

## ***Interactive comment on “Observations and modeling of air quality trends over 1990–2010 across the Northern Hemisphere: China, the United States and Europe” by J. Xing et al.***

**J. Xing et al.**

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Received and published: 14 January 2015

We would like to thank the referee for a very thoughtful and detailed review of our manuscript. Incorporation of the reviewer's suggestions has led to a much improved manuscript. Below we provide a point-by-point response to the reviewer's comments and how we have addressed them in the revised manuscript.

General

[Comment]: The authors repeatedly invoke “coarse spatial resolution” as a reason for many of the model's shortcomings in comparison with observations. This well may

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be the case, however, the authors should give some thought (and some discussion in the Conclusions) about exploring this limitation in future work, possibly via finer-scale simulations nested over one or more of the focus domains.

[Response]: we agree with the reviewer and have provided additional description about limitation of coarse spatial resolution in the revised manuscript (P26 L4-7), as below:

“To future explore the limitation of coarse spatial resolution, we are currently conducting a study with a finer-scale simulation over the CONUS domain for the same simulated period as from 1990 to 2010. A detailed description and comparison will be provided in a separate paper (Gan et al., in preparation)

Gan, C.-M., Pleim, J., Mathur, R., Hogrefe, C., Long, C.N., Xing, J., Wong, D., Gilliam, R., Roselle, S.J. and Wei, C.: Assessment of long-term simulations with various observations for better understanding of aerosol effects on radiation “brightening” in the United States, in preparation”

[Comment]: Figures 3-9 need more detailed captions to explain the identities of each Figure component. These graphics are dense with information, but it is not immediately obvious to the reader exactly what is being presented. It is possible to infer from the text what each component of the Figure represents, but a more informative caption would make for a better presentation for the average reader.

[Response]: we thank the reviewer for pointing this out; more detailed captions for each graphics in Figures 3-9 are provided in the revised manuscript, as below: Figure 3 captions:

(a) Simulated SO<sub>2</sub> trend from WRF-CMAQ (unit:  $\mu\text{gm}-3 \text{ yr}-1$ )

(b) Upper-Color map: simulated SO<sub>2</sub> trend in East China overlaid with observed SO<sub>2</sub> trend from China-API, dot represents each observation site, computed on the basis of annual means over the 2005–2010 period with a linear least square fit method, dot size is determined by the significance of trend, i.e., larger symbols denote more significant

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trends at 0.05 level (unit:  $\mu\text{g m}^{-3} \text{ yr}^{-1}$ ) Lower-Scatter plot: observed and simulated SO<sub>2</sub> concentration, network-mean for each year corresponding grid cells from model simulation are selected for comparison (unit:  $\mu\text{g m}^{-3}$ )

(c) same as (b) for Europe - AIRBASE

(d) same as (b) for Europe - EMEP

(e) same as (b) for the U.S. - AQS

(f) same as (b) for the U.S. – CASTNET

[Comment]: A final general suggestion is that the text be further proofread for acceptable English grammar and usage. Some edits are noted in the specific comments below, but further changes may be needed.

[Response]: We have reworked all the sections of the manuscript to improve the written English and editorial quality.

[Comment]: p. 3, lines 15-16: It's debatable that this is the "ultimate" goal of any country. Possibly, the authors meant something like "an important goal for any country."

[Response]: As the reviewer suggested, we changed "ultimate" to "important" in the revised manuscript (P3 L15).

[Comment]: p. 6, lines 18-19: It would be appropriate to include a brief summary of results from the WRF performance evaluation here, in particular noting any biases that may have an impact on the results presented in this manuscript (e.g., temperature, precipitation, etc.).

[Response]: We have included a brief summary of WRF evaluation in the revised manuscript (P6 L17-P7 L1), as below: "WRF performance for the simulation of hourly surface temperature (T), relative humidity, wind speed and direction was evaluated through comparison with observations from NOAA's National Climatic Data Center (NCDC) Integrated Surface Data (ISD with lite-format) which provides hourly (or with

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3-hour interval) meteorological observations over a long historical period across the globe. The mean bias of T, wind-speed and direction over the simulation domain is -0.4 K, 0.4 m s<sup>-1</sup> and -3 degree respectively, within the benchmark range suggested by Emery et al. (2001) for retrospective regional-scale model applications which is  $\leq \pm 0.5 \text{ K}$ ,  $\leq \pm 0.5 \text{ m s}^{-1}$  and  $\leq \pm 10 \text{ degree}$  respectively"

[Comment]: p. 7, lines 13-15: It is puzzling to the reviewer why BVOC emissions were kept constant over all simulated years, although it likely does not significantly impact the results obtained. A rationale for choosing constant BVOC emissions should be provided.

[Response]: We agree with the reviewer that detailed BVOC emissions with high temporal resolution will definitely improve the accuracy of the results. Unfortunately, at the time this study began, there were no available BVOC emissions covering such spatial and temporal scale as simulated here. We have clarified such limitation in the revised manuscript (P26 L1-2), as below: "The trend of biogenic emissions, which hasn't been considered in this study, might also impact the analysis."

[Comment]: p. 11, lines 12-16: A more detailed explanation should be offered for the difference in sulfate bias between the U. S. networks and the European network, which is an interesting result. Does the reference to "uncertainty in precipitation" refer to something found in the WRF evaluation of this time period? Are there differences in precipitation biases between the U. S. and Europe? If so, they should receive more discussion here.

[Response]: We appreciate the suggestion from the review, and we further investigated the WRF performance of the precipitation. However, there are no significant differences in the biases between the U.S. and Europe which are -0.14mm and -0.10mm for 6h-duration precipitation respectively. The difference in sulfate bias between the U.S. networks and the European network might be associated with different SO<sub>2</sub> biases in these two regions, i.e., a moderate bias (NMB=-9.4%) in US-CASTNET but a

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relatively larger bias (NMB=+67%) in EU-EMEP. The transition rate from SO<sub>2</sub> to SO<sub>4</sub><sup>2-</sup> is likely underestimated in both regions, leading to the underestimation of SO<sub>4</sub><sup>2-</sup> in the U.S. and the better estimates of SO<sub>4</sub><sup>2-</sup> in Europe. We added some discussion about this in the revised manuscript (P12 L5-10), as below: “Better performance is shown at EU-EMEP, with NMB within  $\pm 10\%$ . The difference in sulfate biases between the U.S. networks and the European network might be associated with the different SO<sub>2</sub> biases, i.e., a moderate bias (NMB=-9.4%) in US-CASTNET but a relatively larger bias (NMB=+67%) in EU-EMEP. The transition rate from SO<sub>2</sub> to SO<sub>4</sub><sup>2-</sup> is likely underestimated in both regions, leading to the underestimation of SO<sub>4</sub><sup>2-</sup> in the U.S. and the better estimates of SO<sub>4</sub><sup>2-</sup> in Europe.”

[Comment]: p. 9, line 2: Should be: “... considered during periods of missing ...”; p. 10, line 21: Should be: “... worst ...” not “worse””; p. 16, line 11: Should be: “... trends in observations in the urban network ...”. p. 16, line 12: Should be: “... that causes the model to fail to represent ...”. p. 24, line 13: Should be: “... in Europe and North America has been ...”. p. 25, line 15: Should be: “... this relative ratio could potentially ...”.

[Response]: These typos have been corrected in the revised manuscript (P9 L12; P11 L7; P17 L7; P17 L8; P25 L11; P26 L21).

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C11205/2015/acpd-14-C11205-2015-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 25453, 2014.

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