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Interactive Comment

Interactive comment on "Classification and investigation of Asian aerosol properties" *by* T. Logan et al.

Anonymous Referee #2

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General Comments

This paper attempts use aerosol optical properties provided in the AERONET database to classify aerosol type as pollution, dust, or biomass burning. They also introduce a new parameter for classifying aerosol type, $\alpha(\omega_{oabs})$, which is the powerlaw exponent of the single-scatter co-albedo. The authors demonstrate clustering of aerosols according to their typing scheme at known pollution, dust, and biomass burning sites, and use this clustering information to discuss regional and seasonal aerosol characteristics at four Asian AERONET sites. I like the idea of classifying aerosols according to optical properties, but I had trouble with many of the details of this paper (listed below).

Specific Comments

The authors discuss $\alpha(\omega_{oabs})$ long before it is defined with equation 5 on page 10. That

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is, they mention this parameter in the abstract, on pages 5, 6, etc., but they do not tell the reader what this combination of symbols represents until much later. Thus, the reader is left in the dark regarding this parameter until page 10.

page 7, line 16: This sentence is not quite correct, as it describes the aerosol optical depth as integrated along the path of the photon. However, aerosol optical depth is extinction that is integrated vertically through the atmospheric column. These will not be the same values, except when SZA = 0. The authors undoubtedly know this, but improving the accuracy of the wording will help newcomers to the field.

page 7, line 21: $\tau(\lambda)$ is not measured by any space-based instrument. Rather, it is retrieved.

page 8: Using $\alpha_{440-870} < 0.8$ to denote coarse mode particles and $\alpha_{440-870} > 0.8$ to denote fine mode particles is not a great idea, as mixtures of fine and coarse aerosols can easily have $\alpha_{440-870} > 0.8$.

page 9, line 23: This paragraph needs clarification, as it does not seem to be logically consistent. For instance, line 23 says "For weakly absorbing particles (e.g., non-refractory OC particles), the $\alpha_{abs440-870}$ values are near or below one. . ." But what about BC, which is a strong absorber and has AAE values near 1 (as the authors also mentioned in their following sentence)? The authors then go on to say how $\alpha_{abs440-870}$ is 2 or greater for OC dominated particles, contradicting their original statement.

page 10, Eq 5:
Here, the authors are basically applying the Angstrom exponent relationship to the single-scatter co-albedo; personally, I don't believe that this is appropriate. The empirical Angstrom exponent relationship works for aerosol optical depth (AOD) and absorption aerosol optical depth (AAOD) because both of those parameters are nearly linear wrt wavelength on a log-log scale. This is not necessarily true for the single-scatter co-albedo, and therefore Eq 5 is not appropriate. If the authors believe that this empirical relationship is robust, they need to demonstrate that it accurately describes the

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multi-wavelength (440, 675, 870, 1020) co-albedo, and that it does so consistently for pollution, dust, and biomass burning aerosols.

page 10, line 12:
The authors state "We use this parameter to explain aerosol absorptive behavior in low mean τ_{440nm} conditions (e.g., mineral dust regions)." ... and then they go on to explain how they only use it when $\tau_{440nm} > 0.4$. What do they mean by low τ? Something greater than 0.4, but lower than what value?

page 10, line 22+:
The authors cite theoretical work of Yoon (2011) to describe the spectral behavior of τ and τ_{abs}, but the Yoon citation refers to a workshop; hence, the reader does not have access to the details of this study. There are plenty of papers that discuss this topic (some are already cited by the authors), so the authors should point the reader to something accessible in the peer-reviewed literature, and the Yoon citation should not be used.

Also, the descriptions in this paragraph refer to “strong," “intermediate," and “weak" wavelength dependence of τ and τ_{abs}, but the range of allowed values for these adjectives is not described. Additionally, these descriptions do not seem to be consistent with their Figure 5. That is, the authors claim that “Biomass particles have a strong wavelength dependence in $\tau(\lambda)$... Pollution type particles have an intermediate $\tau(\lambda)$ dependence..." However, figure 5 indicates that both biomass burning (Alta Floresta) and pollution (NASA Goddard and Mexico City) indicate similar Angstrom exponents (i.e., similar y-axis values).

page 11, lines 8, 14, 25:
The authors discuss “good agreement" with model outputs of Yoon (2011) with AERONET at four selected sites, but they do not show any model output. Also, since Yoon (2011) is unavailable to the reader, there is no way of knowing the details of the model. If the authors want to discuss/compare AERONET data products to the Yoon

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(2011) technique, they really need to describe the Yoon (2011) technique in this paper, and show some of the Yoon (2011) results.

page 13, line 26: The authors attribute an increase in Reff from Spring to Fall at Mukdahan to hygroscopic growth (there is no Summer data), but it seems odd that RH would be higher in the Fall than in the Spring; a comment about the climate in that region would strengthen this statement. A statement about the RH at Mukdahan also appears on page 14, line 10. Thus, tying these comments to one of the many RH climatologies that are available would strengthen this speculation.

page 13, line 28: I didn't check the citations that the authors provided, but is biomass burning really dominated by coarse mode particles in that region?

page 16, line 2:
Here, and in at least one other place in the paper, the authors attribute low absorbing Angstrom exponent (AAE) values near $\sim$1 to "weakly absorbing" aerosols. However, AAE is an indicator of spectral dependence of absorption, not the magnitude of absorption. Black carbon is an example of an aerosol that is highly absorbing, yet it has a low AAE of about 1 (as long as the particles are sufficiently small).

Figure 2 is described as "statistical results," but what does this mean? Are these average values for the various sites, or medians? Are they averages for all available data? How much data per point? Are the results normalized and then averaged, or averaged and then normalized? Again, the authors point to Yoon (2011) for the methodology, but this is a "workshop" presentation that is not available to the reader. Also, some indication of the spread of the data should be shown (like standard deviations or standard errors). Finally, this figure would be more useful on a log-log scale; that way the reader can compare the different Angstrom exponents (i.e., logarithmic slopes) between the sites.

The discussion in section 4.1 is difficult to follow because of the irritatingly poor graphics (thin lines, small fonts. etc.). For instance, it is still difficult to read the subscripts in the

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legends of Fig 3 at 300% magnification! Everything in a figure should be readable on a laptop screen, since this is how people often read papers these days.

Figure 4 should include standard deviations of the means for all months, as it is quite possible (probable?) that much of the variability they discuss is not statistically significant.

Clusters II and IV seem to be nearly identical in Figure 5; how would one differentiate these two clusters without a priori knowledge?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 18927, 2012.