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Atmospheric Measurement Techniques Discussions

Interactive comment on "New-generation NASA Aura Ozone Monitoring Instrument (OMI) volcanic SO_2 dataset: Algorithm description, initial results, and continuation with the Suomi-NPP Ozone Mapping and Profiler Suite (OMPS)" *by* Can Li et al.

Anonymous Referee #2

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Title: New-generation NASA Aura Ozone Monitoring Instrument (OMI) volcanic SO2 dataset: Algorithm description, initial results, and continuation with the Suomi-NPP Ozone Mapping and Profiler Suite (OMPS).

Li et al. reported on a new algorithm to retrieve volcanic SO2 from OMI and OMPS. It will have a great value for users of OMI operational product. I recommend publication after addressing the following comments:

1) Page 3, last line: Is it true that PCA technique allows for studying regional trends in SO2? I would think that even BRD SO2 product would be able to track those trends. Please rephrase.

2) Page 5, l1-2: what is 'significant correlation'? It would be good to give more details. Generally speaking , it is not very clear what is the correlation between SO2 and all other PCs and why it is apparently not introducing biases in the data (also for anthropogenic SO2). An illustration of the correlation coefficients (between the PCs and SO2) and discussion is needed.

3) Equation 2: Please specify what are the Intensity terms (I0, I1, I2), it is not mentioned. Also an analytic dependence with relative azimuth angle is given without any explanation on why it is so. It is not fully clear why the formulation with the different terms is used. I presume it is to reduce the size of the lookup table for the relative azimuth angle (please clarify). It is also not clear why the fitting of R is not performed directly on total intensity (the left term of eq.2). This could be done using a small LUT (because of neglect of gaseous absorption).

4) P6, l 22: it is written "..extrapolate R to shorter wavelengths" while in Fig 1 it reads "longer wavelengths"

5) P6, l24-33: why is a multiple dimension linear interpolation not done (in one step)?

6) P6, l10: the interpolation error could be reduced by having finer LUT grids and / or use higher order interpolation (e.g. spline) without increasing unreasonably the LUT size. The author should justify why this was not preferred. When changing fitting window, in principle a new calculation of PCs should be performed. Is it the case or does the algorithm simply sample the original PCs over the new fitting range? If so, what is the impact of this simplified approach on the results?

7) Ge et al., 2016 cannot be found in the references list.

8) Section 3.1 contains no new information compared to previous work (Li et al., 2013).

The author should consider replacing this section by other results.

9) In section 3.2, it would be interesting to show the inverted reflectivity R and illustrate the wavelength range used (smallest wavelength).

10) Section 3.2 and 3.3: it is not clear why in one case (Kasatochi) the LF and PCA STL results are so close (is PCA really improving the situation?) and for the other case (Sierra Negra) the two algorithms produce results that are so different. Please expend the discussion.

11) Section 3.2, Fig 5: it is surprising to see a strong change between PCA TRU and PCA STL. The expected change in measurement sensitivity from 13 to 18 km should be small and is incompatible with this observed change.

12) P10, l19: the author should describe better what are those challenges.

13) P12, l2: The author states that differences between OMI and OMPS SO2 VCDs are attributed to differences in spatial resolution but, based on Fig 8, it is hard to believe. It is clear that there are patterns (with higher SO2) in the SO2 map which are typically of the same size or larger than the OMPS pixels. Please clarify.

Typos

-page 2, l20: “due the small ..” –> “due to the small..”

-page 5, l7 and 19: the same notation (I) is used for two different quantities (according to the text): sun-normalized earthshine radiance and backscatter radiances at TOA. Please clarify.

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