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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "Depolarization Ratios Retrieved by AERONET Sun/Sky Radiometer Data and Comparison to Depolarization Ratios Measured With Lidar" *by* Youngmin Noh et al.

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We responded to all the comments by the reviewer. The criticism and suggestions by the reviewer were appropriate and improved the quality of our manuscript. We appreciate such efforts.

Authors' response to reviewers' comments

Paper No.: acp-2016-1181 Title: Depolarization Ratios Retrieved by AERONET Sun/Sky Radiometer Data and Comparison to Depolarization Ratios Measured With Lidar

Revision of the paper



Reviewer #2

Main comment: This manuscript compares the particle linear depolarization ratio retrieved by AERONET with respect to the one measured by lidar. To this aim, a column-integrated depolarization ratio is retrieved from the lidar depolarization ratio since the AERONET depolarization ratio is column-integrated. From my point of view, the main achievements are the good correlation between both depolarization ratios using a considerable database from different stations and the parameter 'dust ratio' derived from the AERONET depolarization ratio. This 'new' parameter which can be used as proxy of the present of dust in the atmospheric column. The presented work is really interesting and it is a good contribution to the scientific community. Therefore, I recommend its publication. However, the authors should consider the following comments:

Major comments: - Lidar depolarization measurements are used in this paper to validate the AERONET-derived depolarization ratio. Thus, the good lidar performance has to be demonstrated (it is used as reference!). However, only one line is dedicated to the technical specifications of the lidars (page 7 line 144) where two papers with more than 10 years are cited. The lidar depolarization technique had some advances in the last decade (from the 'polarization-dependent' of receiver transmission detected and corrected by Mattis et al., 2009 to the new theoretical framework and systematic-error estimation presented by Freudenthaler 2016; AMT and Bravo-Aranda et al., 2016; AMT). Thus, I suggest to include the depolarization calibration description in this paper highlighting the good performance. : As reviewer suggested, description of the depolarization ratio calibration has been included in the revised manuscript in the line from 218 - 234. "In order to obtain reliable depolarization ratios, the data of the lidar measurements must be calibrated before physical quantities such as the linear volume depolarization ratio can be retrieved. It is important to calibrate the signal intensities of the and first, before the linear volume depolarization ratio is calculated. Thet calibration method of the lidar system is explained in detail by Shimizu et al. (2017) and Nishizawa et al. (2017). The difference of the sensitivity between two PMTs that are used by the lidar system to detect these components is checked regularly by the following method. A sheet polarizer whose polarizing direction is set at 45° to the polarizing plane of the emitted light is inserted in front of the beam splitter cube, and the backscatter signal from the sky is recorded as a reference signal. In this reference record, the light intensities of the two channels are equal after the sheet polarizer, so the calibration constant can be obtained by comparing the recorded values of and . In the next step, the sheet polarizer is rotated by 90° which sets the polarizing angle at $-45°$, and another reference signal is recorded. Then relative calibration of and channels using signals measured for the polarizing angles at $\pm$ 45°. This pair of reference signals reduces any error caused by a poor positioning of the sheet polarizer (Freudenthaler et al., 2009; 2016). The reference signals are usually recorded once per year for each lidar (Shimizu et al., 2017)."

- It is stated that the lidar and AERONET depolarization ratio is well correlated. However, the correlation doesn't occur at the 'same' wavelength. Why? I miss discussion in this way. : The related discussion has been added in the revised manuscript in line 325 - 388. "We tried to find the reason for the comparably low correlation at 440 nm. For that reason, we retrieved the at 532 nm by interpolating the value of at 532 nm on the basis of the four AERONET wavelengths. In the next step the differences between and at 532 nm ( ) were calculated by deducting (at 532 nm) from (at 532 nm) for all 580 cases for which we have at the four AERONET sites. The values of were varied from 0.14 to -0.09. In the following step the data were sorted according to the differences of . In the final step we divided these differences into intervals of 0.02, i.e. 1: >0.12, 2: 0.10 - 0.12, 3: 0.08 - 0.10, ..., 11: -0.06 – (-0.08), 12: <(-0.08). Figure 5 shows the variation of the averaged at the five wavelengths and the values of at 532 nm divided by the differences of . The differences of between the wavelengths at 440 nm and 1020 nm are high. We find that decreasing of with increasing wavelength if the value of is low. The differences between at 440 nm and at 1020 nm become less for increasing interval number, i.e. for the interval number 7 (0 - 0.02); i.e. the yellow triangle pointing to the right . The value of at 532 nm shows lower values than at 1020 nm in those



