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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Response of winter fine particulate matter concentrations to emission and meteorology changes in North China" *by* M. Gao et al.

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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 meteoro-

logical 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.

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

1) Table 1: Assuming that PM2.5 in this study is defined as SO4+NO3+NH4+BC+OC, it seems that PM2.5 value in the first row (1960) is not correct.

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 NOx 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.

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.

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.

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 war-

rants more discussion. Would changing absolute humidity make more sense? Would dynamics change when changing RH?

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