Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2017-161-RC1, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "OMI measured increasing SO_2 emissions due to energy industry expansion and relocation in Northwestern China" *by* Zaili Ling et al.

Anonymous Referee #2

Received and published: 6 May 2017

The manuscript discusses SO2 changes observed by OMI and links them to the national regulations of SO2 emissions. The paper demonstrates again the usefulness of satellite monitoring of air pollutions in China, the world largest SO2 emitter. It is shown that major changes in OMI records are linked to the emission reduction legislation. In general, the paper is well written, although some places require clarification. It can be published after minor revisions.

Comments

1. It is difficult to follow geographical names used by the authors. For example, Midong appears on p. 8, l. 145, without any mentioning of its location. As I understand, it is a district, but then the authors are talking about Urumqi-Midong region (p. 12, l. 241)

and Midong industrial park. Give more information about the cities and regions, provide cities coordinates, show all cities from Figure 2 in Figure 1.

2. P.7, l. 117, Figure 2. There is an explanation why the Urumqi plot is different from the others. Note that the measured SO2 concentration at Urumqi is the highest among all cities shown in Figure 2, while the OMI VCD values are the lowest. It suggests that the monitoring stations are located very close to the emission source (a power plant south of Urumqi?) and the emissions are not very large. The SO2 VCD values of about 0.1 DU are close to the noise level. The emission source is probably not large enough to produce elevated SO2 values in OMI data.

3. P.8, l. 145, Figure 2. SO2 emissions shown in Figure 2 for Midong are under 25 kt per year. OMI is not sensitive enough to see such emission sources, its sensitivity level is 30-40 kt per year (Fioletov et al., 2016). If there is a OMI hotspot in the area, that it is likely that the emissions from the source responsible for that hotspot are not in the emission inventory.

4. P. 19, l. 388-393 and Figure 10. This part is not clear. Papers McLinden et al., 2016, and Fioletov et al., 2016, used OMI Level 2 data merged with the wind profiles to estimate emissions from point sources. As I understand, the authors used Level 3 gridded data. What wind data were used and how the time was determined for grid cells? What is actually shown in Figure 10? The legend is in molecules, i.e., it can be interpreted as total SO2 mass. The caption says that it is in DU. Or, is it the emission rate? If the authors estimated emissions, they should elaborate more on the results. Do the estimated emissions agree with the reported ones? Are there any other sources within the areas shows in the two squares of Figure 10? If so, why are they not on the plot?

5. P.19, l. 393 and p. 20, 398, also Figure 11. The authors are talking about "SO2 burthen" and then "SO2 emission burdens" both in molecules. Are these two terms the same? It they are in molecules, they represent the total mass integrated over an

area and it is more convenient to show them in tones. If they represent emissions, they should be in units of mass per unit of time. Something is missing here.

6. P. 35, Table 1. What are the units in the OMI SO2 VCD column? Are the values in % per year for all columns except the last two where the values are in % per 5 years? Please clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2017-161, 2017.