Reply to Anonymous Referee 2:

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

The paper describes important improvements to the algorithm for the retrieval of aerosol properties in the UV. Generally, the manuscript is in good order, describes the used procedures and shows the results of the improvement. The main criticism I would have is the suitability of CO measurements to distinguish biomass burning from other aerosol types.

We do not argue that CO is suitable to distinguish biomass burning from all ***other aerosol types****. The proposed CO/AI scheme is intended to specifically distinguish biomass burning aerosols from* ***desert dust aerosols****.*

Certainly, the use of these measurements in the algorithm is an improvement compared to no distinction or a climatology, but the claim that it can be used in this way is not substantiated by references or an own study, nor validated and quantified.

We agree that the initial version of the paper lacked a literature review on the issue of the high spatial and temporal correlation of carbonaceous aerosols and CO column amounts. Such a correlation has been identified, described and documented by several studies over the last twenty years. We have added to the revised version of the paper. a brief account of previous work on this issue. Biomass burning is the most important source of carbonaceous aerosols, and also a significant source of CO (the second most abundant trace gas produced by biomass burning). The existence of a natural correlation between the concentration amounts of these two combustion by-products should therefore be expected, as has been documented based on airborne [Andreae et al, 1994], and satellite observations [Edwards et al., 2004; Edwards et al., 2006; Lou et al, 2010; Witte et al., 2011]. A multi-year analysis of MODIS aerosol optical depth and MOPPIT CO column amounts in both hemispheres [Edwards et al., 2004; Edwards et al., 2006] documented high correlation between these two parameters over regions and times of the year when carbonaceous aerosols biomass burning and wild fires was the predominant aerosol type. No correlation between AOD and CO concentrations was observed for other aerosol types. A correlative analysis of Air Quality related measurements during the 2010 Moscow also shows MODIS AOD and OMI Aerosol Index to be highly correlated with AIRS CO column amounts [Witte et al, 2011]. Luo et al. [2010], also found a clear spatial correlation between Tropospheric Emission Spectrometer (TES) CO measurements and the OMI Aerosol Index signal of the smoke plume generated by the 2006 Australian fires [Torres et al., 2007, Dirksen et al., 2009]. These previous analyses support the suitability of CO as an adequate tracer of carbonaceous aerosols.

The pathways of CO and biomass burning from any source will differ as time progresses, leading to uncertainties in the dust-smoke differentiation. This should at least be mentioned in the manuscript. Furthermore, no comments are made about the possibility of dust and smoke that can be mixed, a common situation in the Sahel and over North China, among others. In these case it is not clear from the manuscript what would happen. If CO would be present in the same air mass it is likely to be labeled as biomass burning aerosols.

This should be investigated. I am aware that these are difficult cases, but these cases are the main reason why aerosol retrievals are still highly uncertain. This should also at least be mentioned in the paper and investigated.

The important issue brought up by the referee has been partially discussed (page 5630, initial manuscript). We have extended the discussion to include the issue of dust/smoke mixtures and elevated CO that, as pointed out by the referee, will lead to the selection of smoke type.

I recommend the manuscript for publication when these above issues are resolved. More detailed recommendations and minor revision are provided below.

Abstract

Indeed, in the abstract the main difficulty becomes clear: it states that the "AIRS CO measurements are used as a reliable traces if carbonaceous aerosols". This reliability should be estimated and substantiated.

We stand by our assertion that CO is a suitable way of distinguishing between carbonaceous and desert dust aerosols. The suitability of CO as an adequate carbonaceous aerosol tracer has been historically demonstrated over the last 20 years. The reviewer's valid criticism on this issue prompted us to expand our admittedly short and incomplete literature review on the subject. This issue is addressed at length in the reply to the second general comment above. In the abstract we have replaced 'reliable' with 'adequate'.

Section 2.2

As an AMT manuscript, one should expect a thorough description of the methodology. This is done throughout most of the paper, but section 2.2 relies too much on Figure 1. A better description of the algorithm is needed here in the text, explaining the reasons behind the thresholds that are used.

Section 2.2 has been revisited. A more clear algorithm description is presented.

E.g. In paragraph 5625, 20-24 add the number for Delta R, and explain why it is used. Similarly, paragraph 5626, 4-14 describes the AI0 and COI0 threshold, but no references or explanations for these thresholds are given. Only later in the text (5631, 1-2) the reasons are explained. This should be incorporated or referred to here.

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 and 1.8 for the southern hemisphere, based on Yurganov et al., [2008, 2010)]. The value of AI_0 (0.8 for both land and ocean conditions), is a slightly smaller value that the one used in the interpretation of TOMS AI data [Herman et al., 1997]. The above explanation is included in the revised version of the manuscript.

In paragraph 5626, 15-20 the cloud flagging is described, but lacks an explanation of the threshold that are used. This should be explained better in the text, and substantiated with references.

The cloud flagging discussion has been improved.

