

Interactive comment on “The effects of recent control policies on trends in emissions of anthropogenic atmospheric pollutants and CO₂ in China” by Y. Zhao et al.

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Title: The effects of recent control policies on trends in emissions of anthropogenic atmospheric pollutants and CO₂ in China

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We thank for the comments and suggestion from reviewer 2. Following is our point-by-point responses to those comments and corresponding revisions.

Reviewer #2

C11052

1. 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.

Response and revisions: First we should acknowledge that we did not describe the assumption clearly. We assumed the ratios of BC and OC to PM unchanged for industrial and transportation sectors, and applied BC and OC emission factors for residential & commercial sector.

We strongly agree with the reviewer that the share of BC in PM will increase along with improved technology on vehicles, and we've referred to relevant study in the revised manuscript. However, as pointed by the reviewer, it is difficult to get a clear trend for the BC share to PM emissions from vehicles (and OC to PM from residential boilers as well) since current results from domestic measurements are still very limited, particularly if a relatively short period (five years) is concerned like this work. In the revised manuscript, we've taken the reviewer's suggestion and added a comment to justify the assumption and to suggest the future research.

2. Line 15-16: It is not clear to percent of what is referred here: % of capacity, no of units, fuel use, electricity production?

Response and revisions: It is % of capacity, and we've clearly indicated that in the revised manuscript.

3. 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?

C11053

Response and revisions: Section 3.1 evaluates the emission factor changing of coal-fired power plants, thus the declined NOX and CO2 EF expressed as the pollutants per unit of generated electricity are attributed to improved combustion efficiency of coals.

4. 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.

Response and revisions: We agree with the reviewer that either field tests or regular monitoring on PM emission factors (with size profiles) for brick production are very few in China, and we have to rely on the foreign results from RAINS model developed by IIASA (Klimont et al., 2002, see also our response to Question 8). We've added words of caution in use of those emission factors, as suggested by the reviewer.

Since this section analyzes the trends of emission factors instead of emissions, the effects of increased brick production on emissions are not discussed here. We agree with the reviewer that the activity levels for certain sectors including brick production are of considerable uncertainty, since they are not from official statistics but relevant associations (internal data). We've indicated this in Section 2.2 in the revised manuscript.

5. Page 24996 Beginning of section 4.1: The discussion of the sectoral trends in SO2 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 SO2 were looked at). The whole section does not

C11054

have a single reference, also for NOx 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 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.

Response and revisions: As indicated by the reviewer, we presented our results on the emission trends, by species and sector, in Section 4.1, and conducted detailed comparisons between our work and other studies in Section 4.2. Most of recent papers discussing China's emissions including Lu et al. (2011) were referred to in that Section.

In the revised manuscript, we've taken the reviewer's suggestion and indicated in Section 4.1 the consistence between our work and previous work, with added reference Zhang et al. (2012). Regarding Wang et al. (2011), they focused mainly on validation of emissions using satellite data for an earlier year 2005, without any trend analysis, thus we have not included the paper here.

6. Page 25000 Line 20-23: Very recently there was a paper published in Science discussing the difference in CO2 estimates based on national and provincial statistics in China, I think it should be referred here too.

Response and revisions: We believe that the paper mentioned by the reviewer here is not published in Science but in Nature Climate Change. We refer to this paper both in Section 2.2 (for activity level analysis) and Section 4.2, as suggested by the reviewer.

7. 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.

Response and revisions: We agree with the reviewer and the more recent paper is now cited.

8. 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

C11055

aware of any done specifically in China); could the authors back up that statement with a reference to a paper or a study?

Response and revisions: We agree with the reviewer that there are still very few field measurements conducted for PM size distributions of China's brick or lime production processes. In this work the PM profiles of those sources are taken from the RAINS model developed by IIASA (Klimont et al., 2002), following previous work on China's PM emission inventory work (Zhang et al., 2007; Lei et al., 2011; Zhao et al., 2011). We've indicated this in the revised manuscript.

9. 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.

Response and revisions: We've taken the reviewer's suggestion, removed repeated sentences, and shortened Section 5 in the revised manuscript.

10. 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 SO₂ in the US by 2010; not sure about the PM₁₀ trend; adding these would be useful.

Response and revisions: Actually we included the data source of US emissions (USEPA, 2010; now updated to USEPA, 2012). In the original manuscript, we show the trends till 2005 for US because we wanted to compare the emissions of the two country for a same time duration, i.e., 15 years, from 1990 (when the amendments to Clean Air Act was issued) to 2005 for US and from 2005 (when the national policy of energy saving and emission control started) to 2020 for China. Following the reviewer's comments, we check the recent data for US and agree with the reviewer that the emission trends should be extended since significant SO₂ abatement was achieved after

C11056

2005. In the revised manuscript, we've added relevant discussions and provide an updated Fig 7.

11. 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 . . .

Response and revisions: We've taken the reviewer's suggestion and improved the language accordingly.

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C11057

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C11058