intervals. The differences between at 532 nm and at 1020 nm are reduced as the is decreased up to the interval number 7 (0-0.02). We find an increasing of with increasing wavelength from the interval number 8 (0-(-0.02)) . The value of at 532 nm is larger than the value of at 1020 nm in the interval number 8 (0-(-0.02)). Also, the differences between at 532 nm and at 1020 nm as the interval number increased.. If we assume that the value of at 532 nm is close to real value of , the results in Figure 5 indicate that the at 440 nm has been retrieved to be higher value than the at 532 nm in the interval number from 1 (>0.12) to 7 (0-0.02) when the at 532 nm showed low values (less than 0.08). Conversely, when the high at 532 nm was measured, the at 440 nm showed a lower value than the at 532 nm. Figure 6 shows the average of volume particle size distributions of each interval data as separated in Figure 5. We see that the volume size distributions change from fine-mode dominated size distributions to coarse-mode dominated size distributions when the interval number moves from 1 (>0.12) to 12 (<(-0.08)). The important point of Figure 6 is the variation of the volume median radius (Rv). The volume median radius of the coarse (Rvc) and the fine (Rvf) mode shows a maximum value at the interval number 1 (>0.12). Rvf clearly and progressively decreases as the interval number moves from 1 (>0.12) to 12 (<(-0.08)). The Rvf of the interval number 1 (>0.12) is two time larger than the interval number of 12 (<(-0.08)) as 0.28 $\pm$ 0.03 $\mu$m and 0.13 $\pm$ 0.01 $\mu$m, respectively. Rvc also shows a pattern of decreasing values with decreasing values of . But it does not show as progressively as Rvf. Figures 5 and 6 show that the value of at 440 nm tends to be retrieved high for conditions where there is no dust at all or the dust concentration is low. Such conditions are usually dominated by a significant fine-mode of the particle size distribution. When dust particles contribute the main share to the particle concentration, i.e. high values of at 532 nm, the contribution of fine-mode particles is small. When particles in the fine-mode are the main contribution of the particle size distribution, i.e. low values of at 532 nm, the size of the particles in the fine-mode fraction are considered to have a large influence on the retrieval of the values of . This effect is considered to be more significant at 440 nm, i.e. at short wavelengths. Mamouri and Ansmann (2017) found that the value of is maximum at 532 nm and lower at 355 and 1064 nm because of the competing influence the fine-mode and coarse-mode dust fraction have on the overall values (fine + coarse) of at the three wavelengths. Haarig et al. (2017) found that on average the values of for aged Saharan dust were 0.25 at 355 nm, 0.31 at 532 nm, and 0.225 at 1064 nm. Müller et al. (2010; 2012) and Freudenthaler et al. (2009) also found spectral slope of the depolarization ratio with the maximum at 532 nm and lower values at 355 and 1064 nm during the Saharan Mineral Dust Experiment (SAMUM) 2006. The results clearly show a different pattern of the spectral variations of measured by lidar and retrieved from Sun/sky radiometer observations of dust. It is a striking result that at 1020 nm, unlike at 440 nm, is very similar to the values of at 532 nm. Though we cannot identify the reason for this similarity and even if the wavelengths (lidar at 532 nm and AERONET Sun/sky radiometer at 1020 nm) are different we may use the values of at 1020 nm as a qualitative indicator of the presence of mineral dust particles in the atmosphere. It remains open if we can use this parameter also as a qualitative measure of the mixing ratio of mineral dust and anthropogenic pollution particles compared to the more robust parameter (at 532 nm)."

- Page 24 line 509: 'There is considerable evidence of the coating of dust particles by absorbing fine-mode pollution particles in the East Asian region'. This sentence indicates that the 'internal mixing' is frequent(?) in the East Asian region but the presented method is based on external mixing. I miss some clarification in this regard. For example, May we say that the applicability of this method decreases with the 'flight time' of the transported dust layer? The sentence 'There is a higher possibility that pollutants can be mixed during long-range transport in case 2' seems to point in this way. Is there any way to distinguish each situation (internal Vs external)? : As reviewer commented, the discussion about "coating of dust particles by absorbing fine-mode pollution particle" can raise about internal mixing between dust and pollution particles. But, the method in this paper is based on external mixing. The problem is an important in the aerosol mixing study, but dealing with that problem in this paper seems to be able to get out of the subject of this study. For this reason, the expression that could



cause controversy was deleted.

- I strongly suggest to specify the wavelength when the depolarization from AERONET and from lidar is compared. For example, in page 28 line 594-595, the strongest correlation occurs between 1020 (AERONET) and 532(lidar) nm. This clarification is even more important in the summary/conclusion section since someone may directly read this section without paying attention to the rest of the manuscript, leading to misunderstandings. Page 29 line 618 is another example. : As reviewer commented, specifying of the wavelength at 1020 nm was added in the summary/conclusion section.

Minor comments: - Page 4 line 90: lidar.The →→ lidar. The

- Page 10 line 212: the particle linear depolarization ratio can be defined in different ways either perpendicular/parallel or perpendicular/total. I suggest to include a comment saying that both ways are interchangeable with a simple equation (Cairo, 1999). : As reviewer suggested, the sentence has been added to explain about interchangeable of the linear volume depolarization ratio in line 235 - 239. "A different definition of the ( ) is used in many other studies, e.g., Noh et al. (2016; 2013b) and Sakai et al. (2000). The term is defined differently from the definition we use for in our work. It is expressed by the following equation, derived from Eq. (3), see Cairo et al. (1999). (4)"

- Page 11 line 217: the same value of molecular depolarization means that the FWMH of the interference filter is the same one in all the lidars. Please, confirm. : The same interference filters were used for each lidar system. And molecular depolarization ratio was not 0.0044 but 0.014. The related sentences have been added in the revised manuscript.

