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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Identification of column-integrated dominant aerosols using the archive of AERONET data set" *by* Y. Choi et al.

Anonymous Referee #2

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The manuscript aims at applying a classification scheme to discriminate aerosol types over the Anmyon site using column-integrated optical properties derived from AERONET observations. The same scheme is also applied to other well-characterized AERONET sites. This reviewer considers that the paper is not adequately exposed and that it may require substantial revision before it can be accepted for publication in ACP. General and specific comments follow here.

General comments:

1. The classification method is not sufficiently described, in particular the steps regarding the cluster analysis. The authors claim that the proper number of clusters is decided in the clustering process (page 26633, line 25). However the aerosol types

(MD, OC, BC, etc.) were established in advance. I think this crucial point is not clear at all. Are the values of AOD, FMF, SSA and AAE in Table 2 used for clustering or are they the average values for each cluster?

2. The need of AOD(440nm)>0.4 for level 2.0 optical properties is a strong limitation of the method. In practice the method excludes a huge portion of observations in all investigated sites. "Low AOD" does not mean that no classification can be attempted, even though it cannot be as specific as in the higher AOD cases. For instance, low AOD and low Angstrom exponent may easily lead to marine aerosol type identification (Smirnov et al., 2002). A simple scheme based on AOD, Ansgtrom exponent and fine mode fraction of the AOD (see climatology by Holben et al., 2001), or Angstrom exponent derivatives (Gobbi et al.) can give good insight on the aerosol type and is clearly missing in this work.

3. The results section looks at average optical properties of the investigated types. This is a wrong approach because the properties are conditioned by the classification scheme. The authors know it (p. 26640, line 22) but still used the ill-posed approach throughout the paper. In my view, the correct approach is investigating the presence rate of each aerosol type in the investigated sites (as it is also done by the authors).

4. Section 3.1 is a sort of continuation of the methodology section, with references used to support the classification scheme and very few results. I suggest that the few results are merged with section 3.2 and that all methodology and references are merged with section 2.

5. Two very confusing concepts are used in the paper. First: "the dominant aerosols" is used to define situations with AOD(440nm)>0.4, in which the classification scheme can be applied to discriminate the predominant aerosol type. This must be reformulated. Second: the way used to calculate the "occurrence rate of dominant aerosols", based on number of sun hours and number of level 2.0 almucantars (if I understood correctly) leads to strange interpretation of the data coverage, of just few percents. Given the

typical temporal variability of the atmospheric aerosol, I would rather analyze in terms of days instead of hours.

Specific comments

P26632, L5: improve definition of single scattering albedo.

P26632, L19: “inflection point”

P26633, L10: “absorbing aerosols” instead of “absorption aerosol”.

P26633, L26: explain SPSS 12.0 and give citation to Romesburg, 2004 in line 22.

P26634, L3: provide number of days instead of number of measurements (similarly to general comment 5).

P26634, L19: specify wavelength for SSA, at least the first time.

P26635, L1: if a mixture of dust and pollutants was measured, you cannot say “dust AAE measured at Gosan was. . .” because they did not measure dust alone. This is repeated several times in the text. The authors can try to be more precise.

P26636, L8: water vapor is not derived from inversions, it is retrieved from the direct Sun observations.

P26636, L9, In Fig. 3 there is apparently some dependency of fine mode effective radius with respect to water vapor. However quite constant fine mode effective radius was observed by Gonzi et al. (2002) for many AERONET sites. Are you sure that the observed dependency is due to hygroscopic growth?

P26638, L12: I would find more logical applying the classification scheme first to well-characterized AERONET sites; and then to your Anmyon site. However that is just a point of view and I must respect the authors’ choice.

P 26642, L5-11: these last sentences are too ambiguous and unnecessary.

P 26642, L12: the necessary acknowledgment to AERONET program and station PI’s

is missing.

P 26650,Table 2: such high AOD for dust is hard to believe as representative or average value. Furthermore, an extensive property like AOD is not the best choice for type identification.

P 26652, Fig. 2: the second filter regarding imaginary part of the refractive index should take into consideration the estimated uncertainty for this parameter. If the value at 440nm is lower than the value at longer wavelengths but still within uncertainty, I consider that they should not be removed.

P 26653, Fig. 3. This kind of relationships can be of great interest but the authors do not investigate or analyze them in depth. I encourage them to analyze it further and find other ones that can be very illustrative to the aerosol type interpretation.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26627, 2013.