

## ***Interactive comment on “Assessment of meteorology vs control measures in China fine particular matter trend from 2013–2019 by an environmental meteorology index” by Sunling Gong et al.***

**Sunling Gong et al.**

gongsl@cma.gov.cn

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General comments Based the MM5/CUACE model and observational data, an environmental meteorological index EMI<sub>2.5</sub> and an assessment framework were developed in the present work. The roles of meteorology and control measures in China fine particular matter trend from 2013 to 2019 were separately assessed. It was found that the nationally averaged PM<sub>2.5</sub> concentration had declined about 50

Specific comments 1. The current framework considers only the effects of emissions

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and meteorological conditions on PM<sub>2.5</sub> change. Actually, atmospheric chemistry plays a crucial role in shaping PM<sub>2.5</sub> concentration. Can the authors include this factor in the framework? It would be more nice and convincing. Otherwise, the conclusions could not be so solid.

Reply: The same question was raised by another referee. The answer to the question is as follows:

The EMI index was focused explicitly on three major physical processes of iTran, iAccu, and iEmid that are closely related to the meteorological influences. However, the secondary formation of aerosols is implicitly considered in the EMI as the three major physical processes are calculated from the concentrations of aerosols (C) as indicated in Equation (3), which are resulted from the full processes of chemical mechanisms or “a highly parameterized method” that accounts for the secondary aerosol formations. Furthermore, we have done a comparison of simulated PM<sub>2.5</sub> with full processes and the EMI with the parameterized method, and the correlation coefficients between them range 0.72 to 0.93 for the regions in this study. The limitation of non-inclusion of explicit chemical terms in the EMI is pointed out in the manuscript.

2. In the model simulations, both primary and pre-cursor emissions of PM are based on the 2016 MEIC inventory. However, the present work focused on the tendency of PM<sub>2.5</sub> from 2013 to 2019. Did the authors use the same inventory for every year or change the inventory year by year?

Reply: In order to isolate the meteorological impacts, we have used the 2016 MEIC emissions throughout the simulations, i.e. from 2013 to 2019, resulting the differences caused by meteorological changes only.

3. Table 2 shows the observed PM<sub>2.5</sub> difference between 2019 and 2015, why not 2019 and 2013 to be consistent with the title and other parts?

Reply: The same question was raised by another referee. The answer to the question

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is as follows:

There are two issues here that prompted us to use two different time intervals for the comparisons in the paper. The first issue is the completeness of the network observational data series of PM<sub>2.5</sub> in China. The systematical and network observations of PM<sub>2.5</sub> started in China from 2013. However, it took about two years (until 2015) to develop to the current status. Number of monitoring stations national-wide in 2013 was less than 900, reached to about 1400 in early 2015 and maintained the same up to now. To show the completeness of the observed PM<sub>2.5</sub> time series and for most part of the paper, we made the comparison starting from 2013 as graph illustrations. The second issue is the data consistence and policy relevance of the assessment. Statistically, because the national observation site numbers are relative constant from 2015 to 2019, it makes more sense to use the 2015-2019 data for numerical assessment such as those shown in Table 2. The use of 2015-2019 data for Table 2 was also motivated by the introduction of the Environmental Protection Law of People's Republic of China in January 2015. For the regulation assessment point of view, the comparison Table 2 was in line with the date of the law introduction and the impact assessment by emission changes was more relevant to the interests of management to show how effective the law was.

4. In Eq.(3) why the integration is just over dz, not dx dy dz?

Reply: The Equation (3) was introduced to account for the column loading of aerosols in the PBL, that contains most of the aerosol masses, for a grid. The dx dy, i.e. advection terms, is done in the 3-D transport part of the model.

Technical corrections There are numerous typos need to be corrected. I suggest the authors carefully proof read the manuscript to make sure all language problems are fixed.

1. Page 8 Line 2: "Results and Discussion secession" should be "Results and Discussion section". This should be corrected all over the entire manuscript.

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Reply: Thanks. Corrected!

2. Page 8 in Figure 2: "Natiional" should be "National"; "t0" should be "to"!

Reply: Thanks. Corrected!

3. Page 11 Line 17: "sing" should be "sign"; Line 18 "vise visa" should be "vice versa"

Reply: Thanks. Corrected!

4. Page 12 Line 11: I think K<sub>x</sub>, K<sub>y</sub> and K<sub>z</sub> should be turbulent diffusion coefficients; Line 16-17: "on and to" should exchange position; Line 19 "compared" might be "computed"

Reply: Done for the "on and to" exchange. Line 19 "compared" was not changed as we indeed meant to compare.

5. Page 13: Line 5 and Lines 10-11. The explanation on /EMI(p)<sub>2.5</sub> is not consistent; Line 13 "Figure 2" should be "Figure 3".

Reply: Thanks. Corrected!

6. Page 14: Lines 7 to 9, why January 2103 (should be 2013!) to January 2016?; Line 15 "combined" should be "combine".

Reply: Thanks. Corrected!

7. Page 17: Lines 3-10, I could not understand what the authors meant. Line 8 (Wang et al.) is not a proper citation; Line 13 the introduction of Figure 4 is not consistent with the actual caption in Page 18 Lines 2-4.

Reply: As in the Question 2 above, we have used the MEIC emissions for 2016 for all simulations, but applied a monthly variation based on Wang et al. 2011: (Verification of anthropogenic emissions of China by satellite and ground observations). We have checked with the authors of this paper and been assured that the monthly variations were discussed.

We have corrected the Line 13 to "spatial distribution of correlation coefficients

C4

between. . .”

8. Page 19: In Figure 5 the subcaptions of (c) and (d), “contributions” should be “contributions”.

Reply: Thanks. Corrected! The caption for Figure 5 has been re-written.

9. Page 22: Line 4 “2103” should be “2013”; Line 6 (Wang et al.) is not a proper citation; “to” should be removed from “pointing out to”.

Reply: Thanks. Corrected!

10. Page 25: Line 20, “favorite and un-favorite” might be “favorable and unfavorable”. This is also true for other statements hereafter.

Reply: Thanks. Corrected: 4 in total.

11. Page 29: Line 14 “2105” should be “2015”!

Reply: Thanks. Corrected!

12. Page 30: Line 10, to judge whether the meteorological conditions are favorite or not to. . .

Reply: Thanks. Corrected!

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