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Atmospheric Measurement Techniques Discussions

Interactive comment on "Aerosol-type classification based on AERONET version 3 inversion products" *by* Sung-Kyun Shin et al.

Anonymous Referee #2

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This paper attempts to determine the "aerosol-type" based upon the retrievals in the AERONET database. The main aerosol types defined in this paper are Pollution, Pollution-Dominated Mixtures (PDM), Dust-Dominated Mixtures (DDM), and Pure Dust (PD). Additionally, Pollution aerosols are divided further into four more subtypes, defined as Non-Absorbing (NA), Weakly Absorbing (WA), Moderately Absorbing (MA), and Strongly Absorbing (SA).

The main aerosol types are determined exclusively by the linear depolarization ratio (PLDR) at 1020 nm provided in the AERONET database. The pollution subtypes are determined exclusively by the single-scatter albedo (SSA) at 1020 nm.

The paper is well written and provides the reader with a relatively simple way of categorizing aerosols by type, but the paper would be much stronger if additional details are added.

Major issues:

Page 2, Line 32: There are actually quite a few polarization lidars collocated with AERONET... see https://mplnet.gsfc.nasa.gov/ . All MPLNETs are collocated with AERONET, since the AOT constraint provided by AERONET is necessary for the MPLNET extinction profile retrievals. Granted, it is difficult (impossible?) to do meaningful statistical studies with the data because one can only download one day of MPLNET data at a time, but the hardware has been in place for many years.

Page 3, line 13: Dubovik (2006) is an article about incorporating spheroids into the retrieval, and is not the appropriate citation in this context. The authors should cite these papers instead:

Dubovik, O. and King, M.: A flexible inversion algorithm for retrieval of aerosol optical properties from sun and sky radiance measurements, J. Geophys. Res., 105, 20 673–20 696, 2000.

Dubovik, O., Smirnov, A., Holben, B., King, M., Kaufman, Y., Eck, T., and Slutsker, I.: Accuracy assess- ments of aerosol optical properties retrieved from Aerosol Robotic Network (AERONET) sun and sky radiance measurements, J. Geophys. Res., 105, 9791–9806, 2000.

Page 3, Equation 3: I don't understand why the authors present this equation in the section entitled "Parameters", as they never really use it and it never shows up in plots or the discussion. They do state in Section 2.4 (page 5, line 15) that they are using R_d to define PLDR thresholds, and they provide a mapping between PLDR and R_d on lines 32-35 of page 5. However, the definitions of Pollution, PDM, DDM, and dust are rather arbitrary for both parameters.

I would just remove R_d from the paper altogether and define thresholds using the PLDR parameter. Alternatively, move Eq 3 into Sect 2.4 where there is some discussion of how this parameter is used, which would provide some context for the Equation... and then omit PLDR from the remainder of the paper and use R_d in the figures and discussion.

Page 3, line 26: "Shin et al (2018) recently discussed. . ." Some context needs to be provided here... why are PLDR(870) and PLDR(1020) more reliable than PLDR(440) or PLDR(675), when all of these PLDRS come from the same AERONET retrieval? I believe that the authors came to this conclusion because the AERONET PLDR values at visible wavelengths were biased low of lidar measurements at 532 nm, whereas the near-infrared AERONET PLDRS had higher values that were more consistent with the lidar values at 532 nm. Some discussion here would strengthen the choice of PLDR(1020).

Page 4, line 12 and elsewhere: There is not "Gosan" AERONET site... do the authors mean "Gosan SNU?"

Page 4, line 31: Authors state: "The composition of mineral dust often includes clay minerals or iron oxides that lead to strong light-absorption at short wavelengths"

Although iron oxides are highly absorbing, clay minerals are not.

Page 4, line 34: Authors state: "Aerosol particles which are categorized by PLDR into dust particles and pollution particles thus are relatively low light-absorbing at 440 nm compared to their light-absorbing capacity at other wavelengths."

I don't understand this logic. . . the authors just stated on lines 30-32 that "dust often contains. . . iron oxides that lead to strong light-absorption at short wavelengths." Also, authors mention biomass-burning on line 28, which also has strong light absorption at 440nm. So the authors contradict themselves.

Page 5, lines 3-6: Fig 2 does not show these concepts very well... the fine and coarse modes have different vertical axes, which makes it difficult to verify these words with the figure. There are also too many lines that are crossing all over the place, which makes it hard to understand. Since you are really discussing CMFvc vs PLDR, why not just plot these parameters as a scatter plot?

Page 5, line 8-9: Authors state: "External mixtures of mineral dust and non-spherical particles reveal PLDRs in the range from 0.08 to 0.30." What is the basis of this statement? Was this shown in Burton 2012 or Shin 2015? As written, it does not make sense. Dust is non-spherical, so you're discussing mixtures of non-spherical dust with other non-spherical aerosols? It is important to be clear on this point because you are using PLDR to define thresholds for PDM and DDM. However, here you define dust mixtures as 0.08 < PLDR < 0.28, but everywhere else the lower threshold for PDM is 0.06 (not 0.08)... why the inconsistency?

Page 5, line 18: Authors state: "The reason for using PLDR instead of FMF, which has been used, e.g. in Schuster et al. (2006) and Lee et al. (2010), is that the former study provides a clearer separation between non-spherical dust particles and rather spherical non-dust particles."

