

Atmos. Chem. Phys. Discuss., referee comment RC1  
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## Comment on acp-2021-330

Anonymous Referee #1

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Referee comment on "Nonlinear responses of particulate nitrate to NO<sub>x</sub> emission controls in the megalopolises of China" by Mengmeng Li et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2021-330-RC1>, 2021

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### General comments:

Li *et al.* present a model analysis of secondary inorganic aerosol (SIA) in major cities of Eastern China. They show data of PM<sub>2.5</sub> from a network across China from 2012 to 2016 and speciated PM<sub>2.5</sub> from major supersites in Eastern China. They analyze the response of sulfate and nitrate to the reductions in NO<sub>x</sub> that have occurred in the 4 year period, and to a theoretical series of reductions spanning 10 to 80%. Due to the response of oxidants to NO<sub>x</sub>, the changes in SIA are approximately linear in summer but highly non-linear in winter, indicating NO<sub>x</sub> saturation of oxidant production in winter.

The results are relevant to the understanding of air pollution and its recent trends in China and is of interest to the readership of ACP. Publication is recommended following attention to the specific comments below.

### Specific comments:

Line 71: Neither of the stated assumptions, stagnant atmosphere or a fixed NO<sub>2</sub>/NO<sub>y</sub> ratio, is a condition for a box model. Suggest eliminating this statement.

Line 77-78: Is the NO<sub>x</sub> sensitivity of Chinese ozone a well accepted result? Increasing O<sub>3</sub> trends have been attributed, for example, to effects associated with decreasing PM<sub>2.5</sub>, e.g.,

Li, K., D.J. Jacob, H. Liao, L. Shen, Q. Zhang, and K.H. Bates, *Anthropogenic drivers of 2013–2017 trends in summer surface ozone in China*. Proceedings of the National Academy of Sciences, 2019. **116**(2): p. 422.

Line 248: It appears that the oxidation ratio of sulfur decreases, but nitrogen increases, between the warm and cold seasons. Is that correct? In this case, there is more going on than just enhanced secondary oxidant formation. There is also a change in chemical mechanism between S and N.

Line 249: Here and above, specify more clearly what is meant by the two pathways – presumably this is the difference between OH + NO<sub>2</sub> (gas phase) and N<sub>2</sub>O<sub>5</sub> hydrolysis (heterogeneous), but it is not clear from this text or figure 6 what this specifically means.

Line 258-259: Two comments. First, the global burden of nitrate production pathways may, or may not, be relevant at a regional level. Second, there are more updated papers indicating that the two pathways considered here are more equal at the global scale.

Alexander, B., T. Sherwen, C.D. Holmes, J.A. Fisher, Q. Chen, M.J. Evans, and P. Kasibhatla, *Global inorganic nitrate production mechanisms: comparison of a global model with nitrate isotope observations*. *Atmos. Chem. Phys.*, 2020. **20**(6): p. 3859-3877.

Line 275: The responses do not appear "bell shaped". Is there a better descriptor?

Line 301-319: The changes in sulfate in Figures 8 and 9 is quite difficult to discern, so it is not easy to match this text to the changes in the figure. Can sulfate be plotted on its own scale to illustrate these changes? Also, the small dependence of sulfate on  $\text{NO}_x$  indicates that OH is also not a strong function of  $\text{NO}_x$  in this model, but later the OH changes are quoted as being large compared to the changes in sulfate. If OH is the most important factor for sulfate changes, why are the modeled sulfate changes so much smaller than the changes in OH?

Line 329-330: Same comment for sulfate. Changes are very difficult to see.