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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.

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First of all, we would like to thank the reviewer for his/her comments and suggestions which significantly improve the presentations and interpretations in our revised manuscript. Based on the reviewers' comments, we have made major revisions to the manuscript. The revised manuscript and supporting information are attached to Supplement. The reviewers' original comments and our responses are as follows:

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

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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.

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.

Response: We have revised Figure 1. We also added the selected cities shown in Fig. 2 to Figure 1, and marked several "hot spots" regions, including Urumqi-Midong region and Energy Golden Triangle (EGT), Ningdong energy chemical industrial base (NECIB), and Midong energy industrial base (MEIB), in northwestern China 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.

Response: The measured SO2 concentration in Urumqi is the highest among all cities as shown in Fig. 2. However, as the Reviewer noted, the OMI SO2 VCD value in Urumqi was lower than other selected cities. This may be due to the error from systematic biases in OMI-retrieved SO2 VCD. Here we used the level 3 OMI PBL SO2 VCD data produced by the PCA retrievals to estimate the spatiotemporal variation in SO2 pollution in China. The PCA retrievals have a negative bias over some highly reflective surfaces such as many places in the Sahara (up to -0.5 DU in monthly mean).

The systematic bias of PCA retrieval is estimated at ∼0.5 DU for regions between 30°S and 30°N and ∼0.7-0.9 DU in relatively high latitude regions. Located in northwestern China and covered by Gobi desert in the surrounding regions of Urumqi, lower SO2 VCD might be yielded by the PCA retrieval over Urumqi compared with other cities (line 264-278). This point has been added to the revised paper.

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.

Response: We agree with the Reviewer's comments. As shown in Figure 3, the OMI measured SO2 VCD in Urumqi-Midong from 2008 to 2012 was approximately 0.2 DU that was comparable with that in the EGT. However, SO2 emission in Urumqi-Midong was only 4% of that in the EGT in 2012. In particular, SO2 emission in Urumqi-Midong was 0.5% of that in the EGT from 2008 to 2010. This is probably because SO2 emission sources were not reported in emission inventory. Atmospheric removal and advection processes may also contribute to the inconsistence between monitored and satellite observations. These arguments have been added to the revised manuscript (line 287-303).

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?

Response: We thank the Reviewer to point out this confusion. There was an error in previous Fig. 10. In old figure 10 caption and corresponding discussions we talked about SO2 emission burden. In the revised paper Fig. 10 shows SO2 VCD. Corresponding discussions were also revised (line 487-494). The estimated SO2 emissions using the source detection algorithm (Fioletove et al. 2015, 2016), VCDs, and their respective fractions are illustrated in revised Fig. 11. In a new subsection 2.4, we presented the details of SO2 emission estimate using the source detection algorithm developed by Fioletov et al. (2015, 2016) in which wind speed data were used.

We estimated the SO2 burden (in number of molecules in 1026) which represents the total SO2 mass. Again we thank the reviewer to indicate the error in the unit of SO2 burden. Now the revised Fig. 10 shows SO2 VCD with the unit of DU. Revised Fig. 11 shows the estimated SO2 emission with the unit of kt/yr (Fig. 11a and b) and VCD with the unit of DU (Fig. 11c and d) in MEIB and NECIB, respectively.

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.

Response: Please see our last response to the Reviewer. Revised Fig. 11 now illustrates the estimated SO2 emission (Fig. 11a and b) and VCD (Fig. 11c and d) in MEIB and NECIB using the source detection algorithm. In text, "SO2 emission burdens" have been changed to "SO2 emission".

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.

Response: Table 1 presents the annual growth rate for OMI SO2 VCD and economic activities for individual provinces and municipality during 2005-2014 (% yr-1). For OMI SO2 VCD column, they represented annual growth rate of spatially averaged SO2 VCD in the individual regions. In Table 1, the last two columns represented SO2 emission reduction plan during the 11th and 12th Five-Year Plan period, released by Chinese government every five years.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/acp-2017-161/acp-2017-161-AC2-supplement.zip

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