

## ***Interactive comment on “Effects of air pollution control policies on PM<sub>2.5</sub> pollution improvement in China from 2005 to 2017: a satellite based perspective” by Zongwei Ma et al.***

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Responses to RC1

Please find the supplement for the revised manuscript and supplementary materials. We have highlighted the revisions in red font in the revised manuscript.

Comments from RC1: This is an interesting paper putting forward a historical perspective on PM<sub>2.5</sub> surface concentrations in China. The authors propose a statistical method that relates satellite observed aerosol optical depth (AOD) over China to measurements of PM<sub>2.5</sub> at the surface. The authors use the years 2013 and later when

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both satellite and surface measurements were available to train their method. Prior to 2013 there was no ground based network to speak of in China. Then the method, essentially a multivariate regression of information on the atmospheric state, is applied to infer surface PM<sub>2.5</sub> for the entire 2005-2017 period from MODIS AOD. This allows the authors to evaluate the effectiveness of the various Chinese air pollution control policies that have been applied in this period. Based on the satellite-estimated PM<sub>2.5</sub> trends, the authors claim a “periodic victory” for Chinese policies to clean up the air. I think the paper addresses a relevant topic that is appropriate for publication in ACP, but I have concerns about the method, which is not well described in this manuscript. Also for non-Chinese readers, it would be necessary to clarify what the various Chinese policies consisted of. We read very little about what measures were actually implemented, and how they may have had an effect. This is important information to share with an eye on other countries going through a rapid development phase, and wishing to limit the effects of air pollution. The authors owe it to the world, so to speak.

Response: We would like to thank the reviewer for his valuable comments. We have added descriptions of the method. We also incorporated descriptions of major air pollution control measures in the main text. Please see the following responses for details.

(1) The abstract is not very clear. There are many abbreviations referring to policies applicable in certain periods only that will not be immediately clear or well-known to the wide readership of ACP. The authors should rewrite their abstract with a focus on storytelling how Chinese PM<sub>2.5</sub> changes over time between 2005 and 2017, and why. The results summarized should be presented in quantitative fashion.

Response: The abstract has been revised according to this comment and the comment from another referee. We simply explained how these policies can impact the PM<sub>2.5</sub> pollution. The trends analysis was revised and presented in a quantitative way. A brief description of the model has been added. Please see Abstract in P2.

(2) The method to infer PM<sub>2.5</sub> from MODIS AOD is explained only very briefly with

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repeated reference to a previous paper by the same authors. For this paper to stand alone, the authors should provide much more detail on how their statistical “two-stage” method works, and how robust the method is. The authors should briefly explain what drives the relationship between PM2.5 and AOD. Which parameters explain most of the variance and why. Specifically: Provide the equation establishing the relationship between AOD and surface PM2.5 Explain how the fit parameters have been derived, and discuss the orthogonality of the various explanatory terms (humidity, boundary layer height, T, : : ) Discuss the temporal resolution of the relationship (“each year’s model”) Discuss the differences and agreement with the model-scaling approach Since the method relies on the quality of the MODIS and surface PM2.5 data, these aspects should be discussed as well.

Response: According to the comments, we have made corresponding revisions as follows: 1) We added details about the equations of the two-stage model, please see P8-P9; 2) In original manuscript, we have discussed the model performance for each year, see Lines 9-21, P10 in Section 4.1. In revised manuscript, we added the provincial fixed effects, model fitting, and CV results of the first-stage LME model for each year in Tables S2-S5 (Supplementary Materials). And we have discussed it in Line 22, P10 Line 5, P11. 3) A brief description of scaling method was added (Line 25, P3 Line 1, P4). We compared the model performance with previous scaling method studies, see Lines 11-17, P11. 4) Lines 23-25, P7 shows the quality of MODIS AOD data. Lines 17-21, P7 added the issues of PM2.5 data quality.

(3) Related to the lack of information on the method, are the terms “random intercepts” and “random slopes” mentioned on page 7. Without reading the previous paper by the authors in a different journal, it is entirely unclear what these terms mean. It shows that this manuscript cannot be read on its own, which is not the standard for a paper in ACP.

Response: We added details about the equations of the two-stage model, please see P8-P9.

