

Response to short comments on “Response of winter fine particulate matter concentrations to emission and meteorology changes in North China”

We thank Dr. Frank Dentener for valuable comments. This document is organized as follows: the comments are in black and our responses are in [blue](#).

To Short Comments

The authors perform emission and meteorology perturbation studies over East Asia (1960-2010), and I would like to point to the similarity of this paper to another paper in this special issue (Kasoar et al.).

I suggest that the magnitude of the emission perturbations Kasoar study (zero-out of anthropogenic) emissions, versus this study (1960-2010 emission changes) is sufficiently similar to warrant some comparison- e.g. of column amounts or AOD. Could something be said about ‘local’ temperature responses as calculated by the model? One other analysis aspect that I find somewhat missing is not only the role of meteorological boundary conditions, but also the chemical ones. With some extra simulations (combinations of BC and emission perturbations) these aspects could also be evaluated, making the publication even more valuable for the HTAP special issue.

[Responses: Thanks for mentioning this great work \(Kasoar et al.\) published in the same issue. In Kasoar’s study, the results are presented due to removal of SO₂ emissions, but our results are due to all anthropogenic emission changes from 1960-2010. We plot column AOD and temperature responses and compared with the results in Kasoar’s study. As shown below, due to reductions in 2010 from 2010 to 1960, AOD at 600nm decreases by -0.1~-0.4 in highly polluted areas, and temperature increases by about 0.1~0.55 degree. The results are closet to the results from HadGEM3-GA4 model in Kasoar study.](#)

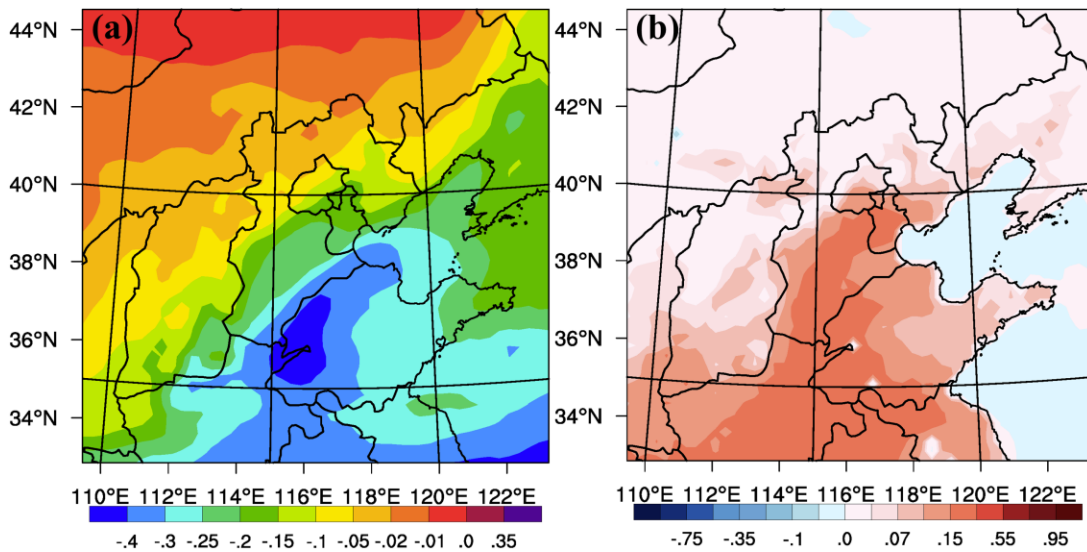


Figure Change in column AOD at 600nm (a) and near surface temperature (b)

Below I offer some comments of technical nature that would warrant additional analysis.

1) Table 1: Assuming that PM_{2.5} in this study is defined as SO₄+NO₃+NH₄+BC+OC, it seems that PM_{2.5} value in the first row (1960) is not correct.

Responses: Thanks for this question. Actually, PM_{2.5} in this study is not just SO₄+NO₃+NH₄+BC+OC. It also includes na,cl, and dust. Na and cl concentrations are very low, so we just ignore it in this study. In table 1, PM_{2.5} is larger than SO₄+NO₃+NH₄+BC+OC because it includes dust. To clarify this, I added “natural dust (the difference between PM_{2.5} and the sum of sulfate, nitrate, ammonium, BC, OC)” in the revised manuscript. Hope it is not confusing now.

2) Table 2: (and text). I find the difference between the single perturbation studies and the combined one surprisingly high. I am wondering if in the combined Sox-NOx-NH₃ perturbation study also VOC and CO were perturbed- which perhaps could explain the large difference?

Anyway the authors should comment on this, because of cause the response of photochemistry to NO_x perturbations can be quite different depending on VOC emissions. One diagnostic analysis is budget analysis: emissions, budget, transport (in-out), lifetime would be very valuable to show.

Responses: Thanks for this great question and suggestion. Yes, you are right, in the combined Sox-NO_x-NH₃ perturbation study, VOC and CO were also perturbed, which might be the cause of the large difference. We have added this comment on the cause of this difference in the revised manuscript. We also added the plots of VOCs: NO_x ratio in 1960 and 2010, which will help readers to understand the photochemical background in 1960 and 2010.

3) page 6, clarify whether the Mozart simulations also used 1960 (2010) emissions. The use of January 2010 warrants some discussion on how representative or typical this month was for a longer climatic period. While even for aerosol with lifetimes of a few days a spin-up of 5 days is rather short, it is certainly not capturing the lifetime of ozone and other components that feedback through oxidants on chemistry. As the authors seem to find large non-linear effects, I think they should consider trying to do longer simulations, if possible.

Responses: Thanks for pointing out this issue. The Mozart simulations used 2010 emissions but we only used boundary and initial conditions for the outer domain, which used emissions for 1960. We downloaded MOZART simulations from NCAR website, which does not cover early years (1960). The results presented used the innermost domain, which takes boundary conditions from outer domain. We have clarified this point in the revised manuscript. This study focuses on a month based simulation, but we are working on running long simulations from a climate change perspective, which will be another interesting story.

4) a table with domain emissions in experiments would be useful. It is not clear how much SO₂ was changing (p.9). Clarify what was done with VOC, CO. I assume that the BC/IC were not changing along with the sensitivity studies, but it should be clarified.

Responses: Thanks for this great question. We have added the changing factor for SO₂ (increased by +220%) in the revised manuscript. Changes in other species were also added and modified based on column values. CO and VOCs were also projected from 1960 to 2010. We have added this clarification. Yes, IC/BCs were not changing. We have also added this point in the revised manuscript.

5) section 3.4 (and 3.3) I am a bit wondering about the consistency of changing RH and T separately- while obvious the parameters are closely connected. I think this warrants more discussion. Would changing absolute humidity make more sense? Would dynamics change when changing RH?

Responses: Thanks for this great question. Yes, T and RH are closely connected. The increases in T will lead to changes in RH, and the changed RH will affect some chemical reactions. But these changes are also due to changing T. Under global warming background, chemical reactions might change via changed RH. Some previous studies perturb absolute humidity using offline models. But it is not easy to implement it in the fully online coupled model. We found that perturbing initial and boundary conditions is one of the solutions, but in WRF, RH is provided in boundary and initial conditions, not absolute humidity. Changing absolute humidity might be more interesting and deserve future investigation. When changing RH, wind fields slightly changed, not significant.