

Atmos. Chem. Phys. Discuss., referee comment RC2
<https://doi.org/10.5194/acp-2020-953-RC2>, 2021
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Comment on acp-2020-953

Anonymous Referee #2

Referee comment on "Modeled changes in source contributions of particulate matter during the COVID-19 pandemic in the Yangtze River Delta, China" by Jinlong Ma et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-953-RC2>, 2021

General Comments:

Ma *et al.* present a model analysis of changes in PM_{2.5} during the lockdown period associated with the COVID-19 pandemic in the Yangtze River Delta, China. The model is compared to observations of total PM_{2.5} in the region, and to a more limited set of speciated PM_{2.5} data from a specific site. The topic is timely and of interest to the air quality and atmospheric chemistry communities, and warrants publication in ACP.

The paper is mainly a modeling study, with relatively little reference to observations. The authors can do more to make the effects of the lockdown on PM_{2.5} clear from their model based analysis. Recommendations are below in the specific comments. The word "model" should appear in the title, as it is not clear to the reader until well into the paper that all of the analysis and attributed changes are based on the model rather than any analysis of the observations. A more appropriate title would be something like "Modeled changes in source contributions of particulate matter"

The authors should pay attention to the specific comments and technical corrections below. They may wish to have a native English speaker proofread the paper for grammatical corrections, although the writing itself is certainly clear.

Specific Comments:

Line 41-42: Why do statistical methods only address primary PM ?

Line 67: The term "PM" is used with primary sources (PPM), but the term aerosol is used with secondary sources (SIA). Suggest choosing either PM or aerosol, but not mixing the two.

Line 79-81: The authors probably mean "due to considerable uncertainties". Beyond the grammar, however, it is difficult to believe that SOA is <10% of PM_{2.5}. Huang *et al.* 2014 (given below, also in the reference section) show that organic matter accounts for 48% of PM in Shanghai. Is there a more recent reference showing a much smaller contribution of SOA?

Huang, R.-J., *et al.*, *High secondary aerosol contribution to particulate pollution during*

haze events in China. Nature, 2014. **514**(7521): p. 218-222.

Line 115: It's not clear what acceptable means here, but the quantitative measures are given above, so suggest simply omitting the last sentence of this paragraph.

Line 121-122: The sentence is somewhat misleading in that it implies that the figure compares observations of speciated $PM_{2.5}$ to the model output. The comparison is between observed and predicted total $PM_{2.5}$ mass.

Figure 1 would be far more convincing if it showed the time series of predicted total $PM_{2.5}$ mass for case 1 (business as usual) and case 2 (lockdown), as well as the difference between the two cases. It is not obvious from looking at this figure alone that the lockdowns had any influence on $PM_{2.5}$.

Line 122-123: The authors should plot predicted vs. observed $PM_{2.5}$ during each period rather than just providing the time series for the comparison. A slope of a linear fit to this scatter plot would provide a quantitative measure of model performance. Similarly, a slope of the case 1 prediction against the observations would show how well this case performed prior to the lockdowns, as well as how much is overpredicted the observations during the lockdown.

Figure 3: The labeling is not quite clear. It appears the authors mean "percent concentration change" rather than "relative concentration" for the circles that are plotted against the right axis.

Figure 4: Why is Case 1 (base case, no reductions) not also shown? It would seem the business as usual case is as important to show as the reduced emissions case. Also, all of the relative differences are negative. Why? Shouldn't the residential sector increase while transportation and industry decrease?

Perhaps what would make the above more clear is the apportionment among sources for case 1 and case 2 – i.e., what fraction of $PM_{2.5}$ is attributable to each source in each case. This measure would likely show that residential was a larger overall contributor for case 2.

Figure 5 is also difficult to read. The authors should consider using a pie chart format in which the contribution from each sector is shown as a wedge in a pie for case 1 and case 2. This would make clear how the sources changed between business as usual and lockdown policies.

Technical Corrections:

Title: Should read "the COVID-19 pandemic"

Line 35: "In the Yangtze River Delta ..."

Line 41: "... is based ..."

Line 50-51: Therefore, updated source apportionment information is needed to support further reduction policy.

Line 57-58: "changes, and these studies cannot be used ..."

Line 110: replace "were met" with "met"

Line 121: "Figure 1 shows predicted ..."

Line 146: Replace "decreasing ratios" with "decreases"

Line 148: Replace "reduced more" with "more reduced"

Line 153, 155: Replace "decrease" and "decrease ratio" with "percent decrease"

Line 163: Replace "Below" with "The next section"

Line 199: Replace "traffics" with "traffic"