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(4) Related to the trends, it is unclear how the trends were determined. Did they use a linear model of the form  $y = a + b t$ , how did they deal with seasonality, weighing of sparsely sampled months, etc.? They need to provide more detail and also include figures showing the temporal evolution of the PM2.5 estimates, along with the satellite data, and ground-based observations for one or a few particular locations.

Response: We added details about the method for trend analysis in Lines 14-24, P9. For seasonality, we have described how we dealt with it in our original manuscript. See Lines 10-12, P9. We deseasonalized the monthly PM2.5 time series by calculating the monthly PM2.5 anomaly time series for each grid cell to remove the seasonal effect.

(5) Section 2 on the policies is too technocratic. We read about the official titles of the policies, but the authors should make clear not just in (the valuable but too long) Table 1 but also in the main text what the policies consisted of. I realize they cannot be exhaustive all the time, but they should provide an assessment of what they think were the most effective measures taken under a certain policy, and the evidence to back this up. This is important to make a convincing case, and allows others to learn from the policies taken. One suggestion is to come up with a figure showing a timeline of the various measures and their anticipated effect on Chinese PM2.5 levels. Such a figure could then later be confronted with the observed PM2.5 evolution, and tell the story whether measures have been effective.

Response: Revisions have been made according to the comments. First, we described major air pollution control measures, corresponding achievements, and how these policies were associated with PM2.5 pollutions in the main text. Such as Lines 4-13, P13; Lines 22-27, P13; Lines 14-21, P14; Line 23, P14 Line 3, P15; Line 23, P15 Line 22, P16. Second, a new figure (Figure 6) to show the overall national and regional trends for different periods and corresponding air pollution control policies. And we moved a table from supplementary materials to the main manuscript (see Table 2), which corresponds to Figure 6.

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(6) P2, L3: polices-> policies

Response: We have corrected this mistake. See Line 3, P2.

(7) P3, L13-14: the citations are quite China-centric. Consider citing studies on SO<sub>2</sub> and NO<sub>2</sub> trends over China from non-Chinese groups, e.g. Itahashi et al. [2012], Krotkov et al. [2016], Miyazaki et al. [2017]. Suggested references: Krotkov, N. A., McLinden, C. A., Li, C., Lamsal, L. N., Celarier, E. A., Marchenko, S. V., Swartz, W. H., Bucsele, E. J., Joiner, J., Duncan, B. N., Boersma, K. F., Veefkind, J. P., Levelt, P. F., Fioletov, V. E., Dickerson, R. R., He, H., Lu, Z., and Streets, D. G.: Aura OMI observations of regional SO<sub>2</sub> and NO<sub>2</sub> pollution changes from 2005 to 2015, *Atmos. Chem. Phys.*, 16, 4605-4629, doi:10.5194/acp-16-4605-2016, 2016. Miyazaki, K., Eskes, H., Sudo, K., Boersma, K. F., Bowman, K., and Kanaya, Y.: Decadal changes in global surface NO<sub>x</sub> emissions from multi-constituent satellite data assimilation, *Atmos. Chem. Phys.*, 17, 807-837, <https://doi.org/10.5194/acp-17-807-2017>, 2017. Itahashi, S., Uno, I., Yumimoto, K., Irie, H., Osada, K., Ogata, K., Fukushima, H., Wang, Z., and Ohara, T.: Interannual variation in the fine-mode MODIS aerosol optical depth and its relationship to the changes in sulfur dioxide emissions in China between 2000 and 2010, *Atmos. Chem. Phys.*, 12, 2631-2640, <https://doi.org/10.5194/acp-12-2631-2012>, 2012.

Response: Thanks for the recommendation. These studies show that satellite remote sensing provides a powerful tool to assess the spatiotemporal trends of air pollutions for both global and regional scales. The references have been added in Lines 19-21, P3.

(8) P4, L11: pollution(s)

Response: This revision has been made (Line 15, P4).

(9) P4, L26: policy, not policies

Response: This revision has been made (Line 5, P5).

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(10) P5, L21: unclear what R2 refers to

Response: It is coefficient of determination. We have added the description after R2 (L26, P5).

