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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Key chemical NO_x sink uncertainties and how they influence top-down emissions of nitrogen oxides" *by* T. Stavrakou et al.

Anonymous Referee #2

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This manuscript is a well-conceived and executed investigation of the impact of uncertainties in kinetic parameters and OH levels on top-down constraints on NOx emissions. The authors acknowledge that there are several additional factors that may influence the sinks of nitrogen oxides in the atmosphere, but restrict their analysis to four important factors. In my opinion, the main achievement of the manuscript is to highlight the sensitivity of the inversion framework to known uncertainties related to NOx sinks. While the authors do carry out model inversions using scenarios with extreme (but plausible) combinations of NOx sinks and compare the optimized NOx sources to the prior estimates, the focus remains on the sensitivity. As the authors point out, the only

anthropogenic source region for which the optimized source changes in a consistent direction, regardless of the sink parameterization, is in China. Interestingly, both scenarios also suggest upward revisions for the fire and soil sources of NOx.

A recent publication (Lin et al., 2012) explores model sensitivity of nitrogen oxides to a wider range of factors, including meteorological parameters, but using a higher resolution model for a specific region. Given that the current manuscript is carrying out a global analysis and is more directly examining the impacts on the inversion framework, I think the focus on the factors identified by the authors is appropriate and well-motivated in the text.

The paper is appropriate for publication in ACP after consideration of the following points:

Specific comments:

I don't follow the logic about chemical feedbacks (P 7891 L 6-10, and P 7893 L 19-25. In the MAXLOSS scenario, why would there by higher levels of NOx in the atmosphere? I can see why increasing the sink would also require increasing the emissions in the inversion framework, but not to the point where the mixing ratios are higher. Please clarify if NOx levels are actually higher in the tropics in the MAXLOSS scenario.

P 7884 L 1-15 It would be useful to know what CTM DOMINO v2 uses to determine a priori NO2 vertical profiles, and how errors in the NOx sinks in this CTM would feed back into the retrieval. In particular, how would the comparisons in Section 5 change if the MINLOSS and MAXLOSS parameters had been used in the CTM that was used in the DOMINO retrieval. Would this make the comparison more internally consistent?

P 7891 L 4 - The discrepancies are largest for natural sources only in a relative sense. For example the absolution change optimized anthropogenic emissions for MAXLOSS is larger than for lightning.

Technical corrections:

P 7873, L 2, ‘comforting’ should be replaced with ‘consistent with’

P 7892 L 10-11, suggest rewording “Should this be confirmed. . . 1Tg N.” as “This lower estimate for NO production efficiency translates to an annual lightning NOx source of 1 Tg N.”

P 7892 L24-26, suggest rewording “ Exception is made. . . a priori” as “In contrast, both the MINLOSS and MAXLOSS inversions result in an increase in anthropogenic emissions from China to 5.8 and 6.5 Tg N, respectively, significantly higher than the prior of 4.8 Tg N.”

P 7895 L 9-11, suggest rewording to “Comparisons above selected regions between SCIAMACHY and modelled NO2 columns. . ..”

Reference

J.-T. Lin, Z. Liu, Q. Zhang, H. Liu, J. Mao, and G. Zhuang, “Modeling uncertainties for tropospheric nitrogen dioxide columns affecting satellite-based inverse modeling of nitrogen oxides emissions” Atmos. Chem. Phys., 12, 12255-12275, 2012

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 7871, 2013.