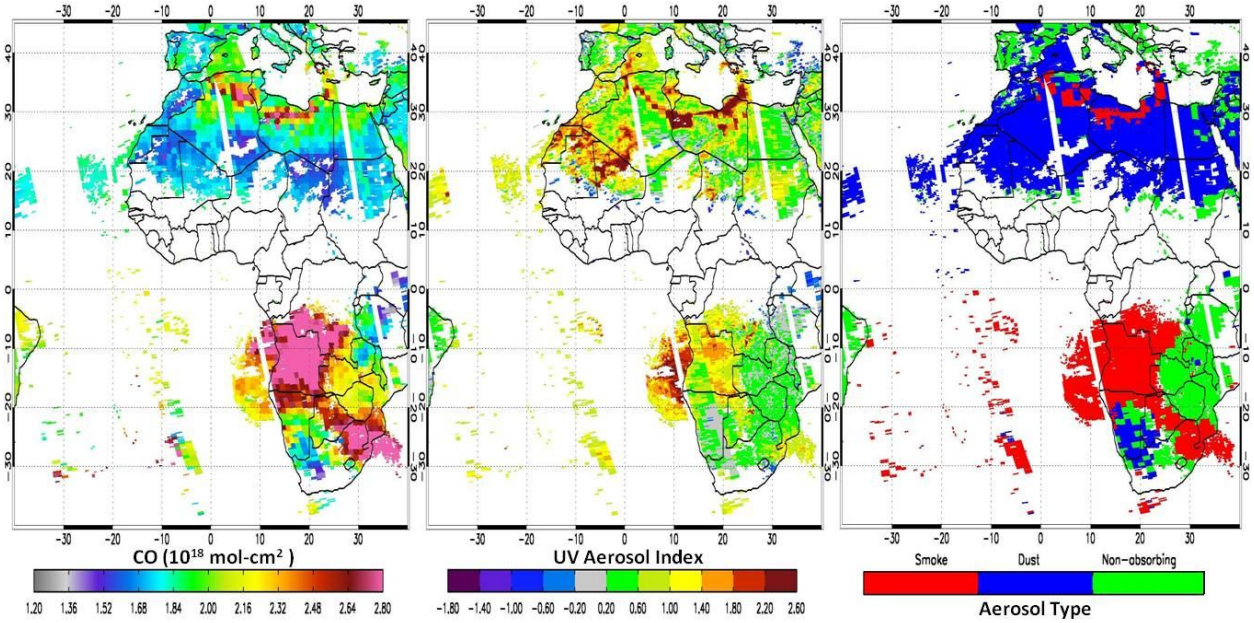
Reply to Anonymous Referee 1:

The reviewer's comments are reproduced below followed by our reply in italics

The manuscript describes the most recent improvements to the OMAERUV aerosol retrieval algorithm from OMI data. The retrieval now includes a priori information on collocated CO concentrations (from the AIRS instrument) and on aerosol layer height (from a custom-made CALIOP climatology). A comparison with sun-photometer measurements at five AERONET sites shows that the performance of OMAERUV has become better in result.

This manuscript presents a novel and interesting approach, but the methods applied are not always well described and in some cases are not substantiated by evidence. The case presented in Fig.3, for example, does not in itself provide enough evidence for the presence of carbonaceous aerosols (as the authors say); more independent data is needed to show that what is seen is not merely a CO plume over (or within) a layer of desert dust.

The original figure 3 showing only the resulting aerosol type have been replaced with a three-panel figure (included below) that, in addition to the aerosol type, shows both the CO and AI fields used in the aerosol type identification scheme. The added figures and expanded discussion of three prominent aerosol features support the validity of our assertion. The AI map shows two clear aerosol layers in Northern Africa over an otherwise low background aerosol conditions (AI less than 0.6). The northernmost aerosol feature is clearly identified by the proposed technique as a smoke layer (high CO content and high AI).



The main core of the CO plume and the AI plume are spatially correlated. Both plumes clearly indicate the path of the smoke layer from its source in Greece, southward transport across the Mediterranean, the arch-shaped pattern over Northern Africa, and its exiting to the

Western Mediterranean. The second aerosol feature over the Western Saharan desert is identified as a dust layer owing to its high AI value and low CO.

Similarly, an interesting ”saturation effect” that occurs with CALIOP 532 nm backscatter data of a thick biomass burning plume is described in the manuscript, but there is no reference to previous work on this matter, and only a single example profile is shown where the effect occurs.

