

Interactive comment on “Influence of aerosols and surface reflectance on satellite NO₂ retrieval: seasonal and spatial characteristics and implications for NO_x emission constraints” by J.-T. Lin et al.

Anonymous Referee #2

Received and published: 19 June 2015

The manuscript presents sensitivity studies to elucidate the impact of the treatment in aerosol properties and/or surface reflectance on the retrievals of NO₂ and top-down estimates of NO_x emissions at regional scale over China. Understanding systematic biases in the retrievals is important especially since NO₂ retrievals have been used recently in several key science and policy-relevant studies (e.g., emission estimation). While this is a direct extension of their previous work (Lin et al. 2014b), the results presented in this work have potential contributions worthy of publication.

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However, the reviewer has the following concerns:

- 1) It is important that results be validated with independent observations (e.g., ground-based measurements). While it is understandable that there are limited measurements to compare with, the results currently presented can only be interpreted qualitatively without some form of validation. A similar comment in the discussion has also been made in this regard. Systematic biases as elucidated by the sensitivity experiments can be due to biases in the inputs as well (i.e., MODIS BRDF, GEOS-Chem aerosol properties, and other prior information).
- 2) What are new additional important findings in this work, which were not reported in Lin et al 2014b? This distinction is not clear in the presentation. A shift in focus on these new findings would strengthen this paper.
- 3) It is not clear whether the results of the sensitivity experiments can be interpreted in a robust manner. First, it appears (from the presentation) that the comparison between REF (POMINO) and DOM (DOMINOv2) is not a fair comparison. As mentioned by the authors, the interpretation of CRF is different between the two. The ‘implicit’ assumption in DOM is not entirely neglecting the aerosol contribution as it is interpreted to be the combined effect of cloud and aerosols (‘effective’, by way of retrieving the cloud properties). In addition, the use of ‘valid pixels’ for REF alone biases the comparison with DOM given that some criteria of pixels being valid are related to CRF. It would strengthen this paper if the difference between DOM and POMINO are better described and that the implicit assumption versus explicit representation is better clarified. Can the systematic biases be quantified in DOMINO retrieval algorithm (as ‘model’ errors)?
- 4) Some descriptions and discussions are not clearly presented. Some terminologies and acronyms need to be described and explained, especially for readers unfamiliar with Lin et al. 2014b. Organization of Figures (numbering) is confusing. See specific comments.

Specific Comments: 1) Abstract: Please briefly define/elaborate LIDORT AMFv6,

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MODIS AOD, OMLER v1, 'subsequently-constrained'.

2) Line 14 p. 12657: please briefly elaborate 'row anomaly issues'.

3) Line 24 p. 12659: please briefly elaborate OMCLD02 v3.

4) Line 10-25 p. 12660: How good are the GEOS-Chem NO₂ and aerosol properties, GEOS-5 profiles over China? What is the implication of using a 'relatively' coarser resolution of prior information from GEOS-Chem on the retrieval. Is the retrieval carried out at 'native' resolution of OMI or is this done after gridding to 0.25 and monthly scale? How would this impact the interpretation of your results especially in terms of consistency, variability and errors presented? What is the rationale behind using GEOS-Chem information instead of MODIS?

5) Line 15-16 p. 12661. Please elaborate. Is the interpretation of the results on the differences between retrieval methods affected by this?

6) Line 7 p. 12664. 'large-scale' retrieval. Please elaborate.

7) Order of figure discussion and introduction is confusing. Figure 3 is discussed after Figure 4 and Figure 5 for example.

8) Line 20-21 p. 12665. Is this something the authors can compare quantitatively with MODIS data?

9) Line 8-9 p. 12666. Similar to comment 8, can this relative uncertainty in NO₂ retrievals due to aerosols be reasonably quantified by using different aerosol fields?

10) Line 10-15 p. 12666. How is this manuscript different from Lin et al. 2014b, given that Lin et al. 2014b carried out similar sensitivity studies on explicit vs implicit assumption?

11) Line 23 p. 12667. How would this criterion (on valid pixels) bias your comparison with DOM?

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12) Line 4 p. 12668. Related to comment 11, why would these differences reflect dissimilar AMF approaches given that 'invalid pixels' for DOM may actually be representing this difference?

13) Line 20-21 p. 12668. Was this resolution (0.05) used in the retrieval or was this regridded to 0.25?

14) Line 11-12 p. 12670. Again, what would be the difference between this study with Lin et al. 2014b?

15) Line 13-24 p. 12670. Should this discussion be more appropriately presented in the aerosol section or 'coupled' section?

16) Line 12-17 p. 12675. What would be the difference between this study with Lin et al. 2014b?

17) Line 25-28 p. 12678 and Line 1-2 p. 12679. Please elaborate on the interpretation of 52% error in top-down etc. This is especially important for readers not familiar with Lin et al. 2012. Also, what is the impact of assuming the same errors for all grid cells in your emission estimates?

18) Line 29 p. 12679. Please elaborate. Does this mean that there is no significant difference overall? What are the implications for this?

19) Line 5-6 p. 12680. What is the rationale for masking low emissions?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 12653, 2015.

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