Atmos. Chem. Phys. Discuss., 11, C2242–C2244, 2011
www.atmos-chem-phys-discuss.net/11/C2242/2011/

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: 24 April 2011

This work starts from a premise of evaluating the OMI OMAERUV aerosol retrieval algorithm against AERONET stations in South America, discovers a significant over-estimation of AOD in a category of retrievals that result in a QA=1, and then "fixes" the problem by running new look up tables that allow for enhanced spectral dependence of the absorption through the UV range. This enhanced spectral dependence results in enhanced absorption at shorter wavelengths (when compared with Black Carbon). The new assumptions of particle properties also makes a measureable improvement to retrievals of Single Scattering Albedo (when compared with AERONET retrievals), and

is also shown to improve AOD retrievals in two other locations besides South America. These results are all sound and important enough to the community to warrant publication of the paper. However, the main importance of the paper is the demonstration that satellite remote sensing is sufficiently sensitive to discern something about particle composition. Nobody before has used satellite retrievals to identify the existence of absorbing organics, and this discovery makes this paper very important.

The science is sound. The conclusions follow from the data, and the results should be published. ACP is an appropriate journal.

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.

In addition, to add to the discussion on the ACPD site, I have indicated some suggested edits here:

1. The introduciton 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.

Why the mention of the GCM study and the Zhang reference here? It doesn't fit.

Koren et al., (2008) cited, not in reference list. Is this the right reference? I assume the authors are referring to the science paper that addressed smoke effects on cloud formation. How is this a reference for "satellite observations of aerosol physical and optical properties"? There are other papers. What about Zhu et al., (2011)? There are others.

Zhu, L., J. V. Martins, and L. A. Remer (2011), Biomass burning aerosol absorption measurements with MODIS using the critical reflectance method, J. Geophys. Res.,

ACPD

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116, D07202, doi:10.1029/2010JD015187.

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

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.

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

3. Lambertian equivalent reflectivity needs to be defined.

4. 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.

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.

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.

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