

Interactive comment on “Continuation of long-term global SO₂ pollution monitoring from OMI to OMPS” by Yan Zhang et al.

Anonymous Referee #3

Received and published: 25 February 2017

In their manuscript “Continuation of long-term global SO₂ pollution monitoring from OMI to OMPS”, Zhang et al. report on a detailed comparison between boundary layer SO₂ retrievals from the two satellite instruments OMI and OMPS. The same PCA algorithm is applied to both data sets and very good agreement in annual means, spatial correlation and temporal correlation is found for three selected regions without the need for radiance or column adjustments.

Satellite observations of pollution SO₂ are an important topic, and with the aging of OMI, continuation of the existing long-term time series by the OMPS instrument(s) is an important topic in order to sustain the ability to track changes in anthropogenic emissions. The detailed comparison of two satellite data sets presented fits well into the scope of AMT. The manuscript is clearly structured and well written and provides quantitative results in both figures and tables. I therefore recommend the manuscript

for publication in AMT after addressing the questions and suggestions listed below.

General point

My only general concern with the paper is that while it provides detailed comparisons for annual means and shows many correlation plots, it does not show the type of figure which probably is the most important one for users of the data sets: direct comparisons of time series. I suggest that the authors add figures showing time series of OMI and OMPS for the three regions on annual, monthly and daily resolution. This enables the readers to judge how similar the seasonality of SO₂ in the two data sets is, how long-term annual trends will be affected by the change in sensor, and how similar the time series over the largest SO₂ hot spots of the world are for the two instruments.

Detailed points

- Please add the recent OMI SO₂ study by Theys et al. to the literature review in the introduction
- Page 4, line 13: Effective cloud radiance fraction of 30% - please give details on how that was determined in OMI and OMPS
- I do not understand why the authors choose to change the selection criteria between OMI and OMPS. They state that the results are very similar in OMI data but then why change the settings? Later, the difference in selection criteria is given as one of the reasons for deviations between OMI and OMPS SO₂ columns. I think that identical selection criteria should be used for both data sets or if that's not possible, a clear explanation should be given why the different treatment is necessary.
- Page 5, line 9: Smaller OMPS than OMI columns are explained by the lower spatial resolution of OMPS, but is that really expected at the gridding size of 0.5 degrees?

- Page 5, line 22: I do not understand why volcanic SO₂ should have different effects on OMI and OMPS – the boundary layer column is probably overestimated, but shouldn't that be similar for both instruments? Please explain.
- Page 7, line 20: Why do the authors expect an effect of the larger OMPS FOV on SO₂ columns over a background region? In my opinion, only the signal to noise ratio is relevant here. Please comment.
- Table 2: Why are the differences between the two linear regressions so large in some cases?
- Table 2: Please add what's on the x-axis and what's on the y-axis in your regressions as otherwise the slope cannot be interpreted. Please also add units to the values in the table. I also would prefer percentages for the number of values within 50 and 75%.
- Table 2: Why are there more values within 50% than within 75%?
- Figure 1: In the figure caption, a bias correction is mentioned which is not discussed in the main text. Please explain and add to description of method.
- Figure 1: Shouldn't that read South Atlantic Anomaly?
- Although the manuscript is well written and the use of English is overall very good, I think that in some parts, another proof reading in particular with respect to use of articles could further improve the language.

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2016-226, 2016.

Printer-friendly version

Discussion paper

