Atmos. Chem. Phys. Discuss., 11, C4855–C4860, 2011
www.atmos-chem-phys-discuss.net/11/C4855/2011/

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

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Received and published: 13 June 2011

Journal: Atmospheric Chemistry & Physics

Manuscript title: Satellite-based Evidence of Wavelength-dependent Aerosol Absorption in Biomass Burning Smoke inferred from Ozone Monitoring Instrument

Manuscript # acp-2011-115

Response to the Lorraine Remer's Comments:

Thank you Lorraine for reviewing our manuscript. Your comments and suggestions have helped us to improve the clarity and quality of our paper. Below are our response

to your comments and concerns. Let us know if you still have something to comment about this work.

Your comments are printed BOLD letters, whereas our response is in normal fonts.

However, the paper, as written is disorganized, difficult to read and full of grammatical mistakes. Without a Word file to make the edits as I read the paper, I resorted to using a red pen on the hard copy that I printed for myself. This hard copy is now bleeding red ink with corrections. I will hand this edited version of the paper to the second author who I should see later in the week.

The grammatical mistakes pointed out in the hardcopy have been corrected in the revised manuscript. The manuscript has gone through some changes in terms of structure, logical flow, and text. Please refer to our revised manuscript.

1. The introduction to the paper is inadequate. Many statements about what is burning in the Amazon and about OC and BC optical characteristics, with no supporting references. Every sentence is screaming for a reference.

The references to Chand et al. (2009), Guyon et al., 2003, and Andrea and Gelencsér, 2006 have been added in the Introduction.

In terms of characterizing the Amazon and its aerosol, what about the extensive publication record of Paulo Artaxo and his students?

The relevant references have been added in the Introduction. See above response.

Pg 7294: "The spatial distribution of AI shows high values over the central part of South America indicating the presence of absorbing aerosols, and relatively lower AI values towards south" Does this have to be differences in absorptive properties or can it be loading?

The larger magnitudes of AI can be a result of both effects, loading as well absorption properties. The dependence of AI on AOD has been well-known and documented in

Torres et al. (1998), whereas the impact of spectral absorption properties on the AI and consequently AOD retrieval has been demonstrated in this paper. Also note that the AI is sensitive to the height of aerosols layer. Pg 7296: A note on current OMAERUV flag scheme

As per the current quality flag scheme, the retrieval with flag 0 ($\Delta R \leq 0.04$ for dust and sulphate; $\Delta R \leq 0.08$ for biomass burning, and UVAI$\geq$1.0; ΔR=TOA reflectivity-surface reflectivity) are considered to be reliable in AOD and AAOD, whereas retrievals with flag 1 (R388 up to 30% and AI >1.2) are expected to be contaminated by minimum clouds and therefore AOD is not reliable. Also, see next response on flagging scheme.

Pg 7296: What is meant by minimum clouds?

The current OMAERUV flagging scheme includes all retrievals with conditions, $\Delta R \leq 0.04$ for dust and sulphate; $\Delta R \leq 0.08$ for biomass burning, and UVAI$\geq$1.0; where ΔR is the difference between the TOA and surface reflectivities, in to the flag 0 category. In this condition, we expect that these retrievals are cloud-free because the LER hardly goes beyond 10% which is related to aerosol scattering. For the flag 1 category, the LER is allowed to go up to 30%, however, with condition AI>=1.2. Due to higher reflectivity in such cases, it is possible that the OMI pixel may be contaminated by the presence of sub-pixel clouds. However, larger magnitude of aerosol index for these measurements is a strong indicator of the presence of absorbing aerosols over these pixels.

In order to remove cloud-related biases in the OMI-AERONET comparative analysis, the Figure 4 has been replaced by a new one in which only those flag 1 retrievals were included for which the measured reflectivity (or LER) was less than 20%. As a result, the new OMI-AERONET comparison has better statistics (less RMS) and shows less number of retrievals outside the predicted uncertainty.

Why the mention of the GCM study and the Zhang reference here? It doesn't fit. The reference to Zhang et al. has been removed from the text.

Koren et al., (2008) cited, not in reference list Koren et al. has been added in the revised text.

But really Section 4 makes a much better introduction. It seems out of place where it appears now. In fact the opening sentence of Section 5, "As demonstrated in Section 3" draws us back to the logical flow of the narrative. There shouldn't be a Section 4 where it is.

