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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The relative importance of various source regions on East Asian surface ozone" *by* T. Nagashima et al.

Anonymous Referee #2

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This paper presents the relative contribution of various source regions on surface ozone over East Asia using a tagged tracer method with a global-scale chemical transport model. Ozone tracers are tagged from a number of source regions, and the results are presented for East Asian receptor regions by separating the contribution of PBL, FT and ST ozone. My major concern is that the relationships between PBL/FT ozone and individual ozone precursor emissions regions should be further clarified. For example, FT ozone abundance can result from either inter-continental transport or lofting of local pollution. Simply FT contribution can not tell whether this is long-range transport or local pollution. My second suggestion is that the authors should include some discussion about the comparison of the S-R relationships with those in the HTAP papers, at least for EU->EA, and NA->EA results, since a different method is applied in this study. Some recent HTAP papers are published in the ACP/EMEP special issue,

http://www.atmos-chem-phys.net/special$_issue152.html$.

I would recommend publishing this paper in ACP after major revisions and some further clarifications described below:

Specific comments:

1. P9079, L15-20, Some references are needed here

2. P9080, L4-8, There is a major discrepancy in the increasing rate of NOx emissions between bottom-up emission inventories and satellite data. So the word "verified" is misleading here.

3. P9085, L15-18, Why there are more regions designated in the stratosphere than in the PBL and FT?

4. P9085, L18-25, Simply defining the PBL as the six lowermost layers in the model, is somewhat arbitrary. Is this definition consistent with model simulated PBL height? How about the diurnal variation of PBL height?

5. P9109, L1-15, the spring/early-summer maximum and summer minimum behavior of surface ozone along the East Asian coast has been well discussed in the literature, including the effects of regional pollution, biomass burning, monsoonal circulations and clouds. Findings from these papers should be acknowledged and described here:

Ding,A.J. et al: Tropospheric ozone climatology over Beijing: analysis of aircraft data from the MOZAIC program, Atmos.Chem.Phys., 8(1),1-13,2008.

He,Y.J. et al: Significant impact of the east Asia monsoon on ozone seasonal behavior in the boundary layer of eastern china and the west Pacific region,Atmos.Chem.Phys.,8(4),14927ï¡ÍC14955,2008

Lin, M. et al: Multi-scale model analysis of boundary layer ozone over East Asia, Atmos. Chem. Phys., 9, 3277-3301, doi:10.5194/acp-9-3277-2009

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6. P9091, L1-5, Discussion on the Japanese mountainous site (Happo): In addition to the topographical circulation, modeled excessive removal of ozone by dry deposition during the night may also contribute to the bias at the mountain site located above the nocturnal boundary layer (Lin M. et al., acp-9-3277-2009). Did you use modeled surface ozone for comparison? Since the model resolution is relatively coarse, consider interpolating the model results according to the pressure altitude of the observation site. The vertical interpolation should partly reduce the bias.

7. P9091, L7-15, Will the inadequate treatment of emission seasonality and/or the stratosphere-to-troposphere transport also contribute to the underestimate of surface ozone over northern Japan during winter and spring? Do your emissions have a seasonal variation? If not, certainly the underestimate of winter heating emissions from Northeast Asia should play a role in the model bias in the cold season. I am not fully convinced if decreasing dry deposition in wintertime is reasonable. First, the model reproduces wintertime ozone at the Mondy site located in the eastern Siberia reasonably well, suggesting that dry deposition may not be a major issue. Second, by decreasing dry deposition, the lifetime of ozone in winter will increase substantially, and thus affecting the budget of long-range transport contribution during winter. Your results show that "More than half of surface ozone is attributable to the ozone transported from distant sources outside of East Asia in the cold season (October to March)". Is this consistent with the estimates reported in the literature (e.g., HTAP papers)? Will the treatment of temperature-dependent dry deposition have an influence on the budget?

8. P9097, L9-12, Discussion on inter-annual variations of source-receptor relationships: Please clarify if your emissions have an inter-annual variation and how it may affect the variations of S/R relationships.

9. P9093, L10-15 (Figure 4), Please clarify if the contributions of FT are identical to contributions of O3 transported from outside of the East Asian PBL? How do you separate out the East Asian sources that are transported to the FT and brought back to PBL again, from the far distance sources like NA and EU? In other words, the FT

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contribution includes the EA O3 which are lofted into the FT during cold surges and then mixed down to PBL again during subsidence events?

10. P9098, L28 - P9099, L5. Discussion on nighttime ozone behavior is confusing since you indicated in P9098, L8-12 that only the daytime data (10:00-16:00) data are used.

11. P9099, L7-8: This sentence is confusing. Why are the hourly data not suitable to discuss the high-ozone events? Did you mean coarse-resolution model results?

12. Figure 6 and Figure 7: The global model missed the peak of local pollution events during summer (Figure 6). Then the statistics presented in Figure 7 are not reliable at all, especially for the high (60-90ppbv) and extra-high (>90ppbv) classes of ozone.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 9077, 2010.