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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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Title: 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

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

0. This paper evaluated the potential benefit of the ultra-low emission policy on both air quality and human health in the YRD region. No novel technique was developed, or new scientific finding was reported. The results can still provide some scientific reference for related emission control policy and health burden caused by air pollution over the YRD region. Overall, this paper is well written, but more description in the methodology is still needed. A major revision is suggested, and my specific comments are listed as follows.

Response and revisions:

We appreciate the reviewer's remarks and have revised the manuscript according to the reviewer's specific comments, as summarized below.

1. Line 55, in the abstract section, "874 years", but according to on paper Table6, it should be "8744 years of life loss".

Response and revisions:

We thank the reviewer's reminder and the error has been corrected.

2. In the methodology section, please generally introduce the method of how to incorporate the CEMS data and cite the references which have the detailed description.

Response and revisions:

We appreciate the reviewer's important comment. The main principle of the method incorporating the CEMS data has been described and the reference has been provided in lines 251-260 in the revised manuscript: "Besides the commonly used method, Y. Zhang et al. (2019) developed a new method of examining, screening and applying CEMS data to improve the estimates of power sector emissions. CEMS data were collected for over 1000 power units, including operation condition, monitoring time, flue gas flow, and hourly concentrations of SO2, NOx and PM. The emissions of individual unit were calculated based on the hourly concentrations of air pollutants obtained from CEMS and the theoretical flue gas volume estimated based on the unit-level information mentioned above. Compared to MEIC, a larger monthly variation in emissions was found based on the online emission monitoring. Details can be found in Y. Zhang et al. (2019)."

3. Why still using the old version of the CMAQ model? The current CMAQ model (v5.2 or v5.3) has incorporated several trace gas chemistry schemes (e.g., bromine and iodine), which can influence the O3 simulation importantly.

Response and revisions:

We thank the reviewer's important comment. We acknowledge that application of the old version of CMAQ is a limitation in this work. In our recent work (Lu et al., 2020), we tested the model performances for the YRD region with different versions of CMAQ, and found the impact of CMAQ version on simulation for difference species was inconclusive. Generally, the PM2.5 simulation was improved with newer version, but the O3 simulation was not, particularly for the periods with relatively low concentrations. The test revealed the necessity of further intercomparison and evaluation studies for the region. We have added the discussions in lines 419-425 in the revised manuscript.

4. Line 285, why not using the GEMM model in this work?

Response and revisions:

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Discussion paper

We appreciate the reviewer's important comment. Indeed the choice of health model (or Concentration-Response function, C-R function) is of great impact on the result of health effect analysis. In our most recent work, actually, we compared the premature mortalities estimated with IER and GEMM in 2030 energy saving and emission control scenarios for China (Yang et al., 2021). The larger GEMM hazard ratio as well as higher baseline mortality rates resulted in higher PM2.5-related mortalities than IER. Therefore, application of IER got a relatively conservative estimate for the health effect of air pollution and the benefit of emission controls. As the range of PM2.5 exposure in China could be larger than that considered in GEMM (84 μg/m3), we believed IER would applicable for the country. It has been relatively well developed for mortality estimation and has been widely used in quantifying the impact of environmental policies and air quality standards on health burden (Li et al. 2019; Yue et al. 2020; Zheng et al. 2019). We have added the explanation in lines 292-297 in the revised manuscript.

5. In the methodology section, more definition and explanation of YLL was needed. In the health analysis, what is the different meaning of analyzing attributable death and YLL, respectively?

Response and revisions:

We thank the reviewer's comment. YLL represents the years of life lost because of premature death from a particular cause or disease. It is calculated from the number of deaths multiplied by a standard life expectancy at the age at which death occurs. Death rates could not provide a comprehensive picture of the burden that deaths impose on the population, thus YLL caused by PM2.5 exposure was estimated in this study to help describe the extent to which the lives of people exposed to air pollution were cut short. We have added such information in lines 299-303 in the revised manuscript.

6. Line 296, "Pop represents the exposed population in the age-gender-specific group in grid cell", but how to get these data for each grid wasn't mentioned in the context. E.g., did the age distribution of different provinces also come from yearbooks? Was

the ratio of various age groups was the same for all the model grids?

Response and revisions:

We thank the reviewer's reminder. The gender distributions of different provinces were obtained from provincial yearbooks. As the high-resolution spatial pattern of age structure was unavailable when the study was conducted, we assumed the age structure was the same for all the model grids (Gao et al., 2018). We have added the information in lines 326-329 in the revised manuscript.

