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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 #1

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This paper classifies dominant aerosol types at the Anmyon AERONET site, as well as 6 other AERONET sites. Although the paper is quite readable, I don't believe that it should be published in ACP.

My main issue with this paper is that the methods section does not have nearly enough details. The authors results can not be reproduced on the basis of what is presented here. How do the authors classify as MD, MD+ carbon, or mixed coarse when FMF > 0.5? This is not described anywhere. SSA > 0.95 has to be secondary ions with their scheme, but what are the authors including in this set? Anmyon is close to the coast – does this include sea salt? What about secondary organics? What do they mean when they say that BC and OC are the dominant aerosols when FMF < 0.5 and SSA < 0.95? What can't mixtures of BC with secondary ions produce the same FMF and SSA?

Basically, the authors have not presented any data to demonstrate that the scheme outlined in Figure 2 works at all. Afterwards, they have a nice discussion of the results, but the method description is so imprecise that the reader can not conclude anything from the results. The authors need to elaborate the methods section extensively so that others have enough information to reproduce their results, and for the reader to draw meaningful conclusions about the results.

Major issues: Page 2, line 15, the authors state:

"An automatic tracking sun and sky-scanning radiometer, CE 318 (CIMEL Electronique; also called a sunphotometer) measures direct radiation on the principal plane (with fixed azimuth angle and varied zenith angle) and diffusive radiation on the almucantar plane (with fixed zenith angle and varied azimuth angle up to 180 in both sides) at 8 wavelength channels (340, 380, 440, 500, 675, 870, 940 and 1020 nm) (Holben et al., 2001)."

This is not correct, as direct radiation is NOT measured along the principle plane. Both scan modes (almucantar and principle plane) measure the diffuse field. "Direct" radiation is obtained only when the instrument is pointing at the sun. Also, only 4 channels are measured in the almucantar (not 8). Brent Holben certainly knows this.

Page 7, line 21: This clustering scheme is the meat of your paper, but you're glossing over it with a single paragraph and referencing a 258 page book. What criteria go into this black box???

Page 9, line 1: "BC and OC do not show significant differences in AOD, SSA and FMF."... Again, what goes into the clustering, though? The authors are classifying everything with SSA < 0.95 and FMF > 0.5 as BC/OC dominant, so it is not too surprising that you don't see differences in these parameters. How do you discriminate BC from OC in your scheme? Where is the data to back up this statement (and other statements like this)?

Page 10, line 1: "SSA of secondary ions is high because of their colors which hinder absorption..." I don't understand this statement.

Page 10, line 1: "...This is confirmed in Fig. 3..." If you want to "confirm" that SSA increases wrt water vapor, why don't you plot it directly (instead of plotting Reff vs water vapor and SSA vs Reff)? This figure did not confirm anything for me.

Page 10, line 5: "The effective radius (Reff) is defined a (http://aeronet.gsfc.nasa.gov/new_web/Documents/Inversion_products_V2.pdf)"

Effective radius was defined long before AERONET existed:

Hansen, J., and L. Travis (1974), Light scattering in planetary atmospheres, Space Sci. Rev., 16, 527–610.

I believe that this article cites an even older article for Reff as well. You can also find it in Seinfeld and Pandis, of course.

Page 11, line 2: The authors state "Here, we presume that diffuse radiation was measured at one hour intervals. The number of daytime hours was counted on a monthly basis by examining the earliest and latest times at which diffuse radiation was measured."

This is a faulty approach. There is a long period in the middle of the day (sza < 50) where almucantar scans are not included in the level 2 dataset. Thus, the authors are grossly overestimating the number of daytime hours during which diffuse radiation could be measured (at least in the summer months). This once again begs the question – did Brent Holben read this?

Page 12, line 19: The authors are using Cape Verde as typical dust, but that location also sees biomass burning in the winter. How do the authors filter out biomass burning?

Page 12, line 24: The authors state "Heavy aerosol loading at Beijing is confirmed by a high occurrence rate of dominant aerosols, which is 6.3%, the highest among seven

sites, more than five times that at Anmyon."

So basically, the authors are only able to classify the dominant aerosol type 6.3% of the time at your most favorable site? How is this important? Some discussion would be helpful.

PAge 14, line 16 and Figure 5: 14% BC at GSFC is huge! In fact, is looks quite large everywhere in Figure 5, except Cape Verde.

Table 2 and Figures 4 and 5: What is the timeframe for the data, and how many datapoints at each site? Is this all available data at all these sites? What years?

Minor issues: Page 6, line 17: I'd avoid using the nomenclature "FMF" to represent the fine fraction of the volume distribution, as it has become customary to use the term FMF for fine mode *optical depth* fraction (as in the MODIS retrievals).

Page 7, line 10: I don't understand... Are you computing the SSA for the fine mode, here, or is this the SSA of both modes when FMF < 0.5?

Page 8, line 24: See also Chun (PNAS 2013) and Bahadur (PNAS 2013).

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