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## ***Interactive comment on* “The effects of recent control policies on trends in emissions of anthropogenic atmospheric pollutants and CO<sub>2</sub> in China” by Y. Zhao et al.**

**Anonymous Referee #2**

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General Comments: This paper provides a good discussion of recent trends in Chinese emissions of air pollutants and CO<sub>2</sub>. It is well documented work extending to 2010 which is the most recent year for which first dataset of activity statistics are available. The authors discuss thoroughly reasons behind trends making use of the previously published work, including largely their own work, and extending the analysis to the latest few years. While the work does not present an advance in terms of methodologies used for estimating emissions, standard approaches are used, its strength is presentation of several pollutants at the same time and discussing the trends in relation to the developments in their emissions; in that respect it is an update and extension of the work published previously in ACP by Lu et al. (2011). One element that would

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deserve some more attention, or at least discussion using previously published work is uncertainty of the estimates.

Specific comments: Page 24992 Line 7-9: The assumption about constant shares of BC, OC, etc. is not correct. I understand that there might be lack of measurements that drives this but then it should be discussed shortly. As a matter of fact in the last years a number of measurements of emissions from diesel engines shown increasing share of BC in PM emissions from vehicles where EURO standards are met. Also improved combustion in modern stoves shows different PM emission profile with lower shares of OC for example. I believe a comment about the need to consider that in future and justification of the current assumption would be the minimum to consider. Line 15-16: It is not clear to percent of what is referred here: % of capacity, no of units, fuel use, electricity production? Page 24993 Line 6-8: I am wondering if efficiency improvement is the only factor, is the changing share of gas and renewable energy in electricity production not playing a role? Page 24996 Line 6-10: The statement here suggests that the PM emissions of PM from brick sector are well known but I do not think this is the case, there are very few if any on site measurements of emissions, neither regular monitoring, the PM size profiles are also not well known. I think a word of caution in interpretation of these results should be added. Further, while the reduced share of clay bricks produced is very relevant one shall notice the very strong growth in total production of bricks that counteracts the reduction trend to some extent. . .and I personally think that the statistical data on production of bricks is not necessarily reliable and these uncertainties are not discussed here. Page 24996 Beginning of section 4.1: The discussion of the sectoral trends in SO<sub>2</sub> emissions in the last years should include some recently published peer reviewed papers where it was shown too, e.g, Zhang et al (Nature, 2012), Lu et al (ACP, 2011), or Wang et al. (Atmos. Environ., 2011 or 2012, I am not sure which year but it was a paper where remote sensing data for SO<sub>2</sub> were looked at). The whole section does not have a single reference, also for NO<sub>x</sub> and PM giving an impression it was never shown and this a completely new finding. . .I think this is a confirmation and extension/update of some of the previous

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results. In fact the following section 4.2 lists some key relevant studies but I still believe that section 4.1 should include referencing to work that has shown earlier the confirmed here trends. Page 25000 Line 20-23: Very recently there was a paper published in Science discussing the difference in CO<sub>2</sub> estimates based on national and provincial statistics in China, I think it should be referred here too. Page 25002 Line 6: The authors should also look at and refer to the more recent paper by Xu (2011) published in ES&T journal. Page 25003 Line 7: I have not seen many measurements of PM profiles on different types of brick kilns, probably Hoffman kiln would be most relevant (actually I am not aware of any done specifically in China); could the authors back up that statement with a reference to a paper or a study? Pages 25009-25014 I think that sections 5.1, 5.2, 5.3 include a number of repeated statements making the whole discussion very long and I have the impression that some of the important messages are not coming through. I suggest some streamlining, focusing and shortening of these sections avoiding repetition. Page 25036 Fig 7: Few comments: The US numbers have no reference attached to them, please add the source. Why emission trend for US shown only until 2005 and not extended to 2010? The recent EPA (2012) trend report shows nearly 50% reduction of 2005 emissions of SO<sub>2</sub> in the US by 2010; not sure about the PM<sub>10</sub> trend; adding these would be useful.

Technical comments: The paper is really well written so I do not have a lot of comments on formulation. There are few English language issues but I guess they will be picked up by the editors. I feel the title is pretty long but I have no terribly good suggestions how to shorten it; things coming to my mind are “Impact of air pollution policy in China on emission trends in the last decade” but I cannot say I like too. Abstract, Line 15: ‘weakly’ could be replaced by ‘poorly’ Line 22-25: These statement about PM are not quite clear, suggest revisiting the formulation. Page 24987: Line 5-6: Consider moving ‘mainly’ before ‘due’ Page 24988 Line 23: ‘piecemeal’? Page 24989, line 25: Suggest to reformulate: “ Figure S1 in the Supplement shows the source structure used to estimate China’s. . .”

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