7. In the model result evaluation, the authors used different statistical indicators for air pollutants and meteorological parameters because all used indicators were widely applied to both air pollutants and meteorological parameters in other studies. So the same indicators are suggested to be used for both, or the author needs to explain the reason.

Response and revisions:

We thank the reviewer's comment. For meteorological parameters we followed Emery et al. (2001) and applied the main statistical indicators and benchmarks suggested in the study. For air quality modeling, R, NMB, and NME are mostly applied for comparison between simulation and observation, thus they were adopted in this work.

8. Line 303, Table S4 does not have the information of LRI mortality rate

Response and revisions:

We thank the reviewer's reminder. LRI is a common disease among young children, thus we applied uniform mortality rates regardless of age. The LRI baseline mortalities were 13.7 and 11.4 cases per 100,000 for male and female, respectively. We have added the information in the caption of Table S4.

9. Line 441, based on the comparison between Case 3 or 4 and Case 2, it was concluded that the higher relative concentration change happened in July because of the

faster response and high oxidative condition in this month. However, from the comparison of PM2.5 in Case 5 and Case 2, the larger concentration change also appears in January. For SO2 in Case 3 or 4, the decrease concentration in July is also not the largest. The decrease percentage is the largest, but it may due to the lower concentration in July. The analysis is needed to be modified here.

Response and revisions:

We thank and agree the reviewer's comment. The changes in the absolute concentrations of SO2 and PM2.5 were not always the largest for summer, but those in percentages were. To be more accurate, we have revised the text as "larger relative changes were found for SO2 and PM2.5 in summer" in lines 481-482 in the revised manuscript, and "the relatively low concentrations in summer also contributed to the largest percentage changes in SO2 and PM2.5 simulation for the season" in lines 489-491 in the revised manuscript.

10. The difference in Figure 3 and 4 were calculated by (Case 2-Case 3 or 4). Because the formula used in the previous analysis in Table 3 is (Case 3 or 4-Case 2), so consistent formula was suggested to use in Figures 3 and 4.

Response and revisions:

We thank the reviewer's reminder. We have revised Figures 3 and 4 using the consistent formulas with Table 3. The captions of the two figures have also been corrected accordingly.

11. Line 476, the author argued that the modest change of NO2 in central YRD (Shanghai, northern Zhejiang, and southern Jiangsu) caused an apparent enhancement of O3. But from Figure 4 (Oct), the O3 in south Anhui also increased, but the change of NO2 here is much larger than that in Shanghai. How to definite the "modest"? More analysis and a better explanation are needed.

Response and revisions:

We appreciate the reviewer's important comment. We acknowledge that the original explanation did not cover all the important information, and the word "modest" was confusing. As mentioned earlier in the paper, most of YRD was identified as a VOC-limited region for O3 formation. Here the modeling results show that the central YRD (Shanghai, northern Zhejiang, and southern Jiangsu) was the most influenced by the mechanism. Compared to other areas (e.g., southern Anhui as pointed by the reviewer), the relatively less reduction in NOX (and thereby NO2) would lead to significant enhancement of O3 (note much more reduction in NO2 resulted in similar enhancement of O3 in southern Anhui for October). The comparison implies that the O3 formation in central YRD was more sensitive to NOX emission abatement than other VOC-limited regions in YRD. Therefore, more efforts on VOC emission abatement would be required for O3 pollution control in central YRD. We have revised the explanation, as shown in lines 516-523 in the revised manuscript.

12. In the exposure analysis section (3.2.1), is there any basis for choosing these concentrations (35, 45, 55 μg/m3) as interval value?

Response and revisions:

We thank the reviewer's comment. The main reason is that 35 μg/m3 is the annual PM2.5 concentration limit in the current National Ambient Air Quality Standard (NAAQS) for China. We thus apply an interval of 10μg/m3 based on that limit, as the C-R function is usually expressed as %/(10 μg/m3). We have added the information in lines 545-547 in the revised manuscript.

13. Line 610, "The fractions of both avoided deaths and YLL were clearly higher for Shanghai and part of Zhejiang, implying..." From which table or figure can you get this conclusion? Figure 9?

Response and revisions:

We thank the review's comment. The information can be obtained from Tables 5 and 6.

As can be inferred from the two tables, the fractions of Shanghai and Zhejiang to total YRD for both avoided deaths and YLL increased clearly from Case 3 to Case 4. We have added the information in lines 661-664 in the revised manuscript and deleted the phrase "part of" to avoid confusion.

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