**Atmospheric
Chemistry
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Discussions**

Interactive comment on "Regional CO pollution in China simulated by the high-resolution nested-grid GEOS-Chem model" *by* D. Chen et al.

Anonymous Referee #2

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Review of "Regional CO pollution in China simulated by the high resolution nested-grid GEOS-Chem model" Chen et al., [2009]

This paper introduces an updated, high horizontal and vertical resolution, nested version of the global chemical transport model GEOS-Chem applied to China. They use this model to examine the emission locations and meteorological conditions responsible for high pollution episodes in the Beijing region. They go onto to provide new seasonal estimates/regional contributions of CO export to the Eastern Pacific.

I think this paper is very well written and provides surprising finding on the causes of high pollution episodes over Beijing (i.e., high % regional emissions) as well as a better understanding of the seasonal variations in export from Asia. I think the authors can improve the paper significantly in the following areas as described in the comments

below: 1) by doing a much better job of citing previous literature, 2) by clarifying assumptions and previous validation efforts, and 3) by strengthening some conclusions (e.g., improved representation of low-level vortex)

General Comments:

1) You mention in your introduction that there has been little study on outflow in the summer. But, there have been several studies looking at the summertime export of pollution from Asia with GEOS-Chem. In particular, a series of papers by Q. Liang. You mention one, but don't compare results at all. I suggest comparing your results more carefully to this previous work.

Liang, Q., L. Jaeglé , and J. M. Wallace, Meteorological indices for Asian outflow and transpacific transport on daily to interannual timescales, J.Geophys. Res., 110, D18308, doi:10.1029/2005JD005788, 2005.

Liang, Q., L. Jaeglé, et al., Summertime influence of Asian pollution in the free troposphere over North America, J. Geophys. Res., 112, D12S11, doi:10.1029/2006JD007919, 2007.

Liang, Q., L. Jaeglé, D.A. Jaffe, P. Weiss-Penzias, A. Heckman, J.A. Snow, Long-range transport of Asian pollution to the Northeast Pacific: Seasonal variations and transport pathways of carbon monoxide, J. Geophys. Res., 109(D23), D23S07, doi:10.1029/2003JD004402, 2004.

Holzer, M., T. M. Hall, and R. B. Stull (2005), Seasonality and weather-driven variability of transpacific transport, J. Geophys. Res., 110, D23103, doi:10.1029/2005JD006261.

2) P5860, CO Emissions: I suggest a table with CO emissions similar to Wang et al., 2004b and some discussion of how these have changed and been validated (including biomass burning since it is important for your export work). Can you breifly describe how these Zhang et al., 2009 emissions differ from previous estimates? Seasonality imposed? Are their previous validation studies (i.e., how good are your emissions)?

3) P5862: I don't see this Sichuan Basin low level vorticies argument at all from Figure 2. To do this properly I think you need a new figure to zoom in and show the topography and wind barbs similar to what Wang et al. 2004a did in Figure 2 & 7. To me it looks like you are just resolving the emissions in that region from Figure 1. It would be really interesting to prove though that the higher resolution captures some important meteorological feature, not standard in global model resolutions, so I hope you pursue showing this better.

4) Can you offer any indication of the improvement in the model due to increased vertical levels?

5) The model is never going to capture those huge peaks (I think due to errors in winds) and that 1300 ppbv cut off seems arbitrary. It also makes the 7/19 peak look more important than it is. Maybe a better comparison would be to compare the % anomalies from the mean CO for obs and model, this would show that the model captures the variability in ventilation, which is what you are trying to show. It would also be good to report the $r2$ of your model/obs comparison.

6) If you have it, and the DL site is not so influenced by local met a wind direction comparison may be useful (if it is v. locally influenced you shouldn't be showing wind speed).I think the wind direction would really strengthen your processes governing ventilation argument.

7) P5866 L15, " …BJ tracer (representing local emissions) and the TH tracer is 0.52" Does this suggest transport from these regions is favorable on high CO days?

8) P5867: I think your description of the meteorology doesn't match the figure and recommend the following changes. On Jul 3, the winds seems much stronger than 1 m/s that you say they are, that seems consistent with Jul 2. So, July 2 weak winds leads to regional scale buildup. Then on Jul 3 the flow switched to southwesterly flow in advance of the front bringing in regional pollution. Then was cleaned out on the back edge of the system (northwesterly) bringing clean air from the North. This is consistent

with the correlation btwn TF & BJ.

9) Section 5: I would suggest comparing your export calculations to previous work. There is extensive Trace-P work on lofting (i.e., Miyazaki, Koike, I think also something out of Henry Fuelbergs group – a multi-model CO comparison). Also, it would be useful to compare the Liang et al, seasonality work in this section.

10) How does your export calculation vary with the nested vs. standard resolution? How dies the improved vertical resolution impact our understanding of Asian export?

11) There was work during TRACE-P (Heald et al) suggestion that the biomass emissions are overestimated in GEOS-Chem, so would your export be an upper limit?

Specific/Minor Comments:

12) I would suggest including "export" somewhere in your title since you offer the literature new numbers for export

13) P5854 L27: typo: influences "of" physical

14) P5857, L5-7 Introduction: I would suggest adding to the end of sentence : The high resolution nested grid . . ..as the GEOS-Chem model "allows for consistent propagation of features into the domain".

15) P5858, L1-3 Introduction: I suggest referring to Figure 1 here.

16) P5858, L23: Suggest renaming "Modeling Approach" or "Nested Modeling Approach"

17) P5858, L15: Suggest changing "retains a generic high resolution" to " retains the native high resolution"

18) P5859, L12: should be one year, not one month right?

19) P5860, Section 3: I think section three is confusing. I would suggest moving the information below 3 and 3.1 to sub sections of section 2 (e.g., 2. Modeling Approach,

2.1 Nested Grid Formulation, 2.2 Tagged-CO simulation, 2.3 CO Emissions over Asia), these are model descriptions, not science.

20) Then, you can compare the two resolutions cleanly in section 3. I suggest renaming Section 3 to something like "Effect of Model Resolution on CO over Asia", with appropriate sub sections, this would allow the reader to really follow your arguments.

21) P5861 L15: A general comment: Be sure to say simulated CO mixing ratios when identifying the model results

22) Figure 1. I cannot see the dots of the cities in 1b, I suggest changing them to black and in general darken the lines of the countries/coasts. Also a box in figure 1a of the BTH region would be useful.

23) Figure 3. What are the dark vs. light features in Figure 3c., please describe in caption? Would suggest changing that panel for a topographical map

24) P5866 L23, extra space before comma

25) Figure 7. Suggest putting an L with a circle on successive days to identify the low.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5853, 2009.

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