

Interactive comment on “Improved aerosol correction for OMI tropospheric NO₂ retrieval over East Asia: constraint from CALIOP aerosol vertical profile” by Mengyao Liu et al.

Anonymous Referee #1

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This study focuses on the impact of aerosols in tropospheric nitrogen dioxide (NO₂) retrievals from the Ozone Monitoring Instrument (OMI). It is an extension of previous work from the same group. The extension is related to integration of CALIOP/CALIPSO data in their previous approach of constraining GEOS-Chem aerosol information with MODIS AOD observations. The authors claim that by doing so improves OMI tropospheric NO₂ retrievals as judged from enhanced correlation of the data with the same set of MAX-DOAS NO₂ observations the group has been using for data assessment. This reviewer isn't convinced with that claim, and feels that the manuscript requires substantial revisions if it is going to be published in AMT.

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General comments

1. With this manuscript, the authors are delivering a new version of the POMINO product. This product is based on slant columns retrievals from DOMINO, uses nested GEOS-Chem simulation, and applies MODIS and CALIOP/CALIPSO data for AMF calculation, with each item presenting concerns for reporting it as a new version of the POMINO product. NO₂ slant column retrievals used here is version 2 product, which is reported to be erroneous. NO₂ profiles and other model dependent parameters are taken from two versions of nested model that switch from one resolution to another in 2013. They apply CALIOP/CALIPSO data over 2007-2015 to OMI data taken over 2004-present. MODIS AOD data are taken from two versions (collection 5.1 and collection 6 with a switch in 2013). There is enough ground to suggest that the product is erroneous, is not consistent over time, and should not be distributed to users as an updated version.

2. To justify the improvement in the retrieved product, authors have used a small set of MAX-DOAS measurements. Improvements are justified based on improved correlation coefficient with the POMINO product. It appears from Figure 10 that the enhanced correlation might, in fact, be driven by changes in ~ 6 data points only with very large ($> 100 \times 10^{15}$ molec cm⁻²) values. In many instances (for columns $< 100 \times 10^{15}$ molec cm⁻²), the agreement between OMI and MAX-DOAS appears to be better for DOMINO. Author should use different means of validation, larger set of validation datasets, and various statistical methods to assess the products.

3. The whole discussion about processing (filtering, regridding) and comparison of CALIPSO data is distracting and unnecessary. These could be completely removed, shortened, or moved to the Appendix/Supplementary section. Also, data processing is largely subjective. Why not use more mature data assimilation technique instead?

Specific comments

1. Page 9, line 225: This statement may not be true. Please, replace “will not” to “may

not”.

2. Page 9, line 227-231: Please be more specific on AMF calculation. What wavelength range is used for AMF for POMINO/DOMINO? I assume this is more important than the difference between online and look-up table approach.

3. Page 9, line 228: This paper is all about POMINO and DOMINO. Please, say “DOMINO” instead of “in most retrieval algorithms”.

4. Page 10, line 237: What are those “Other aspects”? Please, list them.

5. Page 10, lines 237-239: This statement is likely misleading as look-up table may have been used in certain aspect of your calculation. Please, remove “without use of look-up tables”.

6. Page 10, lines 239-244, 257-259: See my general comment. The same product cannot use simulated fields from two different models. The retrievals should be based on single model.

7. Page 13, lines 314-316: How does the se of CALIPSO constraints affect cloud pressure, cloud fraction, and radiative cloud fraction? Please include relevant results and discussions.

8. Page 13, lines 321-325: Please, clarify this statement.

9. Page 14, line 360: What is the justification of 2-hour averaging of MAX-DOAS? Why do you expect instantaneous OMI measurements compare well with MAX-DOAS averaged over 2-hours? Is this exercise described in the following sentences motivated to show only good results?

10. Page 15, lines 366-367: “to some degree” is redundant.

11. Page 15, lines 374-375: Why is this necessary? How do cloud and haze differ for their impact on measurement sensitivity of OMI?

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12. Page 16, line 395: Please, add citations for this statement.

13. Page 17, line 418: How does the emission strength affect the height of peak extinction?

14. Page 19, lines 470-472: The spatial correlations suggest that GEOS-Chem performs very poorly in simulating aerosol fields. Why do you still use GEOS-Chem? Could not you just use CALIPSO-based aerosol information?

[Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2018-34, 2018.](#)

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