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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) Figures: Improvements of plot quality are recommended. I am reading a PDF version of the manuscript and had some difficulty in reading the font. The authors may consider adopt larger font sizes. More importantly, the nature of this analysis is statistical and error bars and uncertainty ranges are totally missing in the presentation. These are critical in my opinion for interpreting the results and therefore must be added to the relevant plots.

We have increased the size and thickness of the characters, words and plots. We added error bars to the monthly mean plots in Figure 4 to show the variability in aerosol type and size.

2) A more streamlined presentation is suggested. This is not an issue with the English grammar or clarity. Some of the introduction and discussion repeat themselves here and there, which makes the reading and understanding of the paper not as enjoyable had they been more precise and concise. For example, when introducing relevant parameters used in this study the authors can be more direct to the point of their usage, how they are derived etc without mentioning bits of information that are not critical.

We have carefully streamlined the presentation to focus on three specific areas: aerosol regional and seasonal variability, aerosol internal properties as inferred by wavelength dependences in AOD, AAOD and single scattering co-albedo (woabs), and a cluster analysis based upon the size and absorptive properties of Asian aerosols.

3) Given the evidence and work done in this manuscript some of the conclusions are overly strong. Some discussions suffer from being too vague and general. A few examples are given. Line 9 at page 18937: no modeled results are shown here while it is claimed that obs are in 'good agreement'. Line 10 at the next page: 'physio-chemical' properties are not represented by these parameters. The authors can possibly infer some relevant (maybe qualitative) information from these parameters, but they certainly do not 'describe' them. Line 10 at P. 18939: there is no evidence presented for this statement. The same can be said to the statement that refers to figure 1 in the next a few lines. Line 17 P 18940: this kind of general statements is found at many places. Such statements are fine for general introduction, but at places of specific discussion more refined arguments are needed.

We have added the Chung et al. 2012 study as a reference for Section 3 where we discuss the wavelength dependences of AOD and AOD at the four selected Asian sites and the comparison to the results of the Chung et al. 2012 study where we reference Figure 1. Figure 1 is in fact based on an exhaustive study of 19 AERONET sites around the globe which includes dust aerosol dominant, pollution aerosol dominant, biomass aerosol dominant, and mixtures of the dust, pollution and biomass aerosols. We have re-written this section and included the co-albedo wavelength dependence to infer more about the internal properties of the aerosols at each site. As per your suggestion we changed our wording to reflect inferences rather than descriptions of aerosol properties by wavelength dependences.

4) The authors are suggested to modify their abstract and title. For example, the title is quite general while the investigation does not cover that much ground. The four sites only cover a few areas of East and small part of South-east Asia. Majority of the results are not about classification. Instead, most discussions are about seasonal and climatological properties of aerosols at different sites. A more focused and specific title is suggested to replace the current overly general and grand one. The abstract is a bit difficult to read through for any non-specialist. Please consider revising it to make any interested reader be able to comprehend what is done in this work.

Though the study covers four sites in Asia, these sites represent aerosol mode and type dominance that is seen throughout all parts of Asia. We include Xianghe to represent a mixed aerosol region that is seasonally influenced by mineral dust and biomass aerosols and strongly influenced by urban type aerosols year round. Taihu is similar to Xianghe but is more pollution and biomass aerosol dominant. SACOL is within a desert region with few urban/biomass influences. Mukdahan is within a strong biomass generation region. These are the main categories in which many Asian cities fall into. We have modified the abstract to be more specific in describing the main goals of our study.

Minor Comments: 1) There is no Eck et al. 2004 (line 6 p 18932) in the reference list.

This has been changed to say Eck et al. 2005.

2) Figures need error bars.

Error bars have been added to Figure 4.

3) Line 7 p 18937: what are the authors referring to by 'statistical results'. No particular statistical metrics are presented in this figure.

We describe in this figure and in the corresponding section that we use the overall mean values of the data sets at each of the four sites: Xianghe (2001-2010), Taihu (2005-2010), SACOL (2006-2011), and Mukdahan (2003-2009).

4) Line 22 p 18938: is it really that dust particles over Xianghe are coming from Gobi desert? Any reference or evidence for this claim?

We cite our previous study (Logan et al. 2010) and other studies that show Xianghe and neighboring Beijing are periodically affected by dust activity from the Gobi Desert but more prominently in the spring months.

5) Line 7 p 18939: what 'variations' are the authors referring to?

We refer to the seasonal variations in particle volume size distribution and re-word this in the manuscript as per your suggestion. (Lines 287-289)

6) Third Paragraph on p 18939: there's some confusing discussion here. In SON taihu and xianghe show decrease in both modes while it is stated on line 29 that there's an increase in coarse mode.

We have re-worded this to compare the size distributions of the autumn to the summer months and explain the reason behind the slightly higher coarse mode peak in the autumn.

7) P. 18941: why the absorption angstrom exponent is lower over SACOL than Xianghe and Taihu? It is not clearly resolved in the manuscript.

AAE is lower at SACOL because of fewer urban/industrial influences which we explain in Section 4.3.

8) Line 4 p 18942: these aerosols are not weakly absorbing. They only have low Angstrom exponent.

We have re-worded this as per your suggestion.

9) Line 3-5 P 18943:I don't see any significant changes for Mukdahan.

We have removed the section on the absorption Angstrom exponent of the co-albedo parameter in this study as per another referee's suggestion.

10) Significant overlap exists for clusters I and II. The general discussion on clustering approach is not very convincing given the results presented here. The authors are encouraged to either present more results or weaken the conclusion.

We attribute the overlap for Clusters I and II to variabilities in aerosol generation and possible instrument errors. We feel that since we see similar overlap and variabilities in other locations, this method shows promise in identifying the characteristics of aerosols in regions with several aerosol types. We have adjusted the conclusion to reflect that this method needs much more refinement.

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

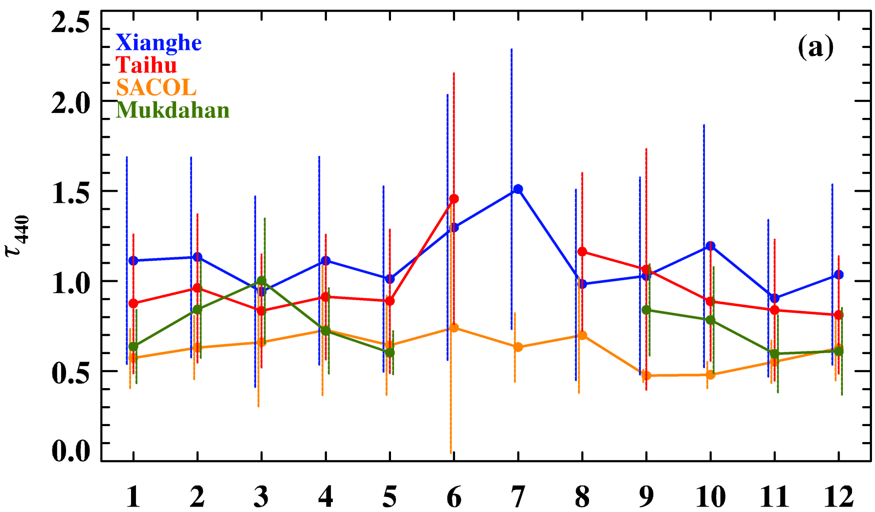

Fig. 1. Figure 4a. Aerosol optical depth at 440 nm plot with error bars denoting one standard deviation.