Review of "Minimizing aerosol effects on the OMI tropospheric NO2 retrieval – An improved use of the 477 nm O2 -O2 band and an estimation of the aerosol correction uncertainty" by Chimot et al.

This paper describes results of applying the aerosol correction to the OMI operational NO2 algorithm, DOMINO-v2. The aerosol correction is applied in two different forms: the implicit aerosol correction using the improved OMI operational O2-O2 cloud algorithm, OMCLDO2 v2, and the explicit correction using aerosol parameters derived with an aerosol neural network (NN). The authors conclude that both approaches to the aerosol correction reduce the biases identified in DOMINO-v2 over polluted cloud-free areas. The paper contains significant original material that can be of interest for the developers of cloud and trace gas algorithms for satellite sensors. The paper subject is appropriate to AMT. Earlier work is adequately recognized and credited. The abstract provides a sufficiently complete summary of the paper. I recommend the paper for publication after the authors address the following comments.

General comments

1. The aerosol model used in simulations assumes a single value of the asymmetry parameter for the Henyey-Greenstein (HG) phase function and a single value of the Angstrom exponent. The model seems to be oversimplified. There is no justification for the selection of these parameters. To support the choice of the HG function, the authors reference the paper by Dubovik et al. (2002). However this paper does not discuss the HG phase function. Additionally, the assumed aerosol model is not consistent with an aerosol model used in the MODIS aerosol retrieval algorithms while the authors propose to combine the MODIS AOT retrievals with their aerosol model in the explicit aerosol correction (see P.8, L. 14).

2. The authors consider absorbing aerosols only with relatively low values of the single scattering albedo (SSA) of 0.9 and 0.95. Maps of the seasonal mean SSA at 500 nm retrieved from OMI from 2005 to 2016 (Fig. 5, Kang et al., Remote Sensing, 9, 1050, 2017) show that SSA values over China are greater than 0.96-0.98 in summertime and mostly greater than 0.92-0.94 in wintertime. The authors should redo their calculations shown in Fig. 4 with more realistic values of SSA for summertime over China.

3. In Sect. 5.5, the authors discuss an important issue of the TOA radiance closure in case of using the MODIS AOT retrievals with OMI-derived climatological surface LER. The authors correctly state that OMI-derived LER is inconsistent with the surface BRDF used in the MODIS aerosol algorithm. Moreover, there are significant differences in absolute values of the OMI-derived LER and MODIS atmospherically corrected surface reflectance/albedo. Those differences are due to that the climatological LERs include a contribution from inevitable aerosol

contamination and possible cloud contamination for OMI pixels which are much larger than MODIS pixels. Because of this essential inconsistency I would suggest to consider dropping the use of the MODIS AOT in the explicit aerosol correction.

4. Results of the use of the explicit aerosol correction are shown for averages over 3 months (see Fig. 4-6). However, it remains unclear how to use the explicit aerosol correction operationally, on an orbit by orbit basis. It would be quite beneficial for a reader to provide practical recommendations for operational processing with the explicit aerosol correction.

5. There are many typos in the manuscript. Some of them are listed in Technical notes below.

Specific comments

P.3, L.22. Please consider adding references to the papers published by other research groups, for instance:

Joiner et al., Retrieval of cloud pressure and chlorophyll content using Raman scattering in GOME ultraviolet spectra. J. Geophys. Res., Vol.109, D01109, doi:10.1029/2003JD003698, 2004.

Vasilkov et al., A cloud algorithm based on the O2-O2 477 nm absorption band featuring an advanced spectral fitting method and the use of surface geometry-dependent Lambertian-equivalent reflectivity, Atmos. Meas. Tech., 11, 4093-4107, doi:10.5194/amt-10-4093-2018, 2018.

P.5, L.13. There are contributions of ozone absorption and Raman scattering to the top-of-the-atmosphere radiance at 475 nm. How the ozone absorption and Raman scattering are accounted for in the definition of the continuum reflectance at 475 nm?

P.5, L.23-24. The surface reflectance is considered “as a second order effect” on the O2-O2 slant column density. This must be justified.

P.7, L.29. Please clarify “the nature of the O2-O2 spectral band”. What do you mean?

P.11, Fig. 2. The NO2 bias dependence on AOT is quite non-monotonic. It looks too “bumpy” to be real. Please explain so strange behavior of the curves in Fig. 2.

P.11, Last paragraph. Data in Fig. 3 are shown for AOT(550) up to 1.5 which corresponds to AOT(475) of about 1.9. So high values of AOT may lead to effective cloud fractions exceeding the threshold of 0.1 that was stated on Page 8, Line 29. Please provide values of effective cloud fraction for those data.

P.11, L.14-17. Just two sentences describe Fig. 4-6. They deserve more lengthy discussion.

P.13, Fig. 9 discussion. Please explain why there is so big difference in the behavior the light blue (SSA=0.95) and green (SSA=0.9) curves in Fig. 9a and Fig. 9b. The curves are close to each other in Fig. 9a and they are dramatically different in Fig. 9b.

P.14, L.16. There is no discussion about ALH in Sect. 3.2.

P.15, L.30. The authors state that the single scattering albedo (SSA), the asymmetry parameter, and the Angstrom exponent are "of second importance". This statement should be justified. Please explain why calculations are carried out for two values of SSA and for a single value of the asymmetry parameter and the Angstrom exponent? Are they "of third importance"?

P.16, L.4-5. It is quite desirable to show NO2 profiles used in calculations.

P.16, L.28. Please clarify the meaning of "an insufficient coverage of the observation scene".

P.17, Sect.5.4. Please provide a value of the effective cloud fraction in Fig. 13b.

P.34. No captions for Fig. 4c and 4d.

P.35. Fig. 9d, 9e, and 9f are missing while they are mentioned in the figure caption.

Technical notes

P.1, L.4. "Minimizing … are", should be "is"?

P. 2, L. 8. "health population". What do you mean?

P.2, L.10. Should be "Ozone".

P.2, L. 18. "mapping ... have", should be "has"?

P.3, L. 5-8.It is hard to follow. Please reword or split the sentence.

P.3, L.7. Should be "Orthogonal"

P.4, L.21. A typo. Should be "477"

P.5, L.9. Remove "x"

P.6, L.4. and elsewhere, e.g. P.7, L.28; P.11, L.14; P.14, L.12. Should be subscripts.

P.10, L9. "One the main". "of" is missing.

P.10, L.17. A typo in "information"

P.11, L.33. "attenuate the biases". Do you mean "reduce"?

P.13, L.33-34. It may be hard to understand this sentence. Please clarify.

P.13, L.34. “This may of course be …”. What is “this”?

P.16, L.29. Remove period after “scene”

P.16,L.31. Should be “potential”?

P.17, L.4. NO2 vertical column “degradations are more important”. Please reword.

P.17, L.18. Should be 550 nm?

P.17, L.25-26. You may want to reword “the transmission of the clear fraction of the pixel through the IPA assumption”. Otherwise, it is not clear.

P.30. Fig. 4 and elsewhere in other figures. The Greek symbol “tau” in Fig. 4a is misspelled.

P.37. Figure caption. Second (a) should be (b).