There is no reference to previous work on the matter because there is no previous work on the matter. As CALIOP data users, we have stumbled onto an issue that has not been discussed in the open literature. We have brought up this problem to the attention of members of the CALIOP team who have acknowledged the existence of the issue identified by our group. Their opinion is that the observed attenuation of the 532 nm channel is associated with aerosol absorption that reduces significantly the number of photons scattered back to the sensor. This is mentioned in the revisited version of the manuscript supported by a personal communication reference to Dr. Ali Omar of the CALIOP team with whom we had a detailed discussion on the observed 532-1064 discrepancies.

The sample aerosol vertical distributions shown on figure 4 do not correspond to single profiles as interpreted by the referee. As a matter of fact, the profile shown over Amazonia is an average of about 6000 individual profiles whereas the one shown over the Saharan includes observations from nearly 1800 profiles. We have extended the discussion making the point that this is not an isolated issue but a persistent characteristic of CALIOP's observations.

Another important point is the missing comparison of the obtained CALIOP aerosol height climatology with similar, published work by Winker and co-workers (ACP 2013). The climatology presented in this manuscript is markedly different from that shown in Winker’s paper and this merits attention (despite the fact that Winker’s climatology contains all aerosol types, in contrast to Torres’ climatology, one would expect a great degree of agreement — particularly because the same CALIOP data is used in both cases).

Although the major features of the global absorbing aerosol load shown in Figure 6 (e.g., Saharan dust layer between 2-4 km in Summer and much lower in Spring and Fall; elevated Fall smoke layer over the South Atlantic) can also be identified in Fig 9 of Winker et al [2013], a direct comparison between the two products is not meaningful as they are essentially a representation of different aspects of the aerosol vertical distribution. The H_{63} parameter on Winker et al [2013] represents the height at which about two thirds of the ***total*** *aerosol load lies below, whereas our calculated Z_{clp} is a measure of the level of peak* ***absorbing*** *aerosol concentration. The H_{63} value is calculated including all aerosol types whereas Z_{clp} was specifically designed to capture the height of absorbing aerosol layers. Other important differences include time of observation (night for H_{63} and day for Z_{clp}) as well as different wavelength which, as discussed in this work, may produce different results height results in the presence of carbonaceous aerosols. The Winker et al [2013] reference has been added and a*

brief discussion of the similarities and differences of the two products is included in the manuscript.

I recommend this manuscript for publication in AMT, but only after a revision addressing the points mentioned above and the minor comments listed below and in the annotated manuscript (see supplementary material).

Minor comments
p.5625, l.20 — How is A388 calculated? Is it the clear-sky reflectance corresponding to the albedo at 388 nm from the database?

Yes. It is derived based on the 15 year-long TOMS record of minimum reflectivity [Herman and Celarier,1998]. This clarification has been added to the revisited version of the paper.

p.5626, ll.2-3 — The so-called COI is not a dimensionless quantity! It is simply the CO column multiplied by a factor to make the number easier to handle. Please do not call it "normalized" (p.5625, l.26) unless you normalize it, e.g. using a reference or a background value.

The COI definition has been reworded.

p. 5626, l.11 — On what are these thresholds based?

Adopted threshold values of COI_0 correspond to the average of AIRS CO climatological annual minima over major biomass burning /boreal fire activity regions. Such values are 2.2 in the northern hemisphere (based on Yurganov et al., 2008) and 1.8 for the southern hemisphere (based on Yurganov et al., 2010). These values are intended to exclude the background (non-biomass burning related) CO levels.

p. 5626, l.21ff — Why is there a difference between the retrieval methods over land and ocean?

Because of the difficulty associated with the separation of ocean color effects from those of low aerosol concentrations, retrievals over the oceans are performed only when absorbing aerosols are present (either DD or CB types) in sufficient amounts as indicated by AI values larger than (or equal to) 0.8. Retrievals over land, on the other hand, are carried out under all conditions of aerosol type load regardless AI threshold considerations. The above clarification has also been added to the manuscript.

p. 5627, ll.6-7 — Why is CALIOP input not used for SF aerosols?

The sensitivity of the retrieval algorithm to the height of ***absorbing aerosol layers*** *is the reason a climatology is necessary. The sensitivity to the vertical distribution of SF aerosols is negligible, so a typical exponential representation works well.*

p. 5627, ll.12-15 — Why is the aerosol layer height assumed to be so much higher at high latitudes than at the equator? Is this because there are less, but more intense (forest) fires at high

latitudes and more, lower-intensity (agricultural, household) fires at lower latitudes? Do the numbers come from a climate model?

Boreal fires are generally associated with strong convection that carries smoke to high altitudes often reaching the UT/LS. The OMAERUV algorithmic assumption of higher (than in the tropics) aerosol layer at mid-high latitudes is based on lidar observations, also supported be MISR stereo-viewing retrievals.

p. 5628, ll.18-19 — But what about other sources of CO? And the high and seasonally variable background value?

