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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Satellite-based evidence of wavelength-dependent aerosol absorption in biomass burning smoke inferred from ozone monitoring instrument" *by* H. Jethva and O. Torres

Anonymous Referee #2

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The paper deals with a necessary update of aerosol models used in the remote sensing retrieval of (mainly) AOD. The authors convincingly show that the OMI aerosol retrieval algorithm is in much better agreement with ground-truth AERONET measurements in the Amazon region during the biomass burning season when the spectral dependence of aerosol absorption is taken into account. This is attributed to the strong spectral dependence of absorption by "brown" carbon aerosols. Not only is this an important finding that should be included in aerosol retrieval by other remote sensing (satellite) instruments, it is also the first satellite-based evidence for the presence of brown carbon in smoke plumes.

Although the findings are relevant and the presented results scientifically sound, the paper is badly written. Aside from grammatical errors (which should be minimized by, e.g., proof-reading by a native speaker), the paper needs to be restructured to improve readability and clarity. The sections most in need of restructuring are:

- P. 7293, lines 3-24

- P. 7295, lines 10-26

- P. 7297, lines 9-22

- P. 7300-7302, whole section 4

- P. 7303, lines 10-18 and lines 19-28

- P. 7306, lines 2-5

Other comments:

P. 7292

l. 19-21: "present study (...) integrated measurements." This statement implies that OMI coupled with AERONET can determine aerosol type (i.e., black (BC) or brown carbon (BrC)) on a single-measurement basis. In the presented results, the improvement in the OMI retrieval when using the BrC absorption w.r.t the use of the BC absorption becomes apparent thanks to statistics (more OMI AOD fall within the uncertainty range in Fig. 4 than in Fig. 2). In short, I think the statement is too optimistic: I would not say that OMI (+AERONET) can distinguish BrC from BC based on the results presented here. OMI (+AERONET) does, however, provide evidence that BrC is present in biomass burning smoke. This is aside from the fact that smoke particles are probably complex internal and/or external mixtures of BC, BrC, secondary aerosols, and numerous other compounds. A statement mentioning the complexity of (smoke) aerosol composition should be added to the paper.

P. 7293

ACPD

11, C2445–C2451, 2011

l. 7: "GCM" Change this to "Global Circulation Model"

P.7294

l. 5: Is (Torres et al., 2007) the only relevant reference here?

l. 8: "SA" Change to "South America"

P. 7295

l. 2-4: "Secondly, (...) cloudy conditions." This is an intrigueing statement that requires more explanation and references to literature.

l. 6. Please cite the spatial resolution of OMI here (numbers).

l. 6. "SCIamachy" Should be written "SCIAMACHY"

l. 7. OMI is a satellite instrument; it does not "derive". Please rephrase the sentence.

l. 11. Please add a reference to the OMI or TOMS AI algorithm here

P. 7296

l. 13-14: "the OMAERUV-AERONET comparison was reasonable." What is reasonable? Please be more quantitative.

l. 24: "Cimel sunphotometers that measures" Change to "Cimel sun-photometers that measure"

l. 26: "over worldwide locations" Change to "at many locations worldwide"

P. 7297

l. 16: "in a given pass" Change to "in a given satellite overpass"

l. 23-26: Mention here that AERONET AOD was interpolated to 388 nm

l. 23-26: Some more details on the OMI quality flags would be very useful here or earlier on in the text.

ACPD

11, C2445–C2451, 2011

P. 7298

l. 20: Why do you not use the OMI surface albedo database compiled by Kleipool et al.?

l. 21-23: "The surface albedo in the UV (...) the error in the aerosol retrieval produced by the uncertainty associated with surface albedo is expected to be very low." Are there sensitivity studies that prove this? A small error in a small number (i.e. UV surface albedo) may lead to large errors in retrieved products? Might the reason also be the strong contribution from Rayleigh scattering in this wavelength range, which makes the contribution of radiation reflected by the surface less important?

P. 7299

l. 8-9: "This suggests (...) is likely to be correct" No, it does not (at least not in general, but only for this case). But it does say that the error is not (only) caused by layer height uncertainty. Does a validation study of OMI aerosol height exist?

l. 11-14: "The use (...) near-UV radiation." Remove this sentence. It appears to be incorrect and is not of importance.

l. 16: Please explain the AAE in more detail, as it is an important parameter for this study. Also add a reference to: "Spectral absorption properties of atmospheric aerosols R. W. Bergstrom, P. Pilewskie, P. B. Russell, J. Redemann, T. C. Bond, P. K. Quinn, and B. Sierau Atmos. Chem. Phys., 7, 5937-5943, 2007"

l. 22: "wavelength-dependence of absorption" Change to "wavelength dependence of absorption optical depth"

P. 7300

l. 3: "RSD" What does this abbreviation mean? Please do not use too many different terms for the spectral dependence of absorption, as it confuses the reader. Use only AAE, or, if you must use another quantity, mention the AAE for that particular case.

