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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Constraints on Asian ozone using Aura TES, OMI and Terra MOPITT" *by* Z. Jiang et al.

Anonymous Referee #2

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Summary

The study first uses MOPITT CO and OMI NO2 to constrain the global CO and Chinese NOx emissions in the GEOS-Chem model for the year 2006. For NOx, emissions are separately scaled for anthropogenic, lightning, soil, and biomass burning sources under the assumption of a linear relationship between the individual emission sources and their impact on the NO2 column. Independent lower tropospheric retrievals from TES are then used to evaluate modeled CO and ozone over Eastern China and its outflow region separately, for the model run with the 2006 optimized emissions, as well as 2006-2010 modeled with a priori emissions. Overall the topic is interesting as is the analysis, but the paper should more clearly connect the analysis with the conclusions being drawn by more clearly framing the questions being addressed here.

General comments

G1. The opening sentence of the abstract sets the stage for a discussion of the rising Asian anthropogenic ozone precursors on tropospheric ozone, yet this is absent from much of the rest of the paper. From Figure 2, the year-to-year increases in ozone and CO in June and to some extent July over E China do not seem to be transported into the outflow region. Does this imply that even if Asian emissions are increasing, the impact is regional and not detectable downwind? Some discussion of the findings of this paper in the context of rising emission trends seems warranted. What are the reported changes in Chinese emission from 2006-2010 and to what extent are these incorporated into the model?

G2. It's not clear how much data is actually available from TES to construct the regional monthly means examined here. Table 1 should include the sample sizes. Is the model being sampled consistently with TES? Are any revisions to understanding based on earlier ozone-CO work needed based on the findings here?

G3. How do the findings for 2006 regarding model biases, and the emission updates needed to match the satellite data, compare with earlier studies using satellite and aircraft data from the NASA INTEX-B field campaign (April-May 2006)? In general, more context could be provided to state clearly the new contributions from this paper beyond prior work focused on this region.

G4. The seasonal variation in the ozone chemical regime discussed here could be compared with earlier work (e.g., Martin, R. V., A. M. Fiore, and A. Van Donkelaar (2004), Space-based diagnosis of surface ozone sensitivity to anthropogenic emissions, Geophys. Res. Lett., 31, L06120, doi:10.1029/2004GL019416) Has there been a change detected in the seasonality of the ozone chemistry due to changing emissions over the past decade(s)?

Specific comments S1. What year-to-year scaling is applied to the Chinese emissions from the INTEX-B 2006 base year inventory? Could the increases in the red line in

June & July over E China be solely due to meteorological variability?

S2. The abstract comments on the seasonal variation of ozone; why not also the inter-annual variation? In the final sentence of the abstract, is this conclusion drawn from anthropogenic NOx contributions shown in Table 2 for the China outflow region? If so, it doesn't seem to hold for summer.

S3. Is the 3-7 ppb noted here for Asian emission increases on western North American free tropospheric ozone referring to the 2003 to 2010 period? If so, what is the increase expected for the 2006-2010 period examined here, and is this consistent with Figure 2, and to what extent does it require chemical production to occur during trans-Pacific transport? Some discussion would need to be added to support the statement in the summary asserting that the emissions from ROA and E China influence background ozone over North America. Given the current analysis, that statement appears speculative and should be removed if evidence is not added to support it.

S4. Why the summer focus here when spring is typically understood to be the season when Asian export peaks and has maximum impact on free tropospheric ozone and trans-Pacific transport (e.g., see www.htap.org reports from 2010 or 2007)?

S5. Section 2.3. It seems appropriate to provide a short description of the data treatment.

S6. If the Jiang et al. 2014ab are not yet available in ACPD, the relevant information for which they are being cited needs to be incorporated here.

S7. The assumption of a proportional relationship between emissions and NO2 VCDs seems problematic for lightning NOx (and possibly biomass burning NOx), which would probably have longer lifetimes than NOx from the other sources.

S8 Section 4.1 Is the 7 ppb TES bias specific to the region/time period being studied here? Is this determined relative to ozone sondes?

S9. How are the correlations in Table 1 calculated? Is the regional average first cal-

culated for ozone and CO, and then the correlation is done for daily regional means? See also comment G3.

S10. The conclusion of consistency between model and TES ozone-CO relationships (p19524) is subjective. It looks like in 5 out of 12 examples over E China, the model and TES suggest opposite signed changes from year to year. The abstract discusses seasonal variations, but the year-to-year changes seem more relevant given the motivation of rising emission trends.

S11. In terms of the optimized emissions in Fig 1, how do we know this isn't correcting for the OH bias discussed in the text? Is there seasonal variation in anthropogenic emissions in the model?

S12. Figure 4 suggests a similar spatial pattern for lightning and anthropogenic NOx so how is this a clean separation of anthropogenic vs. natural?

S13. Please explain why the chemical scheme is sufficient to evaluate this partitioning on the evidence of a small relative bias (Section 4.1 p 19525). The language in the final paragraph of 4.1 is confusing.

S14. P19526 statement referring to Mao et al. 2013. Please comment as to what current understanding suggests and whether the effects seen in Figures 4 and 5, showing ozone decreases associated with isoprene emissions, are consistent with this understanding.

S15. Where did we see the anthropogenic VOC contribution from China (P19529 L 24-25)?

Technical comments

T1. Please provide a short description of what a major quality flag equals 1 means.

T2. For the MOPITT a priori, are the MOZART-4 fields averaged over the same 10 degree x 60 degree grid as for TES?

T3. Section 2.2 please define what is meant by free troposphere; which retrieval levels are examined?

T4. Figures 4,5,6 should state the year for the adjoint calculations (2006)?

T5. Figure 2, is the model red line also smoothed with the TES AK?

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