Interactive discussion on AMTD-2017-286 "Minimizing aerosol effects on the OMI tropospheric NO_2 retrieval – An improved use of the 477 nm O_2-O_2 band and an estimation of the aerosol correction uncertainty"

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We would like to thank the Associate Editor for his feedbacks and suggestions. Below we address them one by one (Associate Editor comments in blue, author and co-authors in black).

Dear authors,

Thank you for submitting your revised manuscript. After reading it through together with your Response to Reviewers, I felt that you did a good job in the Response but that a couple of points were not fully accounted for in the revised text. These are things that you explain well in the Response to Reviewers but could benefit from adding an additional sentence or two in the manuscript, in case readers have similar thoughts. I therefore request the following minor additions, which I would quickly review before accepting the manuscript for publication in AMT. Specifically, these are:

1. The reviewers questioned the choice of the Henyey-Greenstein (HG) phase function. In your response you explained how this was evaluated in your previous 2016/2017 papers, which I think is reasonable, but I'd like to see this more clearly in the text. Could you add a sentence or two mentioning that HG has limitations but was evaluated for this purpose in the 2016/2017 papers somewhere probably early in section 2.3?

We added the following in Sect 2.3.:

"The HG phase function is known to have some limitations compared to more physical models. Nevertheless, it was consciously chosen in Chimot *et al.* (2017) as the main motivation has been as exploratory development of an ALH retrieval algorithm, using the OMI 477 nm O_2-O_2 absorption band, has been the aerosol correction in the visible spectral range in view
30 of tropospheric NO_2 retrieval. Chimot *et al.* (2016) quantitatively demonstrated that, for such a purpose, τ and ALH are the key parameters needed. Other aerosol parameters, that are more related to their optical properties, shape, and size are of a second importance. This is supported by a significant number of additional studies (Boersma *et al.*, 2004; Leitão *et al.*, 2010; Castellanos *et al.*, 2015). The main reason is that aerosol correction needs the length of the average light path in presence of scattering and absorbing particles. This is primarily driven by τ and ALH (in addition to the shape of the NO2 vertical profile), much less by the detailed properties of particles. Consequently, other details describing the shape of the scattering phase function are of second importance, even if not negligible. Moreover, areas impacted by heavy NO_2 are generally dominated by fine spherical particles, weakly absorbing (*e.g.* sulfate, and nitrate) or strongly absorbing (*e.g.* smoke) like in East China, South America, and Russia areas with scenes including urban, industrial, and biomass burning pollution events and for different seasons Chimot *et al.* (2017, 2018). Spheroid particles such as dust are sometimes mixed but do not overall dominate.

The HG function is known to be smooth and reproduce the Mie scattering functions reasonably well with g = 0.7 for most of aerosol types, especially for spherical particles (Dubovik *et al.*, 2002). A similar approach is considered for the operational ALH retrieval algorithms for Sentinel-4 and Sentinel-5 Precursor (Leitão *et al.*, 2010; Sanders *et al.,* 2015; Colosimo *et al.,* 2016; Nanda *et al.*, 2017), and when applying various explicit aerosol corrections in the tropospheric NO_2 AMF calculation 10 over urban and industrial areas dominated by anthropogenic pollution, for instance in east China (Spada *et al.*, 2006; Wagner *et al.*, 2007; Castellanos *et al.*, 2015; Vlemmix *et al.*, 2010)."

2. The reviewers also raised the issue of radiative closure resulting from MODIS (rather than OMI) as a source of AOD. In your Response you mention that you think it's important to keep these results in

the manuscript because as you note this possibility is not "naturally obvious". I agree with you about this and support keeping the analysis as-is, but again adding a sentence somewhere in Section 5.5 (*e.g.* strengthen the final paragraph of section 5.5 a little) or 6 noting this fact would be useful.

We added the following close to the end of Sect. 5.3.:

"Due to the differences in the OMI derived LER and the MODIS surface reflectance, it may be very tempted to select primarily both the OMI τ and ALH variables to avoid inconsistencies when correcting of aerosol effects. However, in this study, such a choice is not necessarily obvious for everyone as the accuracy of the ALH retrieval is strongly dependent on the requested prior τ (Chimot *et al.*, 2017). The most accurate OMI ALH retrievals were obtained with collocated MODIS τ , not with the derived OMI AOT. This would naturally suggest first that the combination of OMI ALH ? MODIS τ shall give the most accurate tropospheric NO_2 AMF. However, the apparent inconsistencies due to the different algorithms employed for each physical product are mostly observed in the present study through the discussion on the TOA radiance closure budget issue. They do not necessarily mean that the aerosol correction is less accurate.

The answer to such a problem is, in our opinion, not clear at this stage. But, given the fact that several studies prioritize the 5 application of multiple parameters from very diverse sources (models, ancillary instruments with different techniques, etc..) to satellite spectral measurements, we think that the issue of radiance closure budget should be kept in mind by the scientific community and further investigated in future research studies. At the end, an optimal trade off must be found between quality of Nv product and the weights given to the original satellite measurement."

The discussion about the TOA radiance closure issue is already briefly reminded in the Sect. 6 (cf. conclusion). Top keep it reasonably concise, we don't think it is very relevant to insist too much about it again.

What I am suggesting is essentially taking some of the rationale you provided in the Response to Reviewers, and inserting similar text into the manuscript itself, as it is likely that most readers of the final paper will not go back and read through the original peer review.

Please let me know if you have any questions about the above, and I look forward to seeing the next and probably final version.

Best wishes,

Andrew