We are not sure what the referee is actually asking here. High CO values from sources other than biomass burning and wild fires will not be correlated with AI. CO from other sources are largely excluded by the selected CO thresholds. The seasonal variability associated with non-biomass sources is much smaller than the large departures observed during biomass combustion events. We found that a single threshold value works well for the full year.

p. 5629, l.5 — What does the sensitivity profile (averaging kernel) of AIRS look like? E.g., how sensitive is it to the lower troposphere?

Highest sensitivity is reported at about 500 mb (~ 5.5 km). The sensitivity is enhanced for high CO levels associated with biomass burning [McMillan et al., 2005]. In general, AIRS CO averaging kernel in the presence of smoke plumes allows detection between about 800 mb to 500 mb (~2.0 to 5.5 km).

p. 5629, l.8 — CO retrievals may be performed for pixels with up to 80% cloud cover, but OMAERUV performs retrievals only for clear sky! How do you account for this difference?

OMAERUV does exclude cloudy pixels. Thus, by definition, only cloud free scenes (as determined by OMI) are considered. Because of the time difference between the observations, there may be instances of cloud presence in the AIRS pixel, but since CO is only used qualitatively we do not see why that would be a problem.

p. 5630, ll.12-17 and Fig.3 — This is not a good example to plead your case. It is obvious from Fig. 3 that there are a lot of absorbing aerosols around and that there is a plume of CO as well. But the co-existence of enhanced CO values with high AI in this case does not tell us if there are smoke aerosols present - this may just be a plume of CO over (in?) a layer of desert dust. Evidence for carbonaceous aerosols on this day can only come from measurements on previous and later days showing an isolated plume (not surrounded by desert dust) of smoke aerosols.

We disagree. The stringent conditions suggested by the referee will never allow a validation since this is a region where dust aerosols are almost always present. That is precisely why a separation method is needed. We believe that the addition to Figure 3 of the CO and AI field maps and the extended accompanying discussion adequately supports our conclusion.

p. 5631, Sect. 4 — CALIOP is actually pretty good at discriminating between dust and other aerosols, so if you're using profile information from CALIOP, why not use their classification, too? If only to separate dust from other types.

For the same reason we can't use directly the aerosol height. CALIOP is not a global mapper.

p. 5632, l.22 — The cited paper does not contain a description of a cloud-screening procedure.

The citation has been removed. The cloud screening procedure is actually described in section 4.2 of this manuscript.

p. 5632, ll.23-26 — If you re-grid CALIOP data to match with OMI pixels anyway, why not just use the OMAERUV cloud screening procedure on both OMI and re-gridded CALIOP data? That seems to be most consistent.

We disagree. The separate analysis of cloud presence in CALIOP observations was necessary to remove clouds affecting the lidar profiles at specific layers. Those cloud features cannot even be detected by OMI.

p. 5633, ll.1-9 — This, if found to be generally true, is a very important finding for all users of CALIOP data! But is there more evidence, either in literature or from your own studies, for this effect? How often does it occur, in other words, is it a significant, general problem? What are possible reasons for this phenomenon?

Yes, it is a significant, general problem. See answer to second general comment above. A detailed analysis on the frequency of occurrence is beyond the scope of this paper. We are just reporting on an interesting issue we have identified as CALIOP data users.

p. 5635ff, Sect. 4.4 — This is a similar approach to that taken by Winker and coworkers in compiling a 3D aerosol climatology from CALIOP (ACP 13, 3345-3361, 2013). It would be good to mention the paper and explain why your results look so different from those shown in Fig.9 in the cited paper.

This comment has been addressed in answer to third general comment in this reply.

p. 5637, Sect. 5 — Please shortly describe the AERONET network and add the appropriate reference (Holben et al., 1998). Mention the collocation criteria, even if they are given in the references mentioned in ll. 15-16.

Done.

p.5638, ll.18-22: These are very nice results! Please elaborate: what are the aerosols in the North? I would have guessed that they are desert dust, but that cannot be the case if they have SSA = 1.

Those aerosols are possibly weakly absorbing boundary layer aerosols. A brief discussion of the feature has been added.

p. 5639, l.2 — The ”near-simultaneity” of the A-Train measurements is not really a fair argument in this case, as you use a monthly CALIOP climatology.

Although the original statement is strictly correct (it refers to sensors on two, not three, satellites, meaning Aqua and Aura) it has been slightly qualified for clarity.

pp.5646-5652 — Please check the resolution of the figures; they don’t appear so nice on my pdf viewer (Acrobat). And make all table and figure captions more descriptive, as it is they are not understandable without the accompanying text.

Figures captions have been revisited for clarity.

References

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