

General Comments

This paper contains very lengthy descriptions of model responses to changes in emissions of NO_x, VOCs, and both in China. The goal of the paper is to help policy-makers mitigate ozone increases in urban areas. However this message gets lost in the lengthy descriptions throughout the paper. The authors should consider significantly shortening their descriptions with a focus on how their findings can clarify the impact of policy measures that either reduce NO_x, VOCs, or both. Do we need a description of changes in pRO_x, pO_x, OH reactivity, NO_x reactivity, and AVOC? Could a subset of the plots provided with a focus on high ozone and PM_{2.5} areas suffice? What is the message the authors want to give regarding PM_{2.5}?

The authors should also better explain their finding that the combined reduction of NO_x and AVOC emissions has a larger effect on both ozone and PM_{2.5} than the sum of the reductions of NO_x and AVOC separately. Currently, it reads for example that the best 'value' to be gained in reducing P(O_x) in summer is reducing NO_x. This is also said on line 390. Overall, the authors have too much detail on specific changes in their model, and insufficient description of the broader new understanding gained or policy-insights developed.

The authors have a modeling setup that could provide insight into the benefits of different types of emissions reductions and help us gain insight into the impacts of the reductions in aerosol on ozone concentrations. The aerosol impacts on ozone could be the most interesting part of this paper but the manuscript as written is far too lengthy and lacks clear and concise messaging. The authors describe many model metrics (pRO_x, pO_x, AVOC) but it is not clear what different insights are gained from each one, or if a singular metric would suffice to describe the relevant model impacts. If the authors are able to revise the paper to increase the value of their scientific analysis and refine their messaging, then it would be appropriate for publication.

Specific Comments

Intro: I would expect that in VOC-limited areas, decreasing NO_x would result in higher OH from reduced loss of OH to OH + NO₂ → HNO₃. Thus, HO₂ would be higher from increased VOC oxidation. Is the aerosol uptake effect on NO₂ from reduced nitrate aerosol really larger?

Line 110 – “nitration” should be “titration”. Also, please clarify the meaning of this statement “and the competition between NO₂ and VOC for OH radicals”.

Line 214 – “Validated” implies the model was correct in the companion study while there were a variety of model shortcomings described such as overestimated summertime NO₂ and PM_{2.5}. It would be better to describe how any model biases impact the conclusions rather than call the model “validated”.

Line 237 – Has anyone done a weekend/weekday analysis of ozone to see whether ozone goes up or down when NO_x is reduced on the weekends, assuming that is the case in China? A quick search found studies like this: <https://www.sciencedirect.com/science/article/pii/S1474706518302110>, or <https://www.nature.com/articles/s41598-020-64111-3>. If so, please cite those studies here as support for your spatial distribution of regimes.

Line 312 – Can you explain why that is?

Section 3.2: This goes into great detail on how the budgets of radicals change, and I find it difficult to see what the overall conclusion is that is either policy-relevant or novel. Instead, it just reads like a helpful description of the model behavior which may be useful for other modelers but is not necessary in the main text. In that case, the paper could be shortened by a quick summary of the major effects (less NO_x = less loss to HNO₃, less VOCs = less pRO_x from OVOCs, less CO = more pRO_x due to higher OH etc) and moving the majority of the discussion to the supplement. If not, the authors need to better state the importance of their description.

Line 390 – Can the authors be more specific about the meaning of “further enhanced”? Why should policy makers bother if most of the impact is from NO_x?

Line 392 – The authors state that reductions in ‘specific AVOCs’ are needed but so far they have only discussed AVOCs as a whole.

Line 424 – The increase in OH in Hong Kong appears very strong as well.

Line 473 – Does the model really have $\text{NH}_3 + \text{OH}$ as a significant sink of OH? If the authors are referring to its impact on SIA and thus HO_2 uptake, this is not clear.

Paragraph starting on line 567 – This discussion is again very lengthy. The figure appears to show that the most important message is that NO_x reductions in July alone result in ozone decreases in several major cities (Beijing, Shanghai, Hong Kong?) while adding in AVOC reductions causes the cities to also see an ozone decrease.

Line 599 – How does reduced AVOCs impact nitrate, sulfate, and ammonium? It is not clear from this sentence.

Line 628 – How much does photolysis increase in your model with reduced aerosol?

Figure 13 – What is in ‘Other’ that is impacted in your ‘TOTAL’ case? This category is a surprisingly large fraction of model $\text{PM}_{2.5}$ and thus deserves more discussion. Overall, Fig. 13 contains a lot of information but is barely discussed.

Line 645 – Why have a schematic for reduction in NO_x emissions, but not AVOC emissions, and the combination of the two?

Section 3.4.1 – Again, I am not sure what the main message is from this lengthy section.

Line 755 – What is the result of the increased ozonolysis? Do we get more OVOCs that impact daytime air quality? This is said later but is not clear here.

Line 771 – Are these primary OVOCs like methanol or ethanol? Or secondary species like HCHO and acetaldehyde? If secondary, then what are their main precursors? Which ‘unsaturated OVOCs’ should be targeted?

Summary – Again, a greater focus on policy-relevant insights would be helpful as there are opposite effects on average compared to in the major cities.

Line 795 – What about reduced loss of OH to $\text{OH} + \text{NO}_2$ which increases the ability to oxidize VOCs?

Line 830 – The reason for the greater joint impact needs to be explained.

Line 853 – Refrain from discussing ‘slight’ changes to focus on the major findings.

Line 869 – Here the authors state that their goal is to help develop a strategy for metropolitan areas. If this is the goal of the paper, the authors should consider a greater focus on the impacts on cities (bar chart figures such as Fig. 4).

Line 869 – The authors already specifically call out categories of VOCs (alkenes, aromatics etc). Could the authors better describe what they mean by ‘more detailed investigations’ here?

Code and data availability: This does not include the modifications made to WRF-Chem described in Dai et al., 2023 and used here.