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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #1

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The manuscript by Logan et al. presents data analysis of four AERONET sites located in Chin and Thailand. They propose that from the AERONET retrievals one can classify aerosol 'composition' and investigate their source characteristics. While their results are of interest for the readers of ACP/ACPD important changes are needed to make this manuscript publishable. My comments are in the following.

Major Comments:

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. 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. 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. 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.

Minor Comments: 1) There is no Eck et al. 2004 (line 6 p 18932) in the reference list. 2) Figures need error bars. 3) Line 7 p 18937: what are the authors referring to by 'statistical results'. No particular statistical metrics are presented in this figure. 4) Line 22 p 18938: is it really that dust particles over Xianghe are coming from Gobi

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desert? Any reference or evidence for this claim? 5) Line 7 p 18939: what 'variations' are the authors referring to? 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. 7) P. 18941: why the absorption angstrom exponent is lower over SACOL than Xianghe and Taihu? It is not clearly resolved in the manuscript. 8) Line 4 p 18942: these aerosols are not weakly absorbing. They only have low Angstrom exponent. 9) Line 3-5 P 18943:I don't see any significant changes for Mukdahan. 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.

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