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Atmospheric Measurement Techniques Discussions

Interactive comment on "Inverse modeling of NO_x emissions over eastern China: Uncertainties due to chemical nonlinearity" *by* Dasa Gu et al.

Anonymous Referee #2

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The manuscript by Gu et al. (2016) focuses on inverse modeling of NOx emissions over China using GOME-2 and OMI satellite measurements. Inversion is done with previously used "local derivative" and "bulk ratio" approaches. The emission estimates from the two approaches are compared, and limitations of the "bulk ratio" approach applied to both single and multi-satellite measurements are discussed. Main strength of this paper lies on the application of the single-cell based emission perturbation scheme in the model to compute the sensitivity factor. The paper is well-written, and should be of interest to the AMT readers. However, a few issues listed below need to be addressed before the paper can be recommended for publication.

(A) Major Comments:

1) Satellite measurements: A careful comparison and characterization of the two satel-

lite retrievals is necessary for a credible emission estimates. Retrieval algorithms for OMI and GOME-2 differ on few aspects (fitting, surface reflectivity, cloud), not just a-priori NO2 profiles as discussed in the manuscript. In fact, a-priori NO2 profiles are less of an issue since they can be replaced with user-supplied model profiles using the auxiliary information (averaging kernel) contained in the data file. For consistency between retrievals and emission estimates, this is a necessary step, but it is unclear if that is indeed done.

2) Uncertainty in satellite measurements and emission estimates: The manuscript should expand this aspect – how are they calculated? Discussion of uncertainty calculation for tropospheric AMF is unclear. Reported uncertainty in stratospheric SCD sounds too large. If uncertainties in tropospheric NO2 are indeed calculated, I recommend including uncertainty figures for both OMI and GOME-2 data.

3) Simulated NO2 column vs NOx emissions: This is very important part of the manuscript, but I have difficulties in understanding and interpreting it. First, the scenario presented in Figure 1 does not fully represent eastern China as NO2 columns vary only up to 3x1015 molec cm-2, not typical of eastern China. Second, I have problem to interpret OMI curve that indicates saturation at NOx emission of $\sim$2.6x1015 molec cm-2 hr-1. Does that mean a decrease in NOx emission would still increase NO2 column by up to 50%? Would a 25% increase in emission result in a factor of two increase in column? Why would these happen? Third, please consider reversing the axes for clarity.

(B) Minor Comments:

4) Page 2, line 28: "There" => "They".

5) Introduction: This section should acknowledge similar work by other groups (e.g. Boersma et al., 2008; few papers using adjoint modeling).

6) Page 4, line 81 (and method section): How was source speciation done?

7) Page 5, line 93: Two GOME-2 instruments are in operation currently. Please, be specific that you are using GOME-2A measurements.

8) Page 5, line 109: Please, include appropriate reference for this statement on $\sim$10% error from a-priori profile.

9) Page 6, line 125: Does not the mode include soil NOx emissions? Please include information about this source.

10) Page 7, line 143: What does “a correction of profile” mean, and how is it done?

11) Page 7, line 156: “inversed” => “inverted” .

12) Page 7, line 156: Why “either OMI or GOME-2 observations” and why not both?

13) Page 9, line 196: “inversed” => “inverted”.

14) Page 10, lines 211-212: Please, use appropriate symbols for T and tau.

15) Page 11: line 238: “inversing” => “inverting”.

16) Page 23, Table 1: How was the total emission for East China (last column) calculated? You mentioned that you analyzed the data for August, 2007 only. Was the analysis expanded other months as well?

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