Interactive discussion on AMTD-2017-286 "Spatial distribution analysis of the OMI aerosol layer height: a pixel-by-pixel comparison to CALIOP observations"

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We thank Referee #1 for his / her valuable comments. They give us the opportunity to solidify our messages and manuscript. Below we address them one by one (Referee #1 comments in blue, author and co-authors in black).

The paper discusses the application and evaluation of a novel technique to retrieve a measure of aerosol layer height based on a Neural Network based technique. Overall, the paper is well written and should be published after the authors address my, mostly-minor, comments below.
My main comment has to do with the evaluation of retrieved dust layer height and the uncertainty analysis.

We took into account all the comments and questions asked by Referee #1 below. We reformulated where necessary according to the remarks and question, in order to ensure a better clarification. More details on these reformulations are given below where appropriate.

I do not understand why they have chosen events during periods (post row-anomaly) when the evaluation using CALIOP data is impossible. The authors should re-do their analysis of the OMI NN dust- retrieval method making use of observations between the launch of CALIOP (mid-2006) and the beginning of the OMI row anomaly problem by the end of 2008.

Following the referee suggestion, we selected another Saharan dust case study occurring prior to the OMI row anomaly development. This time, we mostly focused on diverse cases occurring in July 2007 and selected the day 2007.07.19, one of the days with a low cloud coverage and the large size of dust plume over the Atlantic ocean. Overall, the conclusions remain unchanged apart of the maximum distance between OMI pixels and the CALIOP track which is reduced from 300 km to 100 km.
The average difference (OMI-CALIOP) ALH is -350 m. However, this difference shows quite some large variabilities with a standard deviation of 2.1 km and a low correlation (smaller than 0.4).
We would like to stress that dust particles which are coarse (more than absorbing particles released by fires or nitrate and sulfate resulting from urban and industrialized pollution episodes) and irregularly shaped (thus non-spherical as assumed by the Henyey-Greenstein phase function) are not included in the dataset used for training the NN algorithms. This can lead to significant biases with respect to the modelled scattering phase function. In addition, the use of a prior AOD from MODIS sensor based on a different aerosol model, potentially also inaccurate for pure dust, may lead to some inconsistencies. The resulting impacts may be larger on this case than on wildfires or urban pollution events where released particles generally have a more spherical shape. We propose then to replace the Fig. 9 of our discussion manuscript by the Fig.9 below in our final manuscript.

Please read more below regarding our answer to question of the referee about this specific topic.

