

## ***Interactive comment on “Decadal changes in anthropogenic source contribution of PM<sub>2.5</sub> pollution and related health impacts in China, 1990–2015” by Jun Liu et al.***

### **Anonymous Referee #1**

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Driven by the rapid socioeconomic development and environmental policies, air quality in China has changed dramatically over the past few decades. There is a need for studies with long temporal coverage to illustrate the decadal transition of different source sectors. This study investigated the decadal variations of PM<sub>2.5</sub> concentrations and the associated health impact, as well as the transition of sectoral contributions, which is quite useful for future control strategies. The topic of this study is therefore valuable and the entire manuscript is well written. I only have a few minor comments.

1) The discussion of anthropogenic emissions: The authors mainly discuss the impact of emissions for the sensitivities. The simulations in 2000, 2005 and 2015 were based

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on different meteorological conditions. Although the differences of the sensitivities (i.e., with and without industrial emission) were calculated over the same year (i.e., 2005) first and then the comparison was made across the 25-year, the meteorological differences such as in 2000 and 2005 may yield some differences. In another words, if the same year of meteorology was used for all the simulations, the differences of the sensitivities would not be the same as the current results, meaning that the current results actually reflect the impact from meteorology to a certain extent. Therefore, it is better to add some discussions, i.e., in the uncertainty discussions or the sections related to the sensitivity.

2) NCEP-FNL: the simulations of 1990 and 1995 was based on FNL data in 2000. Actually, besides NCEP-FNL, NCEP-CFSR, ERA-Interim can also be used to drive WRF, and these data was available from 1979. I am not asking the authors to redo the simulations using the reanalysis. In fact, I think it might be useful to add some discussions. For instance, the differences of 1990, 1995 and 2000 really reflect the effect of anthropogenic emissions since the meteorology maintains the same, whereas 2005, 2010 and 2015 reflect the compounded effect from the meteorology on top of the emission. Not sure if this can be quantified by the comparison among two sets of three year simulations (1990, 1995 2000 vs. 2005, 2010, 2015). If this is not possible based on current simulations, some information could be added in the uncertainty discussions.

3) I think it is useful to add some discussions between the policy and the control reflected from the emission inventory. Table 1 listed some policy related to emission control. Table 2 listed the emission inventory, which in a way reflected the control outcome. It can be easily identified that the control such as for SO<sub>2</sub> is higher than the planned value (i.e., planned 10% reduction from 2005 to 2010, the SO<sub>2</sub> emission in 2005 and 2010 is 33 and 27.8, respectively, with a reduction of 16%). This means that the emission reduction is larger than the planned action. I think it is better to discuss a little bit of this kind of information, not only for SO<sub>2</sub>, perhaps a few other species as well.

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4)Supplement Line 28: S3). In general, the CMAQ simulated anthropogenic PM2.5 has a good correlation with satellite-derived dust-free PM2.5 The authors stated anthropogenic PM2.5. I think the PM2.5 is based on emission from anthropogenic and biogenic emission together. Have the authors done a test by turning off the biogenic emission? Please verify the statement.

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