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Interactive comment

Interactive comment on "Air quality and health benefits from ultra-low emission control policy indicated by continuous emission monitoring: A case study in the Yangtze River Delta region, China" *by* Yan Zhang et al.

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Main revisions and response to reviewer's comments

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Discussion paper

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We thank very much for the valuable comments from the reviewer, which help us improve our manuscript. The comments were carefully considered and revisions have been made in response to suggestions. Following are our point-by-point responses to the comments and corresponding revisions. Please note that the line numbers mentioned following refer to the clean version of manuscript.

Reviewer #2 [Report #1] (the Interactive Discussion stage):

0. Under the big pressure of air quality improvement, China has been conducting a series of measures to reduce the emissions of air pollutants, including the "ultra-low" emission policy for power and specific industrial sectors. Continuous emission monitoring system (CEMS) has been gradually installed and operated to examine the real emission status of individual plants. Besides, CEMS provided opportunities of improving the understanding of air pollutant emissions for atmospheric science community. Focusing on the YRD, one of the most economically developed region in China, this paper presented an extended study based on the previously developed emission inventory of power sector using the CEMS measurement (Y. Zhang et al., 2019). The authors applied air quality model and evaluated the YRD emission inventory with CEMS incorporated. They further combined the air quality model and exposure-response model, and analyzed the benefit of ultra-low emission policy on air quality and health. It provides the evidence of emission data improvement and the environmental health implication of current air pollution control policy, thus it fits the scope of Atmos Chem Phys. In general the paper is well organized and clearly written. I would suggest its acceptance for publication, with minor revisions or discussions conducted on the following issues.

Response and revisions:

We appreciate the reviewer's positive remarks on our manuscript.

1. Abstract. Some sentences are unclear and should be rewritten. Lines 49-50: “11%, 7% and 2% of SO2, NOX and PM” for which case?

Response and revisions:

We appreciate the reviewer’s reminder. The language of abstract has been improved, and the numbers mentioned by the reviewer refer to Case 2, as we stated in line 52 in the revised manuscript.

2. Lines 155-157, did Gao et al. (2018) (and some other studies maybe) include the CEMS data for emission inventory development and stress the ultra-low emission policy?

Response and revisions:

We thank the reviewer’s comment. Gao et al. (2018) (and most other studies at the national scale) applied MIX (Li et al., 2017) or MEIC (Zheng et al., 2018), and CEMS data were not comprehensively incorporated. The target year in Gao et al. (2018) is 2013, in which the ultra-low emission policy was not conducted yet.

3. Lines 226-234. It seems that you apply the inventory by Xia et al. (2016) but the spatiotemporal distribution of MEIC? Why not use MEIC directly?

Response and revisions:

We thank the reviewer’s comment. We applied this method basically for two reasons. First, as one of our previous studies, Xia et al. (2016) calculated the annual emissions by province and species for China, using a similar “bottom-up” method with MEIC. Given the difference choice of emission factors for certain sources, there were differences in the amount of emissions for some provinces and sectors between the two inventories. Second, MEIC provided the emission data of total industry but did not report the specific information for industrial boilers, cement or iron & steel factories when this work was conducted. As the ultra-low emission policy was assumed to be conducted for industrial boilers, cement and iron & steel factories in this work (Case

4), we needed to calculate the emissions exactly for the same categories in the base case (Case 2). In this work, therefore, we applied the provincial-level emission data by Xia et al. (2016) and obtained the gridded data according to the spatial distribution of emissions by MEIC.

4. Lines 251-253. Some information is missing here. Case 4 assumes both power and industrial boilers would meet the ultra-low emission limit. Do the two sectors share the same limit? In your previous work (Y. Zhang et al., 2019) you analyzed the emission limit for power sector with CEMS incorporated, but how about references for industrial boilers? Description should be given here.

Response and revisions:

We appreciate the reviewer's important remarks and acknowledge the information was unclear in the original submission. In Case 4 we assumed industrial boilers, cement, and iron & steel factories would meet the requirement of ultra-low emission policy. The limits of flue gas concentrations were determined according to available national or local ultra-low emission standards issued recently (Yang et al., 2021). Thus the limits vary by sector and are different from those for power sector. We have summarized the ultra-low emission limits and standards by sector in a new Table S2 in the revised supplement, and added the information in in lines 267-271 in the revised manuscript.

5. Table 1 seems unnecessary, while the emission data in difference cases are more important (Table S2). Could you combine Table 1 and Table S2?

Response and revisions:

We thank the reviewer's comment. We combined Table 1 and Table S2 in the original submission as a new Table 1 in the revised manuscript.

6. Line 326-328. The difference for SO2 was 10%, did this contradict the statement in lines 45-46 in the abstract: ": SO2, NO2, O3 and PM2.5 concentrations compared to those of Case 2, our base case for policy comparisons, were less than 7% for all

pollutants"?

Response and revisions:

We thank the reviewer's comment. The 10% difference for SO2 was between Case 1 and Case 2, while the statement in abstract indicated the difference between Case 2 and Case 3. Thus they did not contradict each other.

7. Lines 359-360. I cannot quite understand the numbers. You are comparing NMBs and NMEs for two cases, then what did the two groups of numbers exactly stand for?

Response and revisions:

We thank the reviewer's comment. To avoid the confusion, the sentence was rewritten as: "The NMBs between the simulation and observation for the two cases ranged from -34.5% to -6.4%, and NMEs from 23.1% to 37.1%, respectively" in lines 389-390 in the revised manuscript. The wrong numbers has also been corrected in the sentence.

8. Line 367. Is it commonly known that the YRD is under VOC-limited regime in terms of O3 formation? At least some literatures should be provided here.

Response and revisions:

We thank the reviewer's comment. Some recent studies revealed or confirmed that most of YRD was under VOC-limited for O3 formation (Wang et al., 2019; Yang et al., 2021). We revised the sentences as: "As most of YRD was identified as a VOC-limited region for O3 formation (Wang et al., 2019; Yang et al., 2021)" in lines 397-398 in the revised manuscript.

9. Lines 435-437. Did the "ultra-low" emission policy include the limit of VOCs? Or did CEMS include the information of VOCs? Some explanations should be given here.

Response and revisions:

We thank the reviewer's comment. CEMS does not report VOC concentration in the flue gas, and current "ultra-low emission" policy does not include VOC limits, either. We have added the information in lines 474-475 in the revised manuscript.

10. Lines 539-540: Any reason could be provided?

Response and revisions:

We appreciate the reviewer's comment and it is the same as the Question 5 from Reviewer #1. We expect the difference could result mainly from two issues. The first is the varied pollution levels for different study regions. This work evaluated the health effect from PM2.5 exposure for the YRD region, while Maji et al. (2018) focused on 161 typical cities in China. As one of the most developed and industrialized regions in China, the YRD suffered higher PM2.5 pollution level than the national average, leading to the larger fraction of premature death due to PM2.5 exposure. The second is the choice of different mortality rates. The disease-specific baseline incidence rates in Maji et al. (2018) were derived based on the national disease specific mortality in the dataset of GBD study for 2015, while the baseline age-gender-disease-specific mortality rates for the five diseases in this study were obtained from the Global Health Data Exchange database (GHDx). The numbers in the latter were commonly higher except for LRI, resulting in the higher estimates of death rates exposed to PM2.5. We have added the information briefly in lines 585-590 in the revised manuscript.

11. Language issues. Line 119: "environmental concentrations"? Line 125: delete the word "itself"; Line 504: change "sectors" to "boilers"; Lines 531-534: rewrite the sentence

Response and revisions:

We thank the reviewer's reminder and correct the language errors.

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