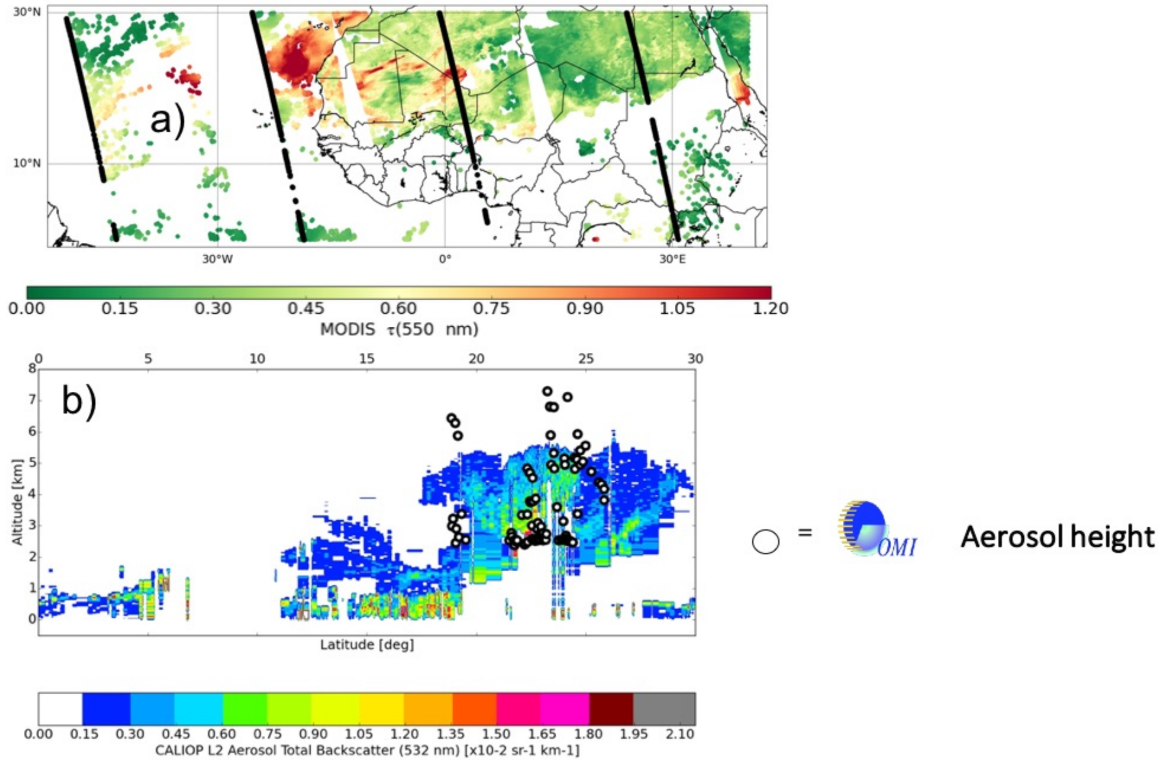

Figure 9: Elevated layer due to a Saharan dust outbreak transported to Western Mediterranean region over sea on 2007.07.19: (a) Map of MODIS Aqua τ (550nm) from the combined DT DB Collection 6 (cf. Sect. 2.3), (b) Retrieved OMI ALH compared with vertical profile of aerosol total backscatter coefficient (532 nm) from the CALIOP L2 aerosol total backscatter (532 nm) associated with the 1st left CALIPSO track over sea in Fig. 9a.

The authors have discussed an uncertainty analysis associated with the assumed values of physical parameters used in the generation of NN training data sets. No error analysis, however, is performed regarding AOD. There seems to be an implicit assumption that the MODIS provided AOD is error-free. There are two ways the uncertainty of the MODIS AOD will propagate to the OMI ALH retrieval:
1) Uncertainty of the MODIS algorithm associated with assumptions on surface albedo, SSA and particle size and shape. Over the oceans, MODIS uncertainties in surface albedo and angstrom exponent are generally very low. The effect of particle shape, however, is non-negligible. Assumed SSA albedo is another source of error. MODIS retrievals over land are subject to uncertainties to all above listed parameters.

As discussed in our manuscript (*cf.* Sect 2.2), an accurate AOD information is required to retrieved ALH from OMI as both AOD and ALH simultaneously drive the O_2-O_2 slant column density (SCD) N_sO_2-O_2. Therefore, retrieving ALH without information on AOD would lead to an ambiguity. The way how an uncertainty on prior AOD impacts the OMI ALH retrieval was already extensively analyzed in Chimot *et al.* (2017). We judged that it was not relevant to repeat this sensitivity analysis in the present manuscript. However, from this study, we mentioned at the end of Sect. 2.2 an accuracy of 0.2 is necessary on the prior τ(550 nm) parameter. We have well in mind that MODIS AOD is not error free.

2) Even if all internal MODIS algorithm uncertainties are well characterized and propagated to the NN ALH retrieval, there is still the issue of the spectral consistency of the assumed aerosol models. Is the NN algorithm designed to assume the same aerosol type as identified by the MODIS algorithm? There may be cases, particularly over land, when the algorithms select different aerosol types. The use of MODIS- AOD for a particular model may not be reasonable for the OMI NN assumed model. It would therefore be desirable to have a consistent retrieval algorithm providing both the AOD and ALH.
Please elaborate on the above-stated points.

We fully agree here with the remark of the referee. The mismatch between the model used for the prior AOD estimation and the one employed for the ALH retrieval may lead to some inconsistencies. However, such an issue is highly challenging and very difficult to properly characterize and estimate.

Our aerosol NN algorithms were designed by using aerosol models regardless of the information sources of all the input variables, *e.g.* AOD or surface albedo. Our main aim was to explore the potential of combining the OMI visible O_2-O_2 absorption band and a machine learning approach to retrieve an ALH parameter. Theoretically, one can run these algorithms with an AOD product derived from any sensor or model. However, our previous study showed how crucial is this AOD quality to mitigate the ALH retrieval bias.
We acknowledge that any mismatch between the MODIS AOD aerosol and the OMI ALH aerosol models can lead to some inconsistencies, and, at the end, not only to some uncertainties but also potential strange patterns in our retrieval. This could even be worse if both models are very far from the true aerosol mixture properties present in the scene and/or differ very much regarding the modelling of the scattering phase function. In addition, one should not exclude other auxiliary datasets that are used for both MODIS AOD and OMI ALH retrievals and could increase these consistencies: *e.g.* surface albedo or directional reflectance.

One cannot exclude that an inconsistency problem may actually occur in the analyzed Saharan dust case. However, since pure dust particles are not included in the training dataset of these NN algorithms, it is difficult to identify such a feature. Nevertheless, we do not see such problems in our other cases and related analyses. Therefore, we think that potential inconsistencies are, at this stage, not critical. But we agree that future developments or improvements or our NN OMI aerosol algorithms should take into consideration the consistency between the aerosol model in the training dataset and the assumed prior parameters.

