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Atmospheric Chemistry and Physics Discussions

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

T. Logan et al.

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1) The authors discuss a(woabs) long before it is defined with equation 5 on page 10. That 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.

We have removed this parameter from our study pending further investigation of the feasibility of using it to classify aerosol types.

2) 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.

We have corrected this line as per your suggestion.

3) page 7, line 21: t(l) is not measured by any space-based instrument. Rather, it is retrieved.

We have corrected this line as per your suggestion.

4) page 8: Using a440-870 < 0.8 to denote coarse mode particles and a440-870 > 0.8 to denote fine mode particles is not a great idea, as mixtures of fine and coarse aerosols can easily have a440-870 > 0.8.

The a440-870 value used to delineate coarse and fine mode particles changes among many studies. We have modified this to 0.75 as per the Eck et al. (2005) study as they used AERONET data.

5) 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 aabs440-870 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 aabs440-870 is 2 or greater for OC dominated particles, contradicting their original statement. We have re-worded this paragraph to list the AAE values for BC, OC, and mineral dust. We explain in Sections 3 and 4 how the absorptive wavelength dependences of AAOD and woabs (single scattering co-albedo) can discern the influences of BC, OC, and mineral dust. 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 multi-wavelength (440, 675, 870, 1020) co-albedo, and that it does so consistently for pollution, dust, and biomass burning aerosols.

We realize the relationship does not obey a true power law, however, we did notice certain trends and values associated with biomass, pollution, and mineral dust. Though we have removed this section completely as per your suggestion, we will conduct a future study on this parameter with more evidence.

6) page 10, line 12: The authors state "We use this parameter to explain aerosol absorptive behavior in low mean t440nm conditions (e.g., mineral dust regions)." . . . and then they go on to explain how they only use it when t440nm > 0.4. What do they mean by low t? Something greater than 0.4, but lower than what value?

We have removed the discussion of the a(woabs) parameter.

7) page 10, line 22+: The authors cite theoretical work of Yoon (2011) to describe the spectral behavior of t and tabs, 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 t and tabs, but the range of allowed values for these adjectives is not described.

We have found and cited the Chung et al 2012 study that contained the figure used in the Yoon (2011) study. We compare and contrast the wavelength dependences among the four sites relative to particle size (t) and particle composition (tabs and woabs) with foreknowledge of the characteristics of regions dominated by biomass, pollution, and mineral dust aerosols. In fact, both the Chung et al. 2012 and Yoon (2011) studies suggest there is no need to assume aerosol types in advance when using real time

data from AERONET which we corroborate in our study.

8) 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 t(l). . .Pollution type particles have an intermediate t(l) 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).

We have seen in this study as well as other studies (in which we cite) that biomass aerosols are consistently smaller than pollution aerosols on average. NASA Goddard has more sulfate type aerosols that are typically submicron and smaller than BC/soot particles exhibited at Mexico City. Though smoke particles from biomass burning can be in the coarse mode, they have been shown to quickly be reduced to submicron size through photo-chemical and aging processes. Therefore, we feel our results show this in detail.

9) 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 (2011) technique, they really need to describe the Yoon (2011) technique in this paper, and show some of the Yoon (2011) results.

We have re-written this section comparing our results to the Chung et al. 2012 study.

10) 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.

We cite the Gautam et al. 2012 study that discusses biomass burning and the climatological pattern in Thailand during the spring and autumn seasons and reword this statement as per your suggestion.

11) 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?

We have removed this statement.

12) 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).

We have clarified this statement in that BC absorbs at all wavelengths and has higher absorption in the near IR. It also has a linear wavelength dependence especially for submicron size particles as compared with OC particles that have strong absorption in the visible leading to higher AAE values. It was not our intention to state BC particles as weakly absorbing but mixtures of BC and OC can either lower or increase AAE values which make using this parameter problematic.

13) 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.

We have re-worded the figure caption to show this is the overall mean t, tabs, and woabs for the time period of the study. The number of observations for each site is provided in Table 1. We also change Figures 2a and 2b to log-log plots as per your suggestion. We do not do this for Figure 2c since the woabs parameter does not follow a true power law relationship.

