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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite constraint for emissions of nitrogen oxides from anthropogenic, lightning and soil sources over East China on a high-resolution grid" *by* J.-T. Lin

Anonymous Referee #2

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The manuscript by Lin is a thorough effort to provide a comprehensive estimate of NOx emissions in China for the year 2006. The author brings together state-of-science models and datasets, and by combining the modeling results in an appropriate manner with the satellite measurements, the author is able to draw conclusions on total NOx emissions over China. The manuscript is a straightforward extension of previous work, that is appropriately cited, but it is at the same time innovative and new in the sense that this is the first inversion over China that uses the high-resolution (0.5 deg x 0.67 deg) nested-grid GEOS-Chem model, in combination with a recently improved OMI satellite dataset. The description of the multivariate regression has much improved compared to an earlier version that I saw.

My main concerns with the manuscript are related to the use of the GEOS-Chem model here. A number of important GEOS-Chem characteristics go undiscussed.

* First of all, which mixing scheme has been used, the original 'instant mixing' scheme or the recently implemented non-local mixing scheme? This is an important issue as it affects vertical distributions of NO2 (important when using the kernel and when dealing with lightning NO2 aloft) and O3 (important in view of potential non-linearities in the inversion method).

* Secondly, it should be made clear right from the beginning what the emitted totals are for anthropogenic, biomass burning, lightning, and soil NOx in GEOS-Chem. Then, with respect to the lightning NOx production, the authors cite an unpublished and unaccessible paper by Murray et al. I have some idea of the method by Murray et al., who appear to be using LIS and OTD to provide horizontal constraints on the lightning flash frequency. Since LIS coverage extends to only 30-35 deg (latitude), and the OTD mission ended in 2000, the horizontal redistribution of flashes over much of China is climatological constraint at best. The author should provide more information on how the lightning NOx production has been done in this version of GEOS-Chem.

* Also, to build confidence that it is actually possible to simultaneously obtain information on lightning and soil NOx patterns vs. anthropogenic NOx patterns, the author should give an idea on the orthogonality of these patterns. Unlike the other reviewer, I believe it is possible to obtain information on lightning and soil NOx emissions over China, as long as these emission categories are sufficiently orthogonal (not just in time but also in space) from the anthropogenic emissions. However, I agree with the other reviewer that the patterns shown in Figures 9 and 10 look pretty questionable with apparent hotspots of top-down lightning and soil NOx emissions that happen to coincide with China's highly populated regions. The authors should clarify all these issues, and put forward a more convincing case as to why the top-down constraints are also meaningful for lightning and soil NOx.

* One last issue I have is with the discussion of the linearity of NOx emissions vs. NO2 columns. In the initial method, the assumption is that NO2 columns respond linearly to changes in NOx emissions, however, the results indicate that the sensitivity is less than one. The authors should discuss the implication of this non-linearity on the ultimate conclusions of this study.

Specific comments

P29808, L18-19: 'They are each less than 6% of anthropogenic emissions annually'. Later on, it becomes clear what the authors mean, but perhaps this can be rephrased here to make clear that lightning NOx and soil NOx each make up less than 6% of the anthropogenic NOx emissions.

P29810, L26-28: The statement that biomass burning emissions are unimportant over China came to me as a surprise. I've seen papers where biomass burning does make a significant contribution to e.g. HCHO emissions. I think the author should provide some more information on how small the contribution of biomass burning NOx emissions is, and give some references. This issue comes back at P29816, L22. Introduction: although it is clear, the introduction reads somewhat as a literature overview, and lacked some focus to me. Why is the author so interested in NOx emissions over China?

P29812: it seems to me that the author should provide some more basis for assuming a larger retrieval error in winter than in summer. It is true that some satellite retrieval errors are probably larger in winter (lower solar zenith angles, thinner boundary layers so more impact of albedo and NO2 profile errors), but aerosol errors could well be larger in summer, when AOT is generally higher. I think Fig. 2 is unnecessary (and holds very little information at all), and the author should give more justification for his (assumed) seasonal behavior of the retrieval errors.

P29813, L11: 'contribute only to 6%', please remove 'to'.

Section 4.1.2: this part is not really clear to me. I appreciate the author's effort to

communicate his method as complete as possible, but this appears more suitable for an Appendix. Instead of all this, it would be better to provide more detail on how lightning and soil NOx are modeled in GEOS-Chem.

P29818, L1: 'many of the areas with stripe patterns in Fig. 7' Which patterns are referred to here?

Figure 3: titles are hard to read in the paper version.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 29807, 2011.