Review acp-2016-1181:

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.
- 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.
- 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)?
- 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.

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).
- 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.
- Page 11 line 223: did the author try to use the backscatter ratio instead of the backscatter? Might it be produce the same result?
- Page 13 line 262: missed space between the symbol and the 'at'.
- Page 13 line 273-274: the sentence indicates 'cases with high δ' but is written '(<0.25)'. Should it be (>0.25)? Or $0.1<\delta<0.25$?
- Page 27 line 580: I think that 'from 2.85 to 1.85 μm between group 1 and group 6' is enough.