14) 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 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. We have re-plotted the figures with better resolution and larger font. 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.

We have added the standard deviations for each month. We do feel the variation is statistically significant and does show a seasonal and regional dependence as evidenced by other prior studies cited in our study.

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

We agree that Clusters II and IV look similar since they both describe the behavior of carbonaceous aerosols generated from combustion. However, the nature of pollution (Cluster II) and biomass (Cluster IV) aerosols are different since many studies point out the size of biomass aerosols are generally smaller (higher AE) where BC with some type of OC coating tends to dominate the absorptive characteristics as opposed to pollution particles where OC tends to dominate the absorptive characteristics.

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

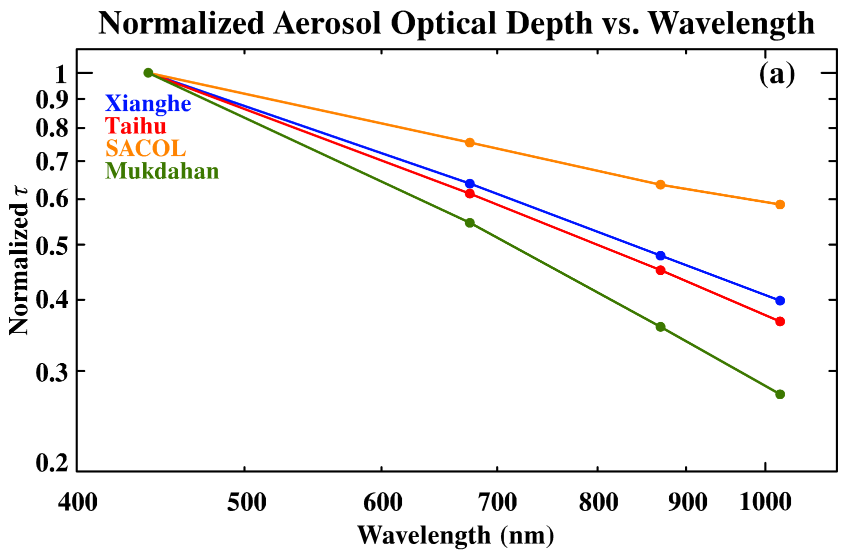

Fig. 1. Re-plot of Figure 2a. Log-log plot of wavelength dependences.

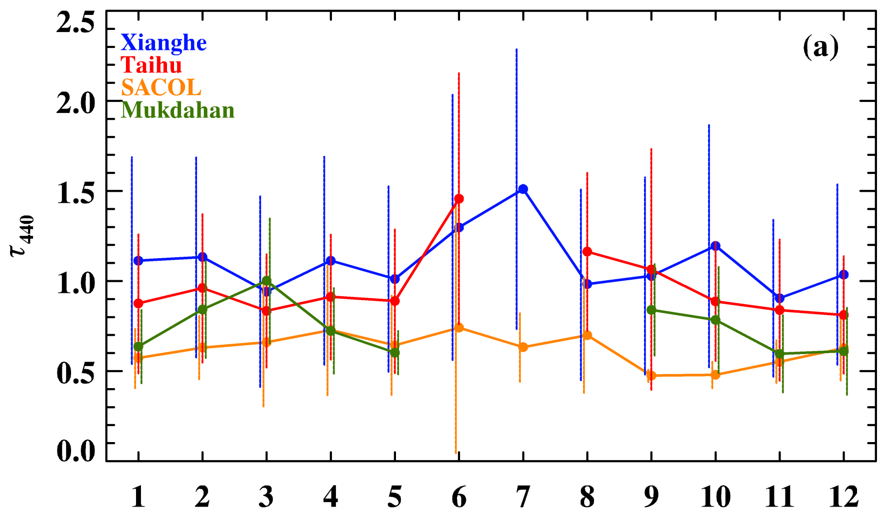

Fig. 2. Re-plot of Figure 4a. Aerosol optical depth plot with error bars denoting one standard deviation.