Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-661-RC1, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Spatial and temporal changes of SO_2 regimes over China in recent decade and the driving mechanism" *by* Ting Wang et al.

Anonymous Referee #1

Received and published: 17 August 2018

This paper uses OMI SO2 retrievals to study the effects of emissions and meteorology on SO2 loading over eastern China during 2005-2016. Monthly OMI SO2 from BIRA DOAS retrievals are compared with estimated SO2 emissions from the China Statistical Yearbook. The authors show that OMI-observed SO2 has decreased significantly over eastern China, particularly for areas with the strongest emissions. They use an EOF analysis to demonstrate that the change is not monotonic and has four phases, with SO2 increasing during 2005-2007, decreasing sharply during 2007-2008 and 2014-2016, and only slightly increasing or decreasing during 2008-2013. They also show that the emissions and OMI SO2 are highly correlated over northern part of the country, but less so for southern China. They propose that abnormally dry and

stagnant conditions over southern China during 2008-2013 may have caused a slight increase in SO2 loading, despite continued reduction in emissions. While several studies have examined the recent changes in SO2 pollution over China using satellite data, this study attempts to provide a somewhat different perspective. The conclusion that meteorology may play a fairly prominent role in the inter-annual changes in SO2 over southern China is interesting. The paper is well-organized and figures are mostly clear. However, I am not completely convinced that the emission data used can fully support the conclusions drawn in the study. I'd recommend that major changes be made before the paper can be accepted for publication in Atmos. Chem. Phys.

Specific comments: The authors indicate that the emission data used in this study have very strong seasonal changes in SO2 emissions from China (almost half of emissions in winter, and only 10% in summer). But this is quite different from many previously published emission inventories which generally suggest a much smaller seasonal change (such as HTAP). Also according to a number of previous studies, the residential sector is in general estimated to contribute roughly 10% of all SO2 emissions. This is quite different from what is shown in Figure 9 of this study. The authors may consider using a different, more widely recognized emission inventory for their analysis and check if their conclusion still stands.

It also appears that the emission data used here are on a provincial level (and not gridded) and the authors calculate the emission strength based on the area of each province. Can the authors confirm that? If so, how do the authors calculate total emissions (for example those in Figure 8) for a domain that partially covers several different provinces? Also note that the emissions and SO2 loading can be quite inhomogeneous even within the same province.

It is not clear how the "north" and "south" are defined in this study. One would assume that Cheng-Yu, PRD, and YRD are all part of the "South". But the SO2 time series in Figure 6 indicates that they have different trends during 2008-2013. How would the authors explain these different trends when Figure 10 appears to show generally similar meteorological conditions for the three regions?

The “sudden downward shift of household emissions” in the south is quite surprising. Do the authors have an explanation for this? Or is this simply indicative of methodology change in the emission inventory?

Figure 4: there seem to be some negative SO2 values in the figure? Can the authors confirm that?

Figure 8: What is the unit for emissions? What does each data point represent in the scatter plot?

Figure 10: which area is the vertical profile in (d) for?

Writing: the authors should also make an attempt to improve the writing. Short, simple sentences in some cases may make the paper easier to follow.

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-661, 2018.