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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Up/Down trend in the MODIS Aerosol Optical Depth and its relationship to the Sulfur Dioxide Emission Changes in China during 2000 and 2010" *by* S. Itahashi et al.

Anonymous Referee #4

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Review on "Up/down trend in the MODIS aerosol optical depth and its relationship to the sulfur dioxide emission changes in China during 2000 and 2010" written by Itahashi et al.

This manuscript is about an analysis of the inter-annual variations of SO2 emissions from China between 2000 and 2010. Authors insisted that the SO2 emissions should increase at a rate of 12.7%/yr until 2005, and have then decreased at an average rate of -3.9%/yr. In order to prove this, authors analyzed fine-mode AOD (AODf), REAS SO2 emissions, CMAQ modeling with the REAS SO2 emissions, and SO2 VCDs (SO2 VCDs from Gottwald and Bovensmann, 2011). This paper is dealing with an interesting

and important topic in the air quality of East Asia. Also, the topic is suitable for ac&p. Therefore, it can be published on ac&p, but it also needs several modifications and improvements, before its publication on ac&p.

Major comments: The topic being handled in the manuscript is important, but this reviewer has four main questions on this work:

1. What is the real use of the CMAQ modeling? The CMAQ modeling was performed only until 2005. Thus, it could not show (or reproduce) the decreasing trend from 2006 to 2010. Then, what is the point of using the CMAQ model in this study? This reviewer recommends to remove it from the analysis. Or, alternatively authors may be able to use only the SO2 bottom-up emission data in this analysis.

2. Authors primarily analyzed AODf in this study, but this reviewer think that a direct quantity to be analyzed is NOT AODf, BUT SO2 VCD. This is because the relation between AODf and SO2 emissions is much more indirect than that between SO2 VCD and SO2 emissions. For example, SO2 is oxidized first through homogeneous and heterogeneous reaction pathways to be converted into sulfate, but still AODf is not sulfate (even if sulfate is a main contributor to AODf). Therefore, this reviewer strongly recommends that authors should use the SO2 VCD (from GOME, OMI, and SCIAMACHY) as a primary variable and AODf as secondary variable in the analysis.

3. The choice of the four study regions in Fig. 1 is somewhat arbitrary. Why did not authors choose the Yellow Sea, even if it is the best region to monitor these increasing/decreasing trends of the SO2 emissions from China? Why is the number 1 box (region) so small? Why did not authors work over the whole East China Sea? This reviewer thinks that the Sea of Japan (box3) would be influenced more by the Korean emissions. Therefore, this reviewer recommends that authors analyze the changing trends of the SO2 VCD and AODf over the four large boxes, for instance, the Yellow Sea, East China Sea, Sea of Japan, and remote Pacific Ocean.

4. This reviewer is not sure that authors are aware of the GAINS-ASIA projection (by

IIASA). According to the GAINS-ASIA projection, the SO2 emissions from China are expected to decrease approximately from 2015 (although it has a little plateau after 2005). Authors should make some comments on this, too.

Some specific comments: 1. Page 21973, line14-16: To use AODf in this study, authors should show a more detailed analysis about the relations between sulfate and AODf (possibly, from their CMAQ modeling results).

2.Page 21976, line 2-4: Again, the SO2 emissions are more directly related with SO2 VCD than AODf.

3. Page 21978, line 17-1: In this reviewer's best knowledge, the CMAQ model does not contain the stratospheric chemistry. The contributions of stratospheric particulate species to AODf would be small. Authors should re-write this sentence.

4. Page 21979, line 13-18: AODf from MODIS and AODf from CMAQ should be compared each other more clearly, even though it was shown briefly in Fig. 2(a) and (b).

5. Page 21981, line17: Again, if authors analyze the SO2 VCDs, then authors can also analyze the increasing/decreasing trends over CEC (over the land. This reviewer believe that it can produce more direct and concrete evidences on the authors' points in this manuscript.

6. Page 21989, Fig. 1: Color scheme should be improved. A pink-like color for >80 Kt/yr grid is not very suitable for such high SO2 emissions.

7. Page 21990, Fig. 2: Characters and scale bars inside the panels are too small to read. Again, why were 3-year averages calculated in Fig. 2(a), but 6-year averages in Fig. 2(b)? How did authors calculate the changes in AODf in Fig. 2(c) and (d)? Subtraction or division? Authors have to explain.

8. Page 21991-21992, Fig. 3 & Fig. 4: Again, characters in Fig. 4 are too small to read. Two figures should be replaced or re-plotted, with the major points 2 & 3 above.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 21971, 2011.