(11) P5, L25: "Validation results indicated. . ." be more specific. Validation done where, when?

Response: Two ways were used to validate the accuracy of historical estimates. First, we compared the historical estimates monitoring data from Hong Kong and Taiwan before 2013. Second, we estimated PM<sub>2.5</sub> concentrations in the first half of 2014 using the 2013 model and compared them with the ground measurements to evaluate the accuracy of PM<sub>2.5</sub> estimates beyond the model year, which can represent the accuracy of historical estimates. This description has been added (Lines 4-8, P6).

(12) P6, L12: suggest to remove referring to Ma et al. [2016]. This paper should describe the method briefly itself.

Response: We added details about the equations of the two-stage model, please see P8-P9.

(13) P6, L20-21: please discuss the representativeness of the PM<sub>2.5</sub> stations for the size of a MODIS pixel, or vice versa?

Response: We pointed out the uneven spatial distribution of ground PM<sub>2.5</sub> monitors. Please see Lines 6-12, P11.

(14) P9, L25: grammar

Response: This sentence has been deleted in our revision process.

(15) P10, L9-10: "strengthened the ECER measures" -> explain how

Response: Major air pollution control measures and corresponding achievements were added. See Lines 4-13, P13.

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(16) P10, L11-12: explain qualitatively how this would have worked

Response: The main reasons were added. See Lines 22-27, P13.

(17) P10, L18-19: explain why further reduction emissions had no beneficial effect anymore

Response: The main reasons were added. See Lines 13-21, P14.

(18) P10, L21-22: rephrase... I don't think bottleneck is the term you should use.

Response: We have rephrase "bottleneck" to "limitation". See Line 13, P14.

(19) P10, L25: "After that" -> be more specific what the policy consisted of then

Response: Major measures included were added in Line 23, P14 Line 12, P15.

(20) P11, section 4.4: it would be useful to include here already how the findings relate to Chinese and WHO air quality standards.

Response: We thought that adding a comparison in Section 4.5 would be better. We added a sentence in Lines 10-12, P17. We want to show that although China has achieved great success in PM2.5 pollution control, PM2.5 levels are still much higher than Chinese and WHO air quality standards.

(21) P11, L6-7: how? we remain in the dark what was actually done and how that helped

Response: Major air pollution control measures and corresponding achievements were added. See Line 23, P15 Line 23, P16.

(22) P11, L11: what explains the regional differences?

Response: We discussed the regional differences in Lines 22-26, P19.

(23) P11, L21: close(d)

Response: This revision has been made. See Line 10, P17.

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(24) P11, L25: what are the "official results"?

Response: They are the "official results" of "APPC-AP performance assessment (Table 4)". This revision has been made accordingly. See Line 13, P17,

(25) P12, L20: "the overall decrease" -> be quantitative

Response: Done. See Lines 23-24, P18; Lines 2, 7-8, P19.

(26) P13, L2: "All these policies" -> it should be made clear what was the essence of this

Response: Since we have added the essence in the main text, we did not add it here again. We refer it to Sections 4.4 and 4.5. See Lines 6-7, P19.

(27) P13, L4: MEE -> ?

Response: the Ministry of Ecology and Environment (MEE), see Line 3, P17.

(28) P13, L9: "air pollution control in China has achieved a periodic victory"-> this is awkward, do the authors mean that the measures taken so far have resulted in a temporary solution, or, more precisely, have succeeded to mitigate the worst aspects of PM2.5 pollution?

Response: What we want to say here is that air pollution control in China has achieved great success in PM2.5 pollution reduction. Sorry for the awkward phrase. We have revised this sentence to "Currently, China has achieved great success in PM2.5 pollution control." See Line 1, P20.

(29) Figure 2: unclear what difference is between upper and lower rows.

Response: They are model fitting (upper row) and cross validation (CV, lower row) results. We have revised the caption accordingly. We have revised the caption of Figure 2. And we added a brief description of CV in Line 25, P8 L3, P9.

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Please also note the supplement to this comment:  
<https://www.atmos-chem-phys-discuss.net/acp-2018-1191/acp-2018-1191-AC1-supplement.zip>

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Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2018-1191>, 2018.