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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Remote sensing of soot carbon – Part 1: Distinguishing different absorbing aerosol species" *by* G. L. Schuster et al.

Anonymous Referee #1

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Review of Schuster et al. Remote sensing of different absorbing aerosol species APCD

This manuscript uses AERONET direct sun and sky scan retrievals to infer differences in absorbing aerosol species. The emphasis is on distinguishing soot, brown carbon, and hematite. Figure 2 shows that there are distinguishing features. However, it is difficult to assess how quantitative the separation between soot, brown carbon, and hematite is. It could be right, but there is just not enough error analysis presented, and not clearly enough, to convince a reviewer who is familiar with the retrievals but not a full-time radiative transfer modeler. I have five comments:

1) Coated aerosols: The retrievals assume internally mixed aerosol except for the fine – coarse mode distinction. But their method of sensing the difference between soot

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and hematite relies heavily on the difference between red and blue optical properties. This difference is quite sensitive to the degree of internal mixing of soot. One can show with a coated sphere model, for example, that the Angstrom exponent of absorption of soot is sensitive to the coating thickness even if there is no brown carbon, and that the Angstrom exponent of BC is sensitive to coating thickness (Lack and Cappa, 2010).

2) Nitrogen dioxide. For a manuscript that relies heavily on blue/red differences, I am left wondering what the impact of NO2 would be, since it has an absorption spectrum that can mimic brown carbon or hematite. Absorption by NO2 in the blue can be significant compared to aerosols.

3) Error propagation, especially systematic errors in direct and scattered measurements. I've gone back and reread the 1998 Dubovik paper on AERONET retrievals of smoke and I still do not understand how systematic errors affect the retrievals of absorption. It seems to me that the only way to tell if absorbing aerosol is present is if there is light taken out of the direct beam that does not reappear elsewhere in the sky. Absorption cannot be determined from just a phase function because pretty much any phase function can always be fit with a multimodal size distribution. If the absorption depends on comparing the direct and scattered measurements, then their relative calibration is important. But for AERONET they are calibrated separately. In addition, common-mode scale errors could lead to incorrect albedos. A photometer sensitivity change that reduced the amplitude of both the direct and sky signals would look like absorption – the sun would be darker and the light would not reappear in the sky. Maybe there is some internal air-mass consistency check that is not explained?

4) Similarly, there is no discussion about how assumptions in the size distributions propagate into the results. I know some of this has been discussed in previous publications, but this analysis represents a new level of detail for which new effects might become important. According to Dubovik et al., (JGR, 1998), ". . . good agreement between the measured and the fitted radiances probably results in reliable estimates of single-scattering albedo and phase function even if the microphysical aerosol model is

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not perfect". Is the black carbon volume fraction similarly insensitive to the microphysical model?

5) I cannot comment fully on the strong statements about the Bond et al. interpretation. I do think that Figure 13 in the discussion manuscript is misleading. The uncertainty in a parameter does not depend on where it appears in the computer code. The uncertainty is the result of error propagation. Just because the AAOD is computed after (or before) the refractive index does not make it more or less certain. Indeed, the SSA, which is listed in the top line, could as easily be computed from the size distribution and refractive index and moved to the bottom line. Another way of putting this might be to consider my previous comments. Would a change in relative calibration of the direct and sky measurements affect the refractive index or the AAOD more? What about a change in assumptions about size distributions? Is the refractive index or AAOD more sensitive? What about coating thickness on soot?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 13607, 2015.