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Atmospheric Chemistry and Physics
Discussions

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.

Anonymous Referee #3

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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.

Specific comments: 1. Line 55, in the abstract section, "874 years", but according to

Table6, it should be "8744 years of life loss".

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.

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.

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

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?

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?

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 is suggested to be used for both, or the author needs to explain the reason.

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

9. Line 441, based on the comparison between case3 or 4 and case2, 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 case5 and case2, the larger concentration change also appears in January. For SO2 in case3 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.

10. The difference in Figure 3 and 4 were calculated by (Case2-Case3 or 4). Because the formula used in the previous analysis in Table 3 is (Case3 or 4 - Case2), so consistent formula was suggested to use in Figures 3 and 4.

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 Figure4 (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.

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

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?

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