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Interactive comment on "Trends in China's anthropogenic emissions since 2010 as the consequence of clean air actions" *by* Bo Zheng et al.

Anonymous Referee #1

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This paper is important documentation of the strong air pollution policies in China in the last decade and their consequences on emissions. It comes at the time when new set of scenarios for the IPCC AR6 Report are being finalized and they should take into account these changes, especially for aerosols where climate impacts are or larger significance.

While the estimated emission trends largely coincide with several recent papers reporting observations, the very rapid decline in SO2, especially in the last 2-3 years, appears even stronger here than some of the observations and it is interesting that there seem to be very little (if any) impact on PM2,5 concentrations in the last few years. Of course

no direct translation of SO2 trends to PM2.5 are expected but bearing in mind that apart from NMVOC all other species are reported to either sightly decline or staying constant, it is a a bit of a surprise. I think this deserves a bit more discussion which might bring the issues like regional distribution of changes or stack hight into it. I'd welcome a general discussion not necessarily very detailed one that would probably fit in the section 4.3, which is very short now.

The other element that is not discussed are the uncertainties. There are several elements which are uncertain in the process of estimating emissions and their trends, including the past (not always good) experience in official data reporting and of course the interpretation of remote sensing data, e.g, the quality or ability of monitoring high stack emissions versus low level sources' changes.

I think the paper is well written and has good illustrations. It also includes all key references that i would know of; referring to my comments above I would suggest to add few for the potential discussion (reference to) of particulate matter trends and relation to the emission trends discussed here.

Few more detailed comments:

Page 2, line 1-6: This paragraph includes reference to short lived climate forces and climate, fine, but I'd suggest to review the text and rewrite it slightly as while the authors list PM, ozone and SLCF then in the following impact statement they do not mention regional climate change. It is mentioned later in bold way how they contribute to local and regional ecosystems impacts as well as climate change...but the latter is really CO2 and CH4 in the first place and not pollutants. Yes, SO2 has an important role but its trajectory is not going to fix (tackle) or screw the climate issue.

Page 2, line 11: ' WHO acceptable standards' - be specific to what you refer, I'd suggest changing the wording and say which standard you mean and give reference. Then also the reference in the next sentence to 'this AQ standard' will be clear.

Page 3, line 23: The reference to China (2018); is this including, referring to actual continuous measurement data or an assessment based on the plant operator and regional reports? I think it makes a bit difference in view of the credibility of these. The ref alone does not appear verifiable. Adding few words and certainty and validation of this would be desired.

Page 4, line 1-2: '...covered all emission intensive industries...' To make the statement stronger I'd suggest to add something about embedded enforcement in this regulation and how did it (or not) worked in the past/so far.

Page 4, line 23-24: It is unclear to what is this referring (the economy standards); is this the sticker value given on produced cars or it is real change in the average on the road? My reading would be this is the sticker value for new sold cars and so not necessarily reflecting the real life change at least for two reasons: Real life consumption is somewhere 20-30% higher and in the urban cycle even more, the fleet composition will affect the true impact of such 'sticker' value change. Few words of clarification woudl be useful in the paper.

Page 7, line 8; I am not able to access this http address. The Sliverlight needs to be installed it says but when i try to do it, I get a message that i actually have it (tried on few browsers) and it is not allowed to install again...but effectively i cannot access and view anything from the link. COudl you check please?

Page 8, line 26: I guess it is not only pains and coatings that contribute to strong growth of NMVOC emissions. The whole chemical industry is responsible and there is more to it than just paints. Please verify and adjust if appropriate.

Page 9, line 13 and 18: the authors use words" 'decreased' and 'exhibited' but i'd say rather 'are estimated to decline' ' were estimated ' ... since these are still estimates not entirely free from uncertainties.

Page 9: There is no specific reference to sectors like bricks and coke manufacturing

for which there are no or very few unpublished estimates of actual emissions so how changes/transformation in these sectors included/evaluated? In general the fact that most reductions were estimated to take place in industry, including small industries, the question about monitoring and enforcement arises. It goes without saying that it is harder to monitor progress in policy implementation over 100s thousands sources vs power plant sector for example. I think the paper needs some, even if brief' discussion of this.

Page 10, line 15: 'old vehicles' - I was wondering what happens to them. Are they scrapped or they move to poorer remote provinces? Is there a record of that? Can you add a statement about the fate of these scrapped vehicles? I think this could reinforce the confidence of readers.

Page 10, section 4.3: As mentioned earlier I'd welcome more discussion here, including uncertainty in OMI retrievals, few more words about the studies quotes as SAT or IM in Table 2 as some of them appear to be OMI related studies but you choose to use the IM component of those - something that was not clear to me first. Then there is issue of PM2.5 observations and virtually lak or very small signal visible there - Example of studies where some of the trends are discussed could include: Fei Yao et al (2018; Sci of Tot Env), Fengchao Liang et al (2018, Sco of Tot Env), Rong Xie et al. (2016, Env International), Haifeng Zhang et al (2016, Env Pollution), Tˆania Fontes et al (2017, J. of Env Management), Xiaoyan Wang et al (2018, Amer Met Soc); Li and Sun (2018, A Economy and Space), C.Q. Lin et al (2018, Atm Env). Also in referecne to the above and Figure 8; few more words of explanation there and uncertainties asspociated with it would be very useful. Actually amazing agreement shown here for recent trends (seems certain) while for 2011 strange 'anomaly' ; how well OMI captures changes in emissions of small low level sources like industries or residential coal versus high level stacks - an issue that potentially can lead to overestimation of strong decline in overall emissions.

page 11, section Conclusion; As mentioned earlier, the language of the paper is like

it all was certain but in reality there is a lot of assumptions made and the 'proof' is a mix of reports (not peer reviewed I assume), peer reviewed studies, measurements, and authors assumptions. Some discussion of uncertainty, even if in qualitative terms would be of great value. Again, the refernce and discussion of impacts on the PM2.5 tredsn (all these actions and plans are done for the PM). How sustainable this reduction is, a rebound likely (CO_2 in 2016 and 2017 was estimated to show revert trend).

Page 22, Figure 7: I am a bit puzzled about the Figure b where For SO_2 only reduction is shown while for other species there is increase from activity driven change. Which sources cause such a change? This is unique to industry it seems, all other charts/sectors show change in the same direction and just the magnitude is different.

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