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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Inverse modeling of Texas NO_x emissions using space-based and ground-based NO_2 observations" *by* W. Tang et al.

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The authors would like to thank Reviewer 1 for the thoughtful and constructive comments. We appreciate the reviewer's description of the paper as well-written and as building on the work of previous analyses. Following are responses to each of the reviewer's specific comments:

1. "The comparison of the AQS inversion to the satellite inversion needs more discussion, particularly the diurnal variability in the ground measurements and the spatial distribution of the sites themselves. This context will probably help to explain the difference in the inversion results. In my opinion, this is the most significant finding of the study and definitely needs more attention."

Lines 20-28 on page 17494 and Figure 1 discuss and illustrate the spatial distribution of the ground monitoring sites. Figure 6 shows the diurnal variability of the ground measurements and the corresponding model results. We revise lines 25-27 in the conclusion section (page 17497) as below to elaborate the causes of difference in the inversion results, in response to the reviewer's suggestion:

"The AQS ground NO2 measurements face limitations due to the inaccuracies of the molybdenum converter method. Furthermore, the mostly urban locations of measurement sites (Fig. 1) may be unrepresentative of the entire region, and do not capture the rural areas where OMI observations suggest NO2 is underestimated. In addition, model shortcomings in simulating PBL heights in the early morning and late afternoon (Fig. 6) may contribute to the low scaling factors in the ground-based inversions."

2. "I am not sure that devoting space in the manuscript to the scaling inversion is warranted. The authors state compelling reasons for why it is not appropriate for this case. This is then confirmed by the application. Perhaps, it's enough to explain why it is not appropriate and then just state that the authors tried it and it failed. I am not sure much is gained from actually showing the results and the accompanying discussion."

The Direct Scaling method was originally pursued to provide an alternate approach featuring more spatial heterogeneous adjustments to emissions. We agree that the Discrete Kalman Filter is a more effective inversion method for this application, and that the results demonstrated its advantages over direct scaling. The results from the DS inversion have been moved to the supplementary material to show that it was attempted while emphasizing the DKF methods and results.

3. "Perhaps more justification or at least discussion of the "additional NOx emissions" might be helpful. For example, it might helpful to include some tables comparing domain-wide emissions totals for the various sources and compare those to "lightning" and "biogenic" to show bulk impacts of your changes on the troposphere. Since more rural regions require larger scaling in the satellite inversion, perhaps, these additions

are still not sufficient."

We calculated monthly averaged NOx emission rates from 7 emission categories for both modeling episodes as shown in Table R1. Lines 1-5 on page 17497 note that the lightning and aircraft NOx emissions added an additional 14% and 6% NOx to the total emissions for the June episode, and 7% and 6% NOx for the Aug-Sep episode. The doubled soil NOx added additional 8% NOx emissions for both episodes. The reviewer correctly notes that even adding these additional NOx emissions did not fully close gap between model and satellite-observed estimates of NO2 columns in rural regions. Other studies have noted similar gaps. Allen et al. (2012) found that the modeled NO2 is still under-representing in the upper troposphere after incorporating the lightning emissions, and Henderson et al. (2012) suggested that the uncertainty could also from the chemical reactions in the upper troposphere.

4. "Table 1 could probably be shown as part of the legend of Figure 3a or not at all, because the figure kind of shows the scaling."

We adopt the reviewer's suggestion by deleting Table 1 and adding the perturbation factors into Figure 3a.

References

Henderson, B. H., Pinder, R. W., Crooks, J., Cohen, R. C., Carlton, A. G., Pye, H. O. T., and Vizuete, W.: Combining Bayesian methods and aircraft observations to constrain the HO + NO2 reaction rate. Atmos. Chem. Phys., 12, 653-667, 2012.

Allen, D. J., Pickering, K. E., Pinder, R. W., Henderson, B. H., Appel, K. W., and Prados, A.: Impact of lightning-NO on eastern United States photochemistry during the summer of 2006 as determined using the CMAQ model. Atmos. Chem. Phys., 12, 1737-1758, 2012.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 17479, 2013.

Modeling episodes	Area (tons/day)	Mobile (tons/day)	Non-road (tons/day)	Biogenic (tons/day)	Aircraft (tons/day)	Lightning (tons/day)	Elevated points (tons/day)	Total (tons/day)
June	453	760	374	237	172	434	543	2974
Aug-Sep	290	766	402	232	171	226	547	2634

Fig. 1. Table R1. Categorized NOx emission rates in inversion region for two modeling episodes.

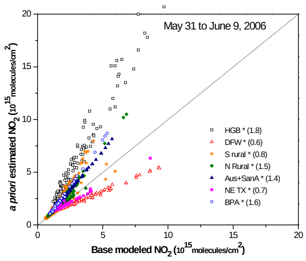

Fig. 2. Figure 3a. Perturbed case in Pseudodata test.