Section 4 has been re-organized in the revised text. It has two subsections, first that discusses the results of near-UV sensitivity analysis to the spectral absorption, and second that provides ground-based evidences on the spectral aerosol absorption based on which a most reasonable value AAE was selected for the new OMAERUV retrieval.

2. Refrain from qualitative statements such as "reasonable" or "remarkably good". These should be quantitative statements

The RMS and correlation values reported in Torres et al. (2007) have been added: "Torres et al. (2007) compared the OMI-derived AOD with that of AERONET direct sun measurements and found that under minimum cloud interference the OMAERUV-AERONET comparison was reasonable (root-mean-square error=0.26 and correlation=0.74 in the AOD comparison; 73% SSA retrievals within $\pm$0.05 of AERONET inversions)."

Also, the OMI-AERONET comparison plots shown in this paper for AOD and SSA have statistical comparisons included in them.

3. Lambertian equivalent reflectivity needs to be defined.

"The lambertian equivalent reflectivity is the diffuse reflectivity of a surface placed at the bottom of the atmosphere that explains the radiance (388 nm) measured by OMI at the top of the atmosphere." This definition has been added in the text.

Figure 3 is never referenced in the text. This is an important figure and the text that

describes relationships between AI values and AAE values needs illustration.

This Figure has been referenced in the text in the revised manuscript. Also, the relevant text has been modified.

5. Please be clear that RSD is the spectral dependence of the imaginary part of the refractive index, and is not, for example the spectral dependence of tau_ab. The discussion gets cloudy. Define tau_ab.

The term 'RSD' has been replaced with 'spectral dependence of the imaginary index' for clarity. Also, TAU_ABS (aerosol absorption optical depth) is defined as, AAOD = AOD * (1 – SSA) where AOD is the aerosol optical depth and SSA is the single-scattering albedo.

6. Can it be shown that cloud contamination is not the explanation for over estimation of AOD? It is stated, but I was left unsatisfied.

The attached PDF file show several subsets of MODIS true color RGB images (http://rapidfire.sci.gsfc.nasa.gov/) for aerosol events observed over the study region of Alta Floresta, Cuiaba Miranda, Campo Grande, and Kanpur regions in South America and India respectively. Each image is over laid with a box of size 0.5 deg by 0.5 deg located at the center of the image that represents an approximate region around the AERONET site over which the OMAERUV retrievals were extracted for the comparison against AERONET.

In all cases presented below, the region around AERONET site was visually appeared to be either totally devoid of clouds or minimally contaminated by sub-pixel clouds. Included in these images are the values of AOD (388 nm) measured by AERONET and retrieved by OMI (standard and new) and MODIS (MYD04_L2). Certainly, the sub-pixel could contamination in a given OMI pixel cannot explain the over-estimation seen in the standard OMAERUV product. Furthermore, the cloud fraction estimated by the MODIS aerosol algorithm is nil in most cases which further support above argument.

A description on the Sept 19, 2005 case has been added in the revised manuscript in section 3.2 Causes of AERONET-OMAERUV differences as:

"The sub-pixel cloud contamination within OMI pixels could be another source of uncertainty in the AOD retrieval. In order to verify this, several OMI-AERONET matchups were analyzed in conjunction with corresponding MODIS true-color RGB images (http://rapidfire.sci.gsfc.nasa.gov/). Figure 3 (new one) shows one such case of a smoke event observed over Alta Floresta region on September 19, 2005. This RGB image is a subset of L2 5-min swath (or granule) centered at Alta Floresta AERONET site. A box of size 0.5 deg by 0.5 deg size around the center of image is the region over which the OMAERUV retrievals were extracted and compared with AERONET. In this case and several others (not shown here), the region around AERONET site was visually appeared to be devoid of clouds. Instead, a thick layer of smoke is clearly visible for which the standard OMAERUV algorithm over-estimated AOD (5.71) against AERONET measurements (2.35). The cloud fraction estimated by MODIS aerosol algorithm is nil which further provides evidence of no clouds over the AERONET site. The similar results were noted for other OMI-AERONET collocated cases. Certainly, the sub-pixel could contamination in a given OMI pixel cannot explain the large over-estimation seen in the standard OMAERUV product."

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/11/C4855/2011/acpd-11-C4855-2011-supplement.pdf

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