

Interactive comment on “Effects of air pollution control policies on PM_{2.5} pollution improvement in China from 2005 to 2017: a satellite based perspective” by Zongwei Ma et al.

Anonymous Referee #1

Received and published: 11 December 2018

This is an interesting paper putting forward a historical perspective on PM_{2.5} surface concentrations in China. The authors propose a statistical method that relates satellite-observed aerosol optical depth (AOD) over China to measurements of PM_{2.5} at the surface. The authors use the years 2013 and later when 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_{2.5} for the entire 2005-2017 period from MODIS AOD. This allows the authors to evaluate the effectiveness of the various Chi-

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nese air pollution control policies that have been applied in this period. Based on the satellite-estimated PM_{2.5} 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.

Specific comments

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_{2.5} changes over time between 2005 and 2017, and why. The results summarized should be presented in quantitative fashion.

Methods

The method to infer PM_{2.5} from MODIS AOD is explained only very briefly with 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 PM_{2.5} and AOD. Which parameters explain most of the variance and why.

Specifically:

1. Provide the equation establishing the relationship between AOD and surface PM_{2.5}
2. Explain how the fit parameters have been derived, and discuss the orthogonality of

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the various explanatory terms (humidity, boundary layer height, T, . . .)

3. Discuss the temporal resolution of the relationship (“each year’s model”)
4. Discuss the differences and agreement with the model-scaling approach

Since the method relies on the quality of the MODIS and surface PM_{2.5} data, these aspects should be discussed as well.

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.

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 PM_{2.5} estimates, along with the satellite data, and ground-based observations for one or a few particular locations.

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 PM_{2.5} levels. Such a figure could then later be confronted with the observed PM_{2.5} evolution, and tell the story whether measures have been effective.

Minor comments

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P2, L3: polices → policies

P3, L13-14: the citations are quite China-centric. Consider citing studies on SO₂ and NO₂ trends over China from non-Chinese groups, e.g. Itahashi et al. [2012], Krotkov et al. [2016], Miyazaki et al. [2017].

P4, L11: pollution(s)

P4, L26: policy, not policies

P5, L21: unclear what R^2 refers to.

P5, L25: “Validation results indicated...” be more specific. Validation done where, when?

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

P6, L20-21: please discuss the representativeness of the PM_{2.5} stations for the size of a MODIS pixel, or vice versa?

P9, L25: grammar

P10, L9-10: “strengthened the ECER measures” → explain how

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

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

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

P10, L25: “After that” ... → be more specific what the policy consisted of then

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

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

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P11, L11: what explains the regional differences?

P11, L21: close(d)

P11, L25: what are the “official results”?

P12, L20: “the overall decrease” → be quantitative

P13, L2: “All these policies” → it should be made clear what was the essence of this

P13, L4: MEE → ?

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?

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

Suggested refereces

Krotkov, N. A., McLinden, C. A., Li, C., Lamsal, L. N., Celarier, E. A., Marchenko, S. V., Swartz, W. H., Bucsela, 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 SO₂ and NO₂ 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_x 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

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and 2010, Atmos. Chem. Phys., 12, 2631-2640, <https://doi.org/10.5194/acp-12-2631-2012>, 2012.

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

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