In contrast, paragraph 5627, 3-20 is an excellent example of a well written informative algorithm description. I would suggest to do this for all panels in Fig. 1.

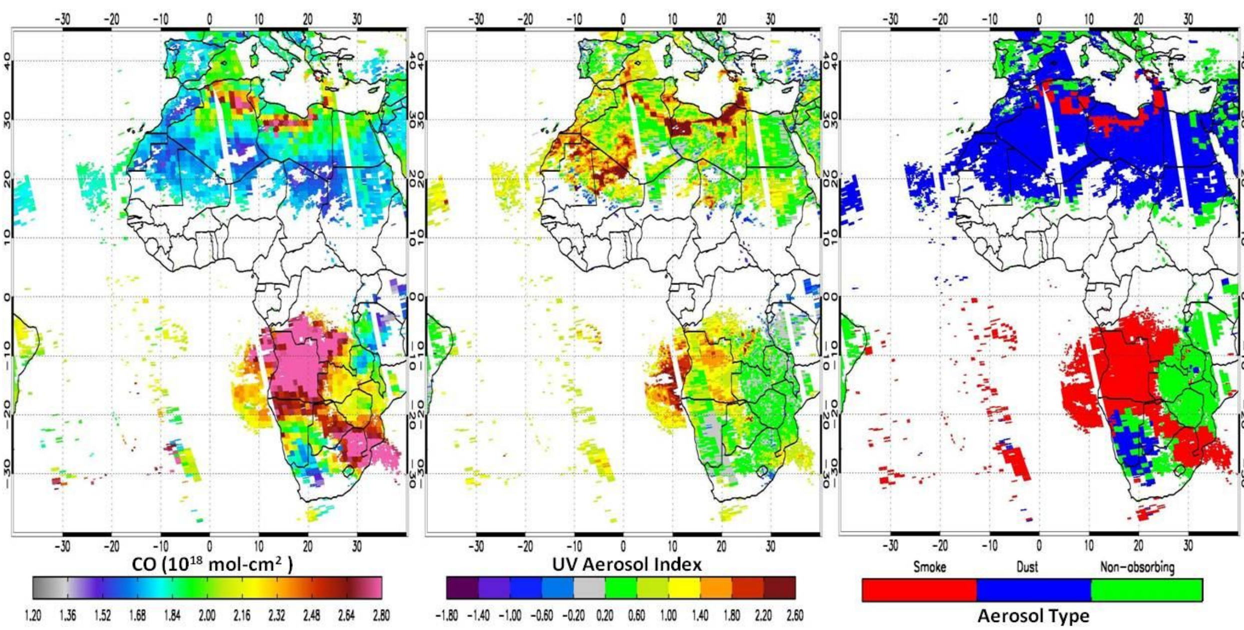
Algorithm description section has been thoroughly revisited.

5627, 22-24. I think the reference to Penning de Vries (2009) is more appropriate here.

The referee's suggestion has been accepted.

5630, 17. "..shows the unmistakable presence of the Greek fires smoke." What it shows is the presence of CO. It also shows the likely presence of the Greek smoke. No validation of any kind, or reference to the reliability of CO as smoke tracer is given, so this statement is premature.

We stand by this statement. 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 spatially correlated CO (its main core) and AI 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. We believe the addition to Figure 3 of the CO and AI field maps and the extended accompanying discussion adequately supports our conclusion.*

Furthermore, in section 3.3 a word or two should be given to the likelihood of dust smoke mixtures, which are common in at least the Sahel and northern China.

A reply to this objection has been given in the general comment section.

5630, 21. After " undetected by the AI." add ", which is sensitive to elevated aerosols."
5634, 17. Change "is shown on" to "in"
5635, 18. was -> were

Done.

Section 4.4 I would suggest to replace the terms Spring, Summer and Autumn (Zclp) to April, July and October (Zclp). Although the images in Fig. 6 are shown as representative for the season, it doesn't actually show the seasonal Zclp.

Suggestion accepted.

5636, 28. Please, indicate clearly which plume or area is described here (probably the plumes over the Acrtic?)

Clarified.

5637, 2. "3.5-40" -> "3.5-4.0"
5638, 2. Small -> A small
5638, 3. height -> height is used.

Done.

5639, 3-5. I disagree with this statement. It has been shown in this paper that the algorithm is improved using the CO measurements (statistically compared with AERONET data), but the reliability of the CO measurements as a tracer for smoke is not shown in this paper. Nor are any references given for this.

We understand the referee's initial skepticism. We believe, however, that the expansion of the literature review on the issue of CO suitability as carbonaceous aerosol tracer, and the more detailed description of the selected case study, provide the adequate context to better appreciate *the expected benefit of using CO as a useful piece of information to improve OMAERUV's performance.*

5639, 5 "Because CO is an adequate tracer of carbonaceous aerosols.." Please, substantiate this.

Already addressed in replying to related comments above.

5639, 21. I think "when" -> "after which" is more appropriate here?

Reworded.

References

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