

To reviewer 2

Thanks so much for your valuable comments, which helped us so much revise the manuscript. We have fully revised it according to all your general and detailed comments. We are more than willing to conduct further revisions if additional requirements are given. The responses to the questions raised in your comment are as follows.

1. The novel insights and research contribution of this study should be better articulated. From the results and discussion section, I found several statements “Page 7, lines 202-203: the variation trend of HCHO agreed well with previous studies (Jin and Holloway, 2015; Shen et al., 2019b)”, “Page 7, lines 207-208: which was consistent with previous studies (Jin and Holloway, 2015; Li et al., 2019a)”, “Page 10, line 291: our findings were generally consistent with previous studies”. It would be better to highlight the position of this study and further justify the research advance, e.g., regarding the methods and datasets, or a more comprehensive picture of ozone formation regimes in China.

Response: Thanks so much for these valuable comments. The comparison of previous studies and this research mainly aimed to add reliability of estimated transitional regime range. But your comment is quite right and we should highlight the unique contribution and findings of this research. In the revised manuscript, the advances of this research was added at lines 324-329 of the revised version.

“In addition to the generally consistent outputs, some advances of this research are listed as follows. First, only a few parameters are required for polynomial model and CCM, which effectively reduced the uncertainties of model setting. Second, considering the differences between model and satellite retrieved datasets (Jin et al., 2020), only observation data were employed in this research, which reduced potential data inconsistencies and uncertainties. Most importantly, given the lack of actual reference data, this research employed two different models to examine ozone formation regimes and the close outputs further proved the reliability of this research.”

Thanks again for your valuable comments, which improved the manuscript significantly.

2. The authors set the implementation of Clean Air Action in 2013 as the breakpoint, which is appropriate for the comparison between these two baselines. However, the contribution from this

policy to driving the decreased NO₂ should be acknowledged in a more systematic and quantitative way, by adjusting a number of confounding factors.

Response: Thanks so much for pointing this out. Yes, the implementation of clean air action has exerted a strong influence on the reduction of NO_x across China, and the effects of clean air action have been massively studied in recent studies. Since the aim of this research was to estimate the transitional range of ozone formation regime and the impacts of Clean Air Action was widely discussed by previous studies, we added relevant explanations at lines 357-362 of the revised manuscript s:

“The influence of Clean Air Action on the reduction of PM_{2.5} concentrations and NO_x has been investigated by previous studies. Zheng et al. (2018) employed index decomposition analysis to quantify the contribution of the Clean Air Action, and suggested that the decreasing rate of NO_x significantly accelerated since 2013. Moreover, Zhang et al. (2020) employed random forest algorithm to remove the effects of meteorological conditions, and evaluated the impacts of Clean Air Action. The results demonstrated that the deweathered NO₂ concentrations in winter 2007 and 2017 were 70.3 μg/m³ and 59.1 μg/m³, with a decreasing rate of 16%.”

Thanks again for this valuable comment.

3. The scaling biases between the station-based observations (i.e., point) and remote sensing based measurement (i.e., 0.25-degree footprint) should be discussed, especially for heterogeneous land cover/land uses.

Response: This is a very good point. According to your comment, we discussed the potential scaling biases at lines 330-333 at the revised version.

“First, the accuracy of the estimated range of transitional regime might be influenced by the scaling biases between station-based observations of surface ozone and space-based HCHO and NO₂. Since ozone monitoring stations are mainly distributed in urban areas, and a 0.25 ° × 0.25 ° grid might cover both the urban and rural areas, the surface ozone concentrations of a grid may be overestimated.”

Thanks again for this valuable comment.