This statement needs to be backed up with data, as it seems contrary to Figure 4 and page 6, line 24, which states: "We find a strong negative correlation of Rˆ2 = 0.80 for FMF vs. PLDR."

Thus, the data presented in this paper indicates that both FMF and PLDR are capable of discriminating dust from pollution. No case has been made that one of these parameters is significantly better than the other.

Page 5, line 20: Authors state "Shin et al. (2019) compared coarse-mode AOD provided by AERONET to dust AOD retrieved with the use of PLDR and showed that the former tends to overestimate the contribution of mineral dust to AOD."

This sentence needs to be expanded, as it is unclear. First, what is being used as the "truth?" Are the authors assuming that their use of PLDR with the thresholds that they have chosen are the "truth?"

Page 6, line 9: Authors state: " The SSA values for aerosol mixtures (e.g., BC with sulphate) vary, depending on the relative humidity and mixing ratio (e.g., 0.91 at 70% RH and 0.5 BC/sulphate mixing ratio for an internal mixture) (Wang and Martin , 2007)."

Check this... SSA = 0.91 is way to high of a value for a 0.5 BC/sulphate mixing ratio and is inconsistent with Wang and Martin (2007), Figure 3.

Page 6, line 12: Authors state: "The biomass-burning aerosol contains BC, while anthropogenic aerosol contains BC and/or NA."

This sentence implies that biomass-burning (BB) aerosols do not contain non-absorbing aerosols (NA), which is not true. Also, BB aerosols often contain BrC that is highly absorbing at 440 nm but non-absorbing at 1020 nm.

Page 6, line 15: The setting of the "typing thresholds," both for SSA and PLDR, is the weakest element of this paper. Although the values chosen are reasonable (e.g., strongly absorbing aerosols have SSA < 0.85, Moderate absorbing have SSA 0.85-0.9, weakly absorbing SSA = 0.9-0.95, and non-absorbing have SSA > 0.95), they are basically pulled out of thin air.

Other studies (e.g., Burton 2012) use histograms and Mahalanobis distances to map multiple optical properties to known aerosol types.

Page 7, line 13: Authors state: "SSA at 1020 nm varies around of 0.87 in Sectors D and F."

This is inconsistent with Fig 5b, as SSA = 0.87 is not even on the scale (i.e., y-axis ranges from 0.88-1.0).

Page 7, lines 16-17: Authors state: "We conclude that the FMF might be too ambiguous a parameter to distinguish aerosol types in mixed dust-pollution plumes. In contrast, PLDR is a more reliable parameter for aerosol type classification in which mineral dust is part of the aerosol."

This is not at all obvious from the author's presentation. In fact, the argument is rather circular. i.e., use PLDR to distinguish type, then use FMF to validate conclusions, then discount FMF as ambiguous. Also, the authors note above that FMF is highly correlated with PLDR ($R^2 = 0.8$), so the two parameters are not really independent.

Page 7, lines 24-31: The authors need to add 12-month averages to Fig 6 so that the reader can follow along. None of the numbers in this paragraph correspond to the figure; presumably these numbers are averages over all 12 months, but the authors do not state this.

Page 8, line 33: This isn't really validation, as the authors do not have measurements to provide them with the "truth." Rather, this is basically a continuation of Section 3.2, but for different source regions.

I think the authors should include the seasonal variability at some of the sites in this section, similar to Fig 6. For instance, the West Africa has seasonal biomass burning as well as seasonal dust cycles. This should show up nicely at Banizoumbou and other African sites with a stacked chart like Fig 6.

Figure 4: Why show PLDR instead of R_p (from Eq 3)? If R_p really does present the contribution of dust to the particle backscatter coefficient, it would be much more interesting to see R_p than PLDR in this figure. Besides, PLDR only provides 4 broad categories of dust/pollution mixtures (dust, dust-dominated mixtures, pollution-dominated mixtures, and pollution), whereas the R_p would provide much higher resolution.

Figure 5: I still don't like the multi-scale y-axis. Maybe try a single log scale for y-axis, if a single linear scale does not work.

Colors are difficult to discriminate, even for the non- color blind. Color-blind people have no chance of understanding this figure. I would add symbols to the lines for those folks.

And you don't really need all those errorbars. . . May show one or more "representative" errorbars.... or just state some representative values in the caption. What do the errorbars mean, anyways? 1-sigma? 2-sigma? 1 SDOM? 2 SDOM?

Table 2: Something wrong here. . . FMF ranges from 0.31 to 0.90, but AE only ranges from 0.5 to 0.65. AE = 0.52 and 0.60 for PDM regions D&F, and AE=0.65 for polluted region G. All of these values are inconsistent with the literature, where AE < 1 is often used for "pure" dust. At a minimum, the authors should provide some discussion as to why their polluted sector has such a low AE.

Table 3: This is a little messy to look at. You don't really need the number of cases for each aerosol type, since that is covered by the first numerical column and the percentages. If you get rid of the number of cases, you'll have one value (the percentage) for each aerosol type, and this will be a lot easier for the reader to process.

Alternatively, this table could be converted into a stacked bar chart of percentages (like Fig 6). One stacked bar for each site.

Minor issues:

Page 2, line 28: replace "non-spherical" with "spherical."

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