

Interactive
Comment

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.

Anonymous Referee #1

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Summary

The authors present a modeling study of air quality trends in the Northern Hemisphere over a 21-year period spanning 1990–2010 using the hemispheric version of WRF-CMAQ. Comparisons are made between model results and observations for both gas phase species and PM_{2.5} chemical components and an analysis is presented showing how emission changes over the period have affected modeled trends in ozone and PM_{2.5} components. The manuscript is an important and generally well-done contribution to understanding changes in air quality in the Northern Hemisphere since 1990. Publication of the manuscript is recommended with only minor revisions as suggested

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below.

General

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

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.

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.

Specific

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.”

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.).

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.

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p. 9, line 2: Should be: "... considered during periods of missing ...".

p. 10, line 21: Should be: "... worst ..." not "worse".

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.

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 ...".

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

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