- Page 11 line 223: did the author try to use the backscatter ratio instead of the backscatter? Might it be produce the same result? :The aerosol backscatter coefficient was used to calculate the weight factor. Also, if we used the backscatter ratio, the result must be the same with using of backscatter coefficient because only the ratio was applied in the calculation.

- Page 13 line 262: missed space between the symbol and the ‘at’. : It has been corrected in the revised manuscript.

- Page 13 line 273-274: the sentence indicates ‘cases with high ðİŻ£’ but is written ‘(<0.25)’. Should it be (>0.25)? Or 0.1<ðİŻ£<0.25? :It was typo. It has been corrected as >0.25 in the revised manuscript.

- Page 27 line 580: I think that ‘from 2.85 to 1.85 $\mu$m between group 1 and group 6’ is enough. : It has been corrected in the revised manuscript.

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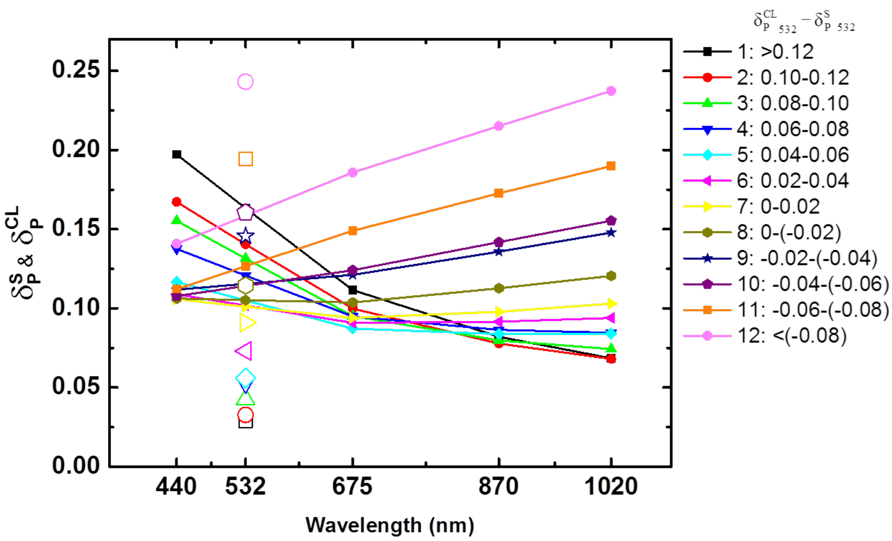
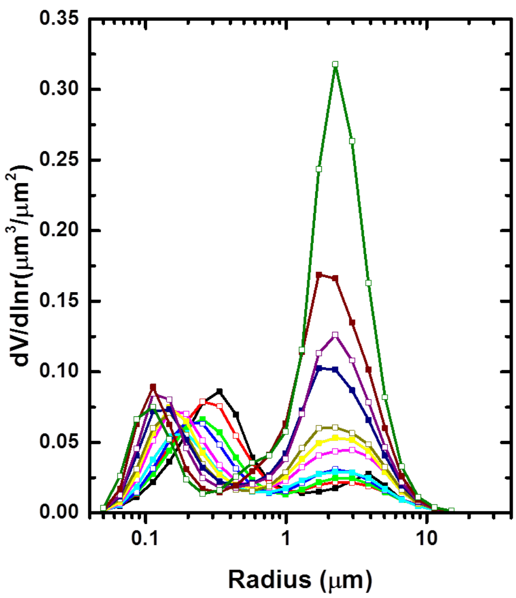



**Fig. 1.**




| $\delta^{CL}_{P\ 532} - \delta^{S}_{P\ 532}$ | $Rv_f$ | $Rv_c$ |
|---|---|---|
| 1: >0.12 | 0.28±0.03 | 2.83±0.49 |
| 2: 0.10-0.12 | 0.26±0.03 | 2.82±0.50 |
| 3: 0.08-0.10 | 0.24±0.03 | 2.71±0.38 |
| 4: 0.06-0.08 | 0.22±0.03 | 2.59±0.44 |
| 5: 0.04-0.06 | 0.19±0.03 | 2.44±0.45 |
| 6: 0.02-0.04 | 0.19±0.03 | 2.49±0.45 |
| 7: 0-0.02 | 0.17±0.03 | 2.38±0.38 |
| 8: 0-(-0.02) | 0.16±0.02 | 2.38±0.44 |
| 9: -0.02-(-0.04) | 0.16±0.02 | 2.27±0.49 |
| 10: -0.04-(-0.06) | 0.15±0.02 | 2.35±0.47 |
| 11: -0.06-(-0.08) | 0.14±0.01 | 2.12±0.34 |
| 12: <(-0.08) | 0.13±0.01 | 2.33±0.26 |

**Fig. 2.**