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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Regional data assimilation of multi-spectral MOPITT observations of CO over North America" *by* Z. Jiang et al.

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We thank the reviewer for the thoughtful and detailed comments. Below we respond to the individual comments.

General comments

(1) This paper reports high resolution inversion of CO emission sources over North America using the nested version of GEOS-Chem model. The authors use the surface level retrievals from the MOPITT version 5 NIR+TIR data product to constrain the inversion. The paper is well written and logically organized in my opinion. The authors

provide a useful discussion of the initial and boundary conditions used for the model runs. Further they investigated the sensitivity of the inversion estimates to the OH fields and pointed out the potential issues and how they may be addressed in future work to improve these high resolution inversion analyses. They have also compared results from their inversions with aircraft data from INTEX-A bolstering confidence in their analysis. The research presented in this work is well within the scope of Atmospheric Chemistry and Physics and I recommend publication after revision.

Thanks for the comments!

Specific Comments: (1) While an impressive amount of work has gone into this paper, it will be nice to have a better understanding of some of their main results. In Figure 5, the authors show extensive areas in North America with strongly decreased a posteriori emissions in summer and attribute this to the oxidation of the biogenic VOCs there being a high bias of isoprene emissions in MEGAN 2.0 inventory that they have used for their simulations. If the summer decrease is due to high bias in isoprene emissions, then the spatial distribution of this anomaly should correspond to the observed isoprene spatial distribution. The isoprene distribution as deduced from HCHO retrievals from OMI shows a strong plume in summer essentially over the South Eastern US (Millet et al. 2008), and much less over the extensive areas in the western US going down to Mexico where the authors show strongly decreased a posteriori emissions (in addition to SE US) as can be seen in Figure 5, particularly between July and September. Therefore it is not clear to me if the wide spread discrepancy in CO emissions in summer can all be explained simply in terms of isoprene high bias in MEGAN 2.0. In any case, I believe the latter is now obsolete and the MEGAN version 2.1 with updates is available. Indeed in a recent paper, Hu et al., (2015, JGR, in press) used a similar high resolution nested grid version of GEOS-Chem with MEGAN 2.1 and found that the model adequately simulates the isoprene observations near a site in US upper Midwest. I would therefore urge the authors to redo their analysis using this updated inventory.

ACPD
15, C3215–C3219, 2015

The MEGANv2.1 inventory does reduce the overestimate in the isoprene emissions found in MEGANv2.0. However, as we noted in Jiang et al. (2015), with MEGANv2.1 GEOS-Chem still overestimates emissions in South America and Africa. It seems as though a key improvement in the implementation of the MEGANv2.1 inventory in Hu et al. (2015) is the use of a new land cover scheme. The resulting simulation does lead to a significant improvement in the modeled isoprene over North America. We now cite the Hu et al. (2015) paper in the manuscript. Unfortunately, this implementation of the MEGANv2.1 inventory is not yet available in the GEOS-Chem adjoint model so we cannot redo the analysis with the updated inventory.

We also believe that reduction in the emissions in Mexico and the southern US will, in part, be driven by the bias in the southern boundary conditions, which is associated with outflow from South America. We have added text in Sections 4.1 (at the end) and 4.2 (in the first paragraph) explaining this.

(2) I find it very interesting that several city scale features are showing up nicely in this plot. The authors mentioned about Toronto. More prominent are two persistent features over the Mexico City area and perhaps Monterrey for most of the year with low a posteriori emissions, except spring. The authors should discuss this–with the large amount of data over the Mexico City area from all the field missions and ground based measurements, it should be possible to find an explanation for the low a posteriori.

The top-down constraints on small-scale features are indeed interesting but it is important that we not overly interpret the results. As we noted in the paper, it is unclear how reliable these features are, given the information content of the MOPITT data. For example, using the nested GEOS-Chem model to carry out an inversion analysis of GOSAT CH4 data, Turner et al. (ACPD, doi:10.5194/acpd-15-4495-2015) estimated that the inversion produced only 39 independent pieces of information on the methane distribution (degrees of freedom for signal (DOFs)), although the inversion was conducted at the model resolution of 0.5° x 0.67°. Given the greater variability in the distribution of CO, we would expect more DOFs from the MOPITT inversion, but until

ACPD
15, C3215–C3219, 2015

we conduct such as analysis it would be premature to emphasize the city-scale source estimates.

(3) I am also a little intrigued that Kopacz et al. (2010) did not find any required adjustment over the entire US and Central America area in summer. Since the authors compare their results with Kopacz et al. (2010) for the same time period, they should add a discussion explaining this difference for the sake of completeness.

We did a detailed comparison between our analysis with Kopacz et al. (2010) in our global-scale inversion paper (Jiang et al. 2015). As we noted in Jiang et al. (2015), Kopacz et al. (2010) used much lower priori emissions, reflecting the significant (60%) summertime reduction in CO emissions recommended by Hudman et al. (2008) (see the discussion at the end of Section 4.1 in Jiang et al., 2015). Also, as noted in Jiang et al. (2015), the contributions from local VOC emissions were not included in the state vector in the Kopacz et al. (2010) inversion analysis. They aggregated the isoprene source with the methane source and optimized solved for global mean source from methane and VOC oxidation. Rather than duplicating what is already in the literature, we have added the following text to the manuscript: "As discussed in Jiang et al. (2015), the seasonal variations of the a posteriori source estimates obtained here are consistent with those of Kopcz et al. (2010), but the magnitude of the sources estimates differ significantly, reflecting differences in the configuration of the inversions analayses. We refer the reader to Jiang et al. (2015) for a more detailed discussion of the differences between the source estimates obatined here and those from Kopcz et al. (2010)."

Technical Comments:

(1) There are several discrepancies in the references: a) The reference Kilch et al., 2014 is not listed in the references. The authors likely mean the paper by Klich and Fuelberg which is listed. b) Palmer et al. 2003 missing in the reference list c) Kopacz et al., 2009 missing in the reference list d) Liu and Nocedal, 1989 year (2010) is wrong

ACPD

15, C3215–C3219, 2015

in the reference list.

Changed.

2. Figure 5 readability will be better if the x axis labels (latitude) are given only for the bottom panels. Further, the color scale may be changed so as to discriminate f=1 more clearly (say white, as in Kopacz et al. ,2010).

Changed.

3. Figure 1 will look better with color scales placed vertically.

Changed.

4. Figure 4 color bar for column CO should be placed on top of the figures alternately, put all the 3 CO column maps (Figure 4a-c) in one row and Fig 4d in a different row.

We have split the figure into Figures 4 and 5.

5. I think the authors need to use a consistent terminology for the sign of the source estimate. for instance, I found the sentences (page 5338) " The estimated winter emissions of Kopacz et al.(2010) are about 20% larger than the summer emissions. Kopacz et al (2010) and Stein et al. (2014) attributed the low bias of northern hemi- sphere CO in winter to an underestimation of road traffic emission" to be somewhat confusing.

Thanks! The text has been adjusted.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 5327, 2015.