ACPD

11, C2445–C2451, 2011

l. 12: Add a reference to (M. de Graaf, P. Stammes, O. Torres, and R. B. A. Koelemeijer, J. Geophys. Res., 110, D01201, doi:10.1029/2004JD005178, 2005) here.

P. 7301

l. 26-29 (and on next page): What kind of samples were collected here? How were they measured, e.g., at what relative humidity? What is the relevance of those results to the OMI retrieval?

P. 7302

l. 1-10: Also compare the results discussed in (Bergstrom, 2007) (full reference can be found above)

l. 8 / l. 19: "The AAE in the range 440-670 nm was ∼1.5" But this is in contradiction with the values cited later: "AAE in the range 2.5 ∼ 3.0" Why is 2.5 assumed for the retrieval, and not 1.5?

l. 23-24: "Using a (...) was carried out." Rephrase this sentence, it does not make much sense in the current form.

l. 26: "observations at the AERONET sites used in the study." Change to "observations at four AERONET sites."

P. 7303

l. 1: "remarkably" Change to "very"

l. 2: What do you mean by ∼100% improvement? Improvement in what quantity, and how is this percentage determined?

l. 6: "none" Change to "one" (look closely in Fig. 2!)

l. 11: "non significant" Change to "small"

l. 19-28: Fig. 5 has three panels, not two (although the far right panel, which is not mentioned in the text, is not essential to the paper and can be left out).

ACPD

11, C2445–C2451, 2011

P. 7304

l. 5: "facilitate the comparison" Should be something like "make a fair comparison possible"

l. 14: "carbonaceous aerosol models" Change to "coloured aerosol models", for BC is also carbonaceous.

l. 20: "The African burning season" Change this to something like "The burning season in Central Africa", since there are two major biomass burning seasons in Africa.

l. 23: "Indo-Gangetic Plane" Should be "Indo-Gangetic Plain"

P. 7305

l. 2: "associated with (...) (EAE>1)." Did you infer this from AERONET measurements of SSA?

l. 5: "MODIS-derived fine mode fraction (...) persistence layer of smoke" How does MODIS FMF show this? Smoke particles tend to be small, like pollution particles.

l. 13: "OMI algorithm identifies aerosol type as smoke" How does the OMI algorithm do this? Based on climatology and/or AI value? Do you know how reliable this is?

P.7306

l. 5: "we attempted to evaluate" Change to "we evaluated"

l. 7: "central Africa, and northern India" This statement is too bold: you only investigated the agreement at two AERONET sites in Africa and India, this does not mean that the good agreement is found for the whole region. Can you add some AERONET stations in these regions to your comparison?

l. 13: "fresh" Change to "new"

l. 19: "therefore should be considered as good quality data." You are probably referring to the quality flag here. Do you mean to say that for these cases the flag should be

ACPD

11, C2445–C2451, 2011

changed from 1 to 0?

l. 20-24: "This paper strongly suggests (...) absorption properties." Not only for OMAERUV, but also in other remote sensing aerosol retrievals!

P. 7307

l. 1: "of biomass burning analyzed here." Add: "but this needs to be corroborated with further evidence from other AERONET stations in those regions."

l. 6: "AAE as inferred from OMI" To my understanding, AAE is not inferred (i.e., retrieved), but assumed in the retrieval.

l. 9-17: Although I think the presented results are quite sensational, these conclusions are taking it one step too far. First, because the OMI retrieval needs input from AERONET to distinguish between BC and BrC; and second, because this can hardly be done on a single-measurement basis, as I mentioned in my first comment (to P. 7292, l. 19-21). It would be wonderful if OMI-AERONET measurements could be used for the "identification of aerosol composition", but it seems that we are still very far from achieving that goal.

Fig. 1: There is a typing error in the figure; "Ji Parna" should read "Ji Parana"

Fig. 3: Add the AAE to the figure legend

Fig. 5: The far right panel can be removed, see also my comment to P. 7303, l. 19-28.

Fig. 7: Can you make two panels out of this, one for Kanpur and one for Mongu?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 7291, 2011.