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Atmospheric Chemistry and Physics Discussions

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 #3

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This manuscript attempts to analyze trends in gaseous and particulate pollutants over the United States, Europe, and China using both observations and a model simulation. Data from several surface networks are used. The simulation was performed using the hemispheric version of WRF and CMAQ. This is a very worthwhile exercise, as the model output perhaps may be more representative of the trends in regions as a whole than observations obtained from specific points. The results should be published. However, I have two major problems with the manuscript as it stands: 1) The manuscript says that an evaluation of the WRF meteorology will be the subject of a separate paper. I think that there should be at least some evaluation of WRF in the current paper. This

could be simply be some summary statistics of model performance. 2) The authors are comparing CMAQ model output (which is I think at 108 km resolution) with the AQS and EU-AIRBASE data, which are primarily from urban areas. Each of these sites are representative of much smaller regions. I do not think this comparison is appropriate. The AQS and EU-AIRBASE data should be averaged over the 108 km grid cells before comparing with the model to obtain a more valid analysis. The CN-API data are already averaged over metropolitan regions. The CASTNET, IMPROVE, and EU-EMEP data are more representative of background conditions and thus can be compared directly with the model.

After addressing these major concerns, as well as the more minor ones below, the manuscript should be published in ACP.

p. 25457, lines 17-19 and p. 25458, line 8: the earlier text mentions nested regional domains at finer resolution, and then in the later text specified the three sub-regions used in the analysis. However, no specific finer resolution is mentioned. This leaves the reader unclear as to whether the sub-regions are or are not nested. I've assumed they are not. Please clarify the text.

p. 25459, lines 16-17: lightning NOx emissions are said to be from Price et al., 1997. This paper indicates the total global emission is 12.2 Tg/yr. This amount is well above the most well-accepted values of 2 - 8 Tg/yr (Schumann and Huntrieser et al, 2007, ACP). Please provide some indication of what the impact of this likely too large emission value is on NOx, O3, and nitrate.

p. 25460, lines 5 - 8: define the acronyms

p. 25463,line 7-8: Some statistics on model precipitation vs. observed should be provided. Then, the authors could more definitively say whether precipitation bias is the reason for the underestimation.

p. 25464, lines 4-7: Were these previous modeling studies at much finer resolution? If

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so, then resolution may not be the issue.

p. 25465, line 8: maybe 'eastern AQS' instead of 'mid-east AQS'. 'Mid-east' is not a commonly used term to describe locations in the US.

p. 25466, line 13: "....capture these trends, yielding trends more similar to those of the emissions"

p. 25467, line 24: should "NOx- and VOC-limited regimes" be reversed?

p. 25472, lines 5 - 7: The authors should note that in China the rate of O3 increase was much smaller during 1995-2002, which was the period when VOC emission growth was much greater than that of NOx emissions. This result indicates greater sensitivity of ozone to NOx emissions than VOC emissions.

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