Other comments:
Page 3, line 4 suppress 'ideally'
Done

Page 3, line 18 add 'channel' after O_2-O_2
Added

Page 3, line 19 replace 'the present' with 'the current'
Done

Page 3, line 20-21, last sentence is paragraph is confusing and actually unnecessary. Remove it.
Done

Page 5, line 24, provide a reference (or elaborate on performed sensitivity analyses by the authors) to substantiate the statement that the AMF does not depend on the aerosol scattering phase function.
We do not have specific references that directly show the low sensitivity of the tropospheric NO2 AMF to the scattering phase function. However, we do have several references (Leitao *et al.*, 2011; Castellanos *et al.*, 2015; Chimot *et al.*, 2016) that showed the low sensitivity to aerosol single scattering albedo, Angstrom coefficient, and asymmetry parameter. All these studies and our experience in radiative transfer modelling with the involved KNMI experts do suggest then this statement.
We added these references on Page 5, Line 24.

Page 8, line 12, spectral characterization applies to the radiation not to the particles. Please rephrase.
The word "absorbing" was missing in the former statement. We reformulated as follows:
"Therefore, we use the NN algorithm trained with $\omega_0 = 0.95$ assuming low abundance of UV and visible absorbing particles."

Page 8, line 26, use the 1064 nm measurement instead of the 532 one. It has been shown by several publications that the CALIOP's 532 attenuated backscatter signal attenuates very rapidly in the presence of smoke layers and, therefore, does not capture the full vertical extent of the layer.
Page 9, lines 28-30 and Page 10, lines 1 to 8. This is not a new finding. Problems with the CALIOP 532 nm measurement have been demonstrated by the quoted literature. The authors should just work with the 1064 channel that works well for all aerosol types.

Indeed, we found publications mentioning the specific problem of CALIOP'532 nm signal and we cite them in our manuscript accordingly. However, our experience on these cases with the OMI ALH retrieval and its comparison with CALIOP L2 and L1 seems to teach us that this problem is mostly known by the research community working with active satellite sensor, and somehow less by the scientists involved with passive sensors, or at least by the persons in trace gas studies.
Furthermore, we would like to draw the attention of the referee to the importance of distinguishing the CALIOP 1064 attenuated backscatter signal (i.e. level 1 – L1 - product) and aerosol backscatter signal (i.e. level 2 – L2). Although the CALIOP 1064 L1 signal probes the full vertical extent of the smoke layer (see our Figs. 7d and 8), the CALIOP 1064 L2 product is actually hampered and limited to the top layer similarly to the CALIOP 532 channel (see Fig. 7b). This is due to the fact that aerosol retrieval in both channels requires first the aerosol characterization which is based on the 532 nm channel (cf. Sect. 2.3). Since it is attenuated in the lower layers, the aerosol extinction retrieval from the CALIOP 1064 nm channel is not applied in these specific layers. Therefore, the CALIOP 1064 nm L2 does not contain the full aerosol vertical profile and cannot either be used for our comparison study. This specific problem was also confirmed *via* a personal communication with Dr. Marc Vaughan who has been strongly involved in the Level 2 CALIOP algorithms.

We already mentioned this point, at a high level, in Sect 3.3., 2nd paragraph.

Page 10, lines 19-27. The poor performance of CALIOP's 532 nm channel is mostly instrumental (i.e., low laser power). As shown by Kacenelenbogen et al. [2011], the HSRL 532 nm channel works equally well for all aerosol types.
Yes, we agree that the 1st (and likely main) problem comes from the low CALIOP Signal-to-Noise Ratio (SNR) and thus prevents to probe tenuous absorbing aerosol plumes. The resulting signal may be likely driven below CALIOP's detection threshold indicating then an instrumental feature. However, we are not completely sure that only this element explains the difference between OMI (a passive sensor with some noise as well) and the CALIOP active instrument. The question of using then a more powerful laser source to circumvent this problem may remain then open. Hopefully, we will have an answer with the future LIDAR space-borne sensors (*e.g.* EarthCare).
Also, we would like to invite the reader to keep in mind that the physical information from a passive spectral measurement contains some differences: *e.g.* the importance of multiple *vs.* single scattering effects included in both measurements.

Page 10, line 28. CALIOP data should not be used to evaluate the NN OMI ALH product because of the loss of OMI-CALIOP collocation after Dec 2008 due to the onset of the row anomaly. There are however, 30 months of data (July 2006 to Dec 2008) that offer hundreds of dust events when full OMI-CALIOP collocation is possible. The authors should replace the currently used post-2008 case studies, with pre-2009 events. Dust activity is seasonal. Therefore, it is not difficult to find 'good' dust cases in the pre-row anomaly period of OMI observations.
This is acknowledged. Please see our discussions earlier in this document.