Atmospheric Chemistry and Physics Discussions

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Comment on acp-2022-219

Anonymous Referee #2

Referee comment on "Long-term variation study of fine-mode particle size and regional characteristics using AERONET data" by Juseon Shin et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-219-RC2, 2022

The paper claims to fulfill a long-term aerosol properties study using AERONET data. The authors present the analyses separating between dust and fine mode particles. Further analyses try to separate between different aerosol components including also the Angström parameter. The different multi-year trends are analyzed and discussed. Therefore, the topic is of interest for Atmospheric Chemistry and Physics.

However, I agree with all the comments raised by referee 1. AERONET project includes extraordinary efforts of many research groups worldwide and homogenize data quality check and criteria. Data use in the paper does not meet AERONET standard quality and the methodology proposed by the authors is questionable. I would like to add some more points that make me not to recommend the paper for publication, and I recommend the authors to re-submit the paper after addressing the main comments and those by referee 1:

- Author's methodology is based in depolarization ratios and lidar ratio. To obtain these data it is necessary the retrieval of aerosol size distribution and optical properties from AERONET inversion that combine sun and sky radiances and solve and ill-posed problem. This is good for obtaining depolarization ratios to compare with lidar systems or to compute lidar ratios for elastic lidar. But for doing climatology studies it has no sense because the inversion by itself provides aerosol characteristics. Obviously, the inversion is sensitive to noise in the input measurements and has uncertainties, and by these reasons AERONET establish strict criteria for the retrieval. The most important is geometry that limit retrievals early in the morning and in the late afternoon, and that AOD must be greater than 0.4. For all these reasons I believe that the methodology proposed by the authors provided do not add value to AERONET inversions.
- The limitations in the use of AERONET inversions mentioned above make 7-8 inversions maximum per-day in the best conditions. But the strong limitation in AOD (>0.4) make the analyses is biases for studying AOD statistics. However, AOD from direct solar measurements are less restrictive and AERONET present much more data. So I believe these are the data that must be used. AERONET also provides fine and coarse mode

AOD by the Spectral Deconvolution Algorithm that can be used. All of this is supported by the low number of measurements that authors present in Table S1.

- The authors claim a statistics of Angström parameter for different aerosol types and relate directly Angström parameter with size distribution. The authors must be careful because Angström parameter is an intensive parameter and only tells about the predominance of one mode over other. I found many statements in the text that try to relate Angström parameter with number of particles. That must be corrected. Some critical examples are in Lines 342-343, Line 352, Line 354, Lines 368-369 or Line 395.
- The effort of analyzing 17 AERONET stations is very valuable. But currently AERONET directly provided in its web page a statistics for all AERONET´s stations. Therefore, the added value must be clear. There are more than 400 stations and the analysis must cover all regions covered by AERONET. For example, there are more stations in Europe and South Asia than those presented by the authors. Other regions are excluded with no reasons – e.g. North America
- For climatological studies it is usually required a period of 30 years, so I do understand why to establish a period of 9 years. Obviously there are very few stations with 30 years measurements, but there are AERONET stations since 1995 continuously acquiring. What is clear that the number stations with more than 10 years of continuous data is considerable higher than the number used by the authors.
- There mistakes in the organization of the paper. There are many discussions based on the supplementary material. Figures S1-S7 must be included in the paper. There are many other Figures and Tables not even introduced in the paper, especially Figure 2 and Tables 3 and 4.

Other comments:

I agree with Referee 1 that title is misleading and the abstract does not reflect the main points of the paper.

Lines 44-49: Why to focus on health issues? AERONET sun photometry main use is for climatic reasons.

Line 53: There is no way to measure mass concentration with AERONET sun photometry. Same critic for the statement in Line 68.

Lines 69-70: Dubovik et al., 2002 did not use linear depolarization ratio. The other studies focused more on lidar measurements.

Line 80: 'Depolarization ratio is essential to calculate ratio'. This is only true for lidar data. AERONET can calculate dust ratio using the AERONET inversion code

Line 116: Details of AERONET inversion code are not in Noh et al., 2017. Please correct.

Line 163: The big challenge is how to intercompare database that can depend on the variability of meteorological and local conditions.

Line 198: The mention to SSA is confused. It is obvious that aerosol types depend on SSA

Line 207: ‘In Europe, Southeast Asia, and Northeast Asia, pollution particles are the main type of aerosols ’. This is only related to the specific sites selected. There remote sites where this statement is not true.