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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Absorption Ångström coefficient, brown carbon, and aerosols: basic concepts, bulk matter, and spherical particles" *by* H. Moosmüller et al.

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This paper addresses the links between aerosol complex refractive index, absorption coefficient and Angstrom coefficient. This is a very interesting topic as our community attempts to define these connections in the context of recent discoveries about Brown Carbon aerosols, whose complex refractive index (and thus absorption coefficient) have a significant spectral dependence.

As is described within the paper, the Angstrom coefficient has been used to describe the spectral dependence of aerosol extinction (and absorption) coefficients, and for most aerosol types primarily expresses particle size. This expression becomes more

complicated, however, when the refractive index of aerosols has a spectral dependence. In this case, the Angstrom coefficient represents both size and complex refractive index spectral dependence, and is furthermore dependent upon the wavelengths used to compute the coefficient.

For me, this complex relationship means the original intent of the Angstrom coefficient, as a simple index related to particle size, is no longer valid. It makes much more sense to just stick with the spectrally dependent absorption (or extinction) coefficients than to muddle them into an index that is dependent several aerosol properties and means of definition.

Since much of the community still uses Angstrom exponents, I do think this work is worthwhile in that it links several models for brown carbon aerosol refractive index spectral dependence to the Angstrom exponent. I guess I am a bit disappointed that there is not a larger discussion of the shortcomings of Angstrom coefficients themselves, especially since some of the models have a strange expression as Angstrom coefficients.

The paper also mentions several times that observations of absorption spectra can be quite noisy, which is yet another reason to avoid the use of the Angstrom exponent, since it is often quite difficult to track the relationship between absorption spectra uncertainty and Angstrom exponent uncertainty. I think this paper would have been much more useful if it also presented the uncertainty propagation associated with the various models, since they would ultimately be compared to observations.

Other than these tactical criticisms, this paper is generally well written and presented, and should be published in ACP.

Specific concerns:

1. It is not clear to me the mathematical advantages of the Lorentzian approximation of the damped single harmonic oscillator (SHO) model over the regular SHO model.

Perhaps I'm missing something, so if there are advantages they should be discussed. Otherwise, discussion of this model should be discarded unless its use in previous work can be cited.

2. I'm confused by the wording "divergence toward longer wavelengths" on line 10 of page 24744

3. I'm confused by the last two paragraphs in section 3.2, perhaps it is just wording. Mie theory does NOT depend on the size parameter alone, but on both the size parameter and complex refractive index (which is of course wavelength dependent). I think I understand what you're trying to say here but this isn't the most straightforward way of saying it.

4. I find the labeling on each figure to be too small to be legible, especially for figure 1.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 24735, 2010.