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# *Interactive comment on* "Sensitivity Analysis of the Surface Ozone and Fine Particulate Matter to Meteorological Parameters in China" *by* Zhihao Shi et al.

**Anonymous Referee #1**

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The paper discusses the sensitivity of surface ozone and PM2.5 in China to meteorological parameters. The information presented in the paper is useful to understand the interaction between pollution and meteorology, and regional difference in the sensitivity of emission control measures. I'd recommend the publication of the paper if the following comments are addressed:

(1) The method description is very brief, and the details in implementation may affect the interpretation of the results. In particular, I see one difficulty in this type of sensitivity simulation that a simple perturbation of individual parameters may lead to unphysical meteorological fields. For example, increasing/decreasing T by 1 K under some con-



ditions may turn saturated/unsaturated air into unsaturated/saturated, but since only T is perturbed, no cloud is dissipated/formed in response to changing T. Another example, a simple perturbation of wind speed may generate a wind field that violates the physics, and is inconsistent with the pressure field that feeds into the air quality simulation, which may lead to spurious sensitivities in the result. Even more difficult is to perturb wind direction, though I notice the authors did not assess the wind direction sensitivity. In general, I'd like to see if and how this type of issues is handled by the authors. The current method description is too brief to tell the exact implementation. Other useful details to include are if the perturbations are done for the entire atmosphere or only in the boundary layer, if they are done for the whole day uniformly or only in the daytime.

(2) The responses of emissions to meteorological parameters are not included in the assessment. The responses of emissions to meteorology is a significant contributor to the overall meteorological sensitivity of ozone and PM2.5. To name a few, the effect of T on biogenic emissions, the effect of T on soil NOx emissions, the cloud cover/convection on lightning NOx emissions, the effect of T on power plant NOx emissions (high T leads to higher electricity demand in summer). Because emissions are held unchanged in the simulations, these effects are not included, which makes the analysis incomplete and less informative. This caveat needs to be discussed in the paper.

(3) Evaluation against observations. The O3-T slope from model simulations is often found to be much lower than that derived from observations, suggesting that model tends to underestimate the sensitivity of O3 to meteorology. The current paper provides no evaluations of how good the model in use could reproduce the observed chemical-met relationship. Note this evaluation is different from evaluation of chemical concentrations, and is perhaps more relevant for the current work.

(4) In abstract and elsewhere (such as Line 282), the authors compare the different sensitivities. For instance, the paper says in Line 282 that "the sensitivity of O3 to

T is obviously higher than that of WS, AH, and PBLH”. This is to compare apples to oranges, because these sensitivities are in different units!

The delta concentrations of $O_3$ or $PM_{2.5}$ from two simulations apparently depend on how much you perturb, and it is meaningless to compare which one is bigger unless the perturbations are carefully defined to relate to the variations of individual parameters.

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