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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of long-range transported Saharan dust at the Caribbean by dual-wavelength depolarization Raman lidar measurements" *by* S. Groß et al.

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The manuscript proposed by S. Groβ et al. reports on lidar measurements performed in the Caribbean Island of Barbados. The introduction states the difficulty of studying dust particles after long-range transport and underlines the complexity of involved atmospheric processes such as aging, mixing with other aerosols or nucleation to quote only a few. Facing such a complexity is indeed difficult while ideally lidar measurements should become standard tools for atmospheric studies. In this context, all my comments are intended to improve the science of this manuscript and help potential future readers.

On the methodology

The methodology proposed by S. Groβ et al. to analyze two-component particle mixtures is not the only existing one nor the pioneering one. As the manuscript may potentially have a large impact, other methodologies (Shimizu et al. JGR2004, Nishizawa et al. JGR2007, David et al. ACP2013) could be quoted, especially in a context where more than 45 % of the given references are from one of the co-authors.

On the measurements and the analysis

How is the PLDR-value and uncertainty of 0.27 $\pm$ 0.01 retrieved from the measurements? Looking at the observed vertical profiles, the uncertainty seems larger. Accordingly, how does the "highly accurate $\pm$45° calibration method" (page 19331, line 9) relate with other published methods (Alvarez et al. JTECH2006, David et al., APB2012) that rely on a dozen of points ?

In Figures 6, 8, 10, 12, 13 and 14, could you improve the PDLR-graph so that the reader may see the data points? Why is the PLDR not retrieved below 2 and above 4 km? It may be useful for the reader. In the same way, in Figures 5, 7 and 9, could you modify the δv color scale? I only see two colors while a 8-bins color scale is used.

In their assumed two-component mixture, S. Groβ et al. use "for the aerosol type separation 0.30 at 532 nm for dust and 0.02 for marine aerosols according to the findings for pure Saharan dust and marine aerosols (Freudenthaler, 2009, Groβ 2011b)". According to the observed variability in δp in the quoted papers, how is the 0.30 value chosen? Which value is used at 355 nm? To what extent do the corresponding uncertainties (at 355, 532 nm) modify your conclusions?

On the interpretation of the measurements

I disagree with the interpretation proposed by S. Groβ et al. on several points.

1: To interpret their lidar measurements, S. Groβ et al. assume a two-component particle mixture (page 19331, line 20). In the Caribbean, I would rather expect a three-

component particle mixture, with water-soluble, sea-salt and dust particles to be more realistic: some back-trajectories (like the blueish) fall very close to sea level. While sea-salt particles can be found up to the tropopause (Ikegami 1994), could you discuss on your assumption of a two-component mixture? Reference literature to three-component particle mixtures (Sugimoto et al. Atm. Res.2010, David et al. ACP2013) are not quoted, while they may interest potential future readers. How do you discriminate non-spherical dust from sea-salt particles, which are non-spherical below 40 % relative humidity? This remark is important because aerosols are then classified at the end of the manuscript.

2: As detailed in the introduction, reference literature exist showing that δp is modified for an aged Saharan dust plume (Wiegner et al. 2011) compared to values measured in fresh Saharan dust plumes (Groβ et al. 2011b). Additionally, aerosol lifetime (Amiridis 2009) modifies the lidar ratio. However, in this manuscript, S. Groβ et al. arrive to the opposite conclusion. How do you explain this difference? Is your conclusion a general established fact or a specific particular case? I do not understand the comparison of the SALTRACE experiment in the Caribbean with former experiments performed in Munich and during SAMUM campaigns a few years before. Could you assume that the source regions are the same, and if they are, that the activity of these sources is the same? On this point, I agree with Reviewer #1.

3: I have some concerns with the classification proposed by S. Groβ et al.

i) In the classification scheme, the plotted quantities (i.e. δp and Sp) are representative of the particles mixture and hence not specific to one type of particles (see Miffre et al. GRL2011 for the difference between δp and δns). δp is not a tracer for nonspherical particles (David et al., ACP2013). A "pure case" is however often reported in the manuscript (in the abstract and in the text). Could you provide evidence of a "pure case" here? Can we find "pure case" in the atmosphere? The coauthors have already used this terminology but it has never been defined in terms of chemistry and particles content. Optical devices are not sensitive enough to claim the existence of pure cases

(one compound).

ii) Above all, though δp and Sp are intensive, a great variability can be observed in Sp and δp just by varying the size, the shape or the chemical composition. As published by M. Kahnert (JQSRT, 2015), a prerequisite for a potential classification scheme is that the variation in δp among particles in the same type is small compared to differences in δp among particles of different types and according to the state-of-the art literature, this is not a given. How do you account for this publication?

4: S. Groβ et al. conclude that the intensive properties do not vary over several thousands of kilometers. Literature Reference (Ridley et al. ACP2013) indeed shows that the particle size distribution is modified during transport "Dust particle size showed a weak exponential relationship to dust age. Two cases of freshly uplifted dust showed quite different characteristics of size distribution". Following the link that exists between δp and the particles size, the particle depolarization is then modified. How do you account for this remark in regards to your conclusion that δp remains constant?

By looking at Figure 14 in detail, a potential reader may wonder if this conclusion results from the atmosphere or from the lack of precision or/and sensitivity of the experiment. Could make some comment on this?

One specific comment

Page 19340, line 13: S. Groβ et al. wrote that "It has been shown that the lidar ratio and the particle depolarization ratio are quite different for different types of aerosols" and quoted reference to Sakai et al. (2010) to justify this statement. In their paper, Sakaï et al. only addressed the particle depolarization and nothing is said about the lidar ratio. Could you provide another reference?

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