

Interactive comment on “Global O₃-CO Correlations in a Chemistry and Transport Model during July—August: Evaluation with TES Satellite Observations and Sensitivity to Input Meteorological Data and Emissions” by Hyun-Deok Choi et al.

Anonymous Referee #1

Received and published: 16 February 2017

General

This study explored the ozone-CO correlations on the global scale in boreal summer using a chemical transport model (Global Modeling Initiative (GMI)), driven by three sets of meteorological data: fvGCM with sea surface temperature for 1995, GEOS4-DAS for 2005, and MERRA for 2005. The simulations are compared with the measurements from the Tropospheric Emission Spectrometer (TES) satellite instrument so the model's capability to reproduce the TES data and sensitivity to various meteorological

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data were examined. Three radionuclide tracers were simulated as proxies for various transport-related processes to help untangle the simulated ozone-CO correlations and explain the differences. Sensitivity of ozone-CO correlations to various emissions was tested with GMI-MERRA simulations.

This study has addressed an important issue in atmospheric chemistry. The paper is well written with logic flow of text, clear description of the method and assumptions, proper and adequate literature review, and high quality of figures. This study is novel and solid. It offers new insight on global ozone-CO correlations and underlying mechanisms.

Specific

While GMI simulates tropospheric ozone reasonably well, it underestimates tropospheric CO as suggested in this and earlier studies. This underestimation may cause some biases for the ozone-CO correlations presented in this study. Please discuss.

In the model simulations, the anthropogenic emissions are kept the same (using 2005's emissions) for the simulations driven by the three sets of meteorological data. The surface biomass and biogenic emissions are all the same for the three simulations. Therefore, the differences seen in the three simulations are due to different meteorological fields that also cause the differences in lightning emissions. As understandable, the authors placed their focus on the middle troposphere when comparing GMI and TES results because TES data are least biased at these altitudes (Figures 12-15). Therefore, showing NOX emissions from lightning in the middle troposphere horizontally (like Figure 9) would help interpret Figures 12-15.

For the model validation (Figure 6), please provide the values of correlation coefficients, mean biases, and root mean square error so to help evaluate the performance of each simulation quantitatively (in the figure or in a table).

Page 2, Line 13, the authors claimed the simulated ozone-CO correlation patterns are

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consistent with those derived from TES observations, except in the tropical easterly biomass burning outflow regions. This claim is not be fully supported by Figures 13 and 14. There are large regions with negative correlations in the simulations that are not shown in the TES data. There are other discernible discrepancies between TES and GMI data that should be mentioned and discussed.

Page 21, Line 3-4: The authors stated: “Strong positive O3-CO correlations are present in all simulations at 618 hPa over Indonesia (Figure 12)”. Over the entire Indonesia? The positive correlations appear only over western Indonesia where simulations show high CO.

Remove an extra comma near the ends for Luo et al. (2007 a and b) and Mao, H., and Talbot, R. (2004) in References.

Word “Figure” may not be in bold in the final version.

[Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-1079, 2017.](#)

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