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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.

Anonymous Referee #1

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Review of "Regional data assimilation of multi-spectral MOPITT observations of CO over North America" by Jiang et al.

General comments:

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. Fur-

ther 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.

Specific comments:

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. 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

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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. 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.

Technical comments:

1. There are several discrepancies in the references:

a) The reference Kilch et al., 2014 is not listed in the referencesâĂŤthe authors likely mean the paper by Klich and Fuelberg which is listedâĂŤ 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 âĂŤyear(2010) is wrong in the reference list.

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).

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

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

5. I think the authors need to use a consistent terminology for the sign of the source estimateâĂŤfor 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 hemisphere CO in winter to an underestimation of road traffic emission" to be somewhat

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confusing.

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