should be on the general uncertainties that come along with the operational processing of the satellite data as well as other problems like e.g. differences in spatial/temporal resolution. These problems are only qualitatively discussed and illustrated by showing results for different a priori assumptions of the SO_2 profile for the satellite measurements. Especially for SO_2 at low altitudes, the actual profile (plume altitude) has probably the most critical influence on the final column densities. Generally, it is therefore questionable if the operational satellite products (in this case especially the PBL products) should be used instead of conducting detailed sensitivity studies using radiative transfer model calculations adjusted to the actual set-up. I think this paper has the potential to be published in AMT, but after major revision:

Specific comments:

p. 600, l. 8-9:

How are the "challenging retrieval conditions" actually considered? Especially the OMI BRD PBL product assumes a constant AMF of 0.36 according to an older version of the README file (http://SO2.gsfc.nasa.gov/Documentation/OMSO2Readme.doc). A constant SZA of 30°, VZA=0°, surface albedo=0.05 and surface pressure are assumed. This geometrical assumptions may lead to significant under/overestimation of the SO_2 column, especially for high latitudes where the AMF of 0.36 is likely to be strongly overestimated (and therefore the SO_2 column to be underestimated – as mentioned by the authors).The presumed settings for the OMPS PCA PBL product are the same, right?

As already mentioned, I think it's questionable if these operational products should be used for a "validation" exercise. Some of the parameters indicating "challenging retrieval conditions" are listed in Table 2, but it is not really clear if the associated uncertainties are larger than e.g. the actual satellite ground pixel size or SO_2 fit error. How large are the associated errors?
According to the README file, only OMI pixels of the rows 5-55 should be used for the PBL product because of the increased noise (probably because of the large pixel size and especially viewing zenith angle). Half of the OMI measurements listed in Table 2, however, belong to rows 1-4 and 56-60 (all of them have CF>0.3 anyway). Maybe it would be good to highlight all other measurements or just exclude all measurements at the edge of the swath in Table 2.

p. 602, l. 12-13:

It would be good to add some words about other problems (e.g. spatial/temporal resolution, unknown profile, sensitivity, clouds).

p. 602, l. 12-13:

I think the results from your paper show that this is only the case when you have access to data of a widespread measurement network and not only for just one instrument (as it was also mentioned at the end of Carn and Lopez, 2011).

p. 602, l. 25-27:

What possible reasons were mentioned in Rix et al. for these "large" discrepancies? Are your measurements affected by the some problems?

p. 604, l. 22-25 and p. 612 l. 7-11:

What is the advantage of using the OMI DB data here? Although it is interesting to know that the data is in principle available, the DB data for the PBL scenario (like the PBL SP data) is processed using the BRD algorithm and the main difference is claimed to be the sliding median background correction. However, although the resulting differences between the PBL DB and SP data are mostly small, they're up to about 50% for specific measurements (e.g. 6/9/2014, 9:03, 2.59 vs. 3.86 DU). I think such significant differences for the very same algorithm/instrument clearly point out that you have to put more effort in error analysis and discuss the possible reasons/uncertainties.

p. 607, l. 5-6:

What exactly is meant by "sufficiently continuous"?

p. 607, l. 11-13:

The different satellite products are not "sensitive to different altitude regions". The slant columns are just converted into vertical columns by using AMFs for different a priori profiles.

p. 607, l. 14-15:

It should be already mentioned in Section 2.1 that the DB products are processed by using the BRD algorithm (but uses a different background smoothing correction...what else are the differences?).

p. 607, l. 21-22:

While it was reported that the volcanic plume was close to the ground by several other groups, the satellite SO_2 SCD measurements are also likely to be significantly lower due to dilution, as only the average SCD is detected within large satellite ground pixels while the Brewer measurements are local point measurements. Probably the VCDs are closest to the Brewer measurements for the PBL product because of the associated small AMF.

p. 607, l. 23-26:

As mentioned before, due to the constant AMF of 0.36, the PBL product is probably not well suited for a "validation" (especially for large SZA/VZA).

p. 608/609 and Fig. 2:

Please add error bars. How large are the uncertainties? Are all satellite measurements exceeding the detection limit? Maybe it would be good to show local satellite maps of the PBL product scaled to the maximum detected SO_2 VCD to get a feeling for background noise and uncertainties.
The different products for the cloudy measurements are hard to distinguish. Maybe it would be good to think about using other symbols.

p. 608, l. 1-8:

The PCA PBL result is significantly lower than the BRD PBL result. What are possible reasons?

p. 608, l. 9-17:

While the measurement conditions are quite similar for the previous day, the data for the satellite and ground measurements agree less. What are the possible reasons?

p. 608, l. 18-23:

The largest data gap for the Brewer measurements appears right where the largest SO_2 CD is expected? Is there a specific reason for that or is it just coincidence?

p. 608, l. 24-29:

The PCA PBL and BRD PBL product show a large discrepancy. Again, the PCA value is much smaller and close to zero. What are the possible reasons?

Section 3.3:

While it is interesting to see the data for the air quality stations, I don't really see how this adds something to the "validation" process. No Brewer measurements are presented (and available) and the main conclusion is that the volcanic plume was located at low altitudes over Finland.

p. 612, l. 23-25:

Is this only the case for the PBL products (because of the constant AMF) or for all satellite products?

Minor comments:

p. 601, l. 22:
observation systems

p. 604, l. 4:
Please add:
Carn, S. A., Yang, K., Prata, A. J. and Krotkov, N. A.: Extending the long-term record of volcanic SO_2 emissions with the Ozone Mapping and Profiler Suite nadir mapper, Geophysical Research Letters, DOI: 10.1002/2014GL062437, 2015

p. 604, l. 22:
products

p. 605, l. 23-24:
Maybe good to mention that Kagoshima is located right next to a very active volcano (Sakurajima).

p. 606, l. 22-24:
Probably most important: There's no sun light during winter at high latitudes.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 599, 2015.