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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Observations of the spectral dependence of particle depolarization ratio of aerosols using NASA Langley airborne High Spectral Resolution Lidar" *by* S. P. Burton et al.

Anonymous Referee #1

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General

This is an exciting contribution to the lidar literature. For the first time, airborne polarization lidar measurements are performed simultaneously at three wavelengths. Several case studies are presented and corroborate that high quality observations with a unqiue lidar setup could be realized.

However several points must be improved. The smoke case study triggers many questions that must be checked and answered.

Minor revisions will further improve the paper.

Detailed comments:

Page 24754, second paragraph:

The introduction should be improved. In such an important paper, the field of aerosol depolarization studies must be better reviewed. References to the work of Japanese groups (Sugimoto, Nishizawa and further papers) must be given. The milestone-like work of the SAMUM and SALTRACE groups should be cited (Freudenthaler, Gross, and Tesche papers in Tellus 2009 and 2011). These efforts including the use of the measured particle linear depolarization ratio to separate dust from non-dust aerosol components (Tesche JGR 2009, Ansmann JGR 2011, ACP 2012, Mamouri-Ansmann, AMT 2014) pushed the depolarization lidar work really forward and offered many new opportunities for important quantitative aerosol studies. CALIPSO and EarthCARE benefit from all this basic work (e.g., via Amiridis, ACP 2013). A nice and well balanced introduction increases the importance of the next large step presented in this paper (three wavelength depolarization lidar). Do not hesitate to cite your own recent papers (Burton in ACP and AMT, 2012, 2013, 2014) together with the Gross papers (2011, 2013, 2015…). All these aerosol-typing papers are important contributions to the field and should show up in a compact brief introduction.

I understand that a paper from a NASA group wants to put all the spaceborne lidar activities into the center of interest. But polarization lidar is clearly a stand-alone science field and offers exciting possibilities. So, at least in the second paragraph, one should provide a brief introduction into the field of polarization lidar for aerosol studies (not for cloud studies, that is not necessary here).

It should also be mentioned, that there is another group that does these simultaneous three-wavelength depolarization measurements (27 ILRC paper, Haarig et al.). The Leipzig group gave a presentation at the ILRC conference at New York and showed many desert dust cases from their Barbados SALTRACE campaigns which seem to match very well with your results if I remember all the numbers right. Maybe it is possible to compare these results, but I am not sure whether an extended ILRC paper is available or not. Usually they publish 4-page papers in the conference proceedings.

Page 24755, second paragraph:

Regarding smoke and dust separation with polarization lidar, I like this Tesche paper in Tellus 2011 very much. They distinguished African biomass burning smoke from desert dust and came finally even up with single scattering albedo values for the smoke part. That paper should also be included in the references here.

Page 24756, Sect. 2, Instrumentation:

Can we have detailed information on the aircraft flight heights a.s.l. for all the case studies discussed later on? I have strong doubts that this strange smoke case with very large 355 nm depolarization ratio is based on good signals. I speculate that overlap or overloading problems may have caused these strange results. I will come to this point later again.

Page 24759, Eq.(2), please provide a reference for the equation

Page 24761, Sect.3.1,

Line 22, particle depolarization ratios of 30.4% indicate desert dust backscatter fraction of only 80-90%? I would say the fraction is then close to 100%.

It would be really nice if we can have a full set of profiles of all the retrieved optical properties, maybe based on 10 sec or 30 sec signal averages or what ever is appropriate in case of aircraft HSRL observations. Then we could best compare your results with other publications in this field. I would recommend: left plot: profiles of particle backscatter at all three wavelengths, center left: profiles of particle extinction coefficient at both wavelength, center right: profiles of lidar ratio at both wavelength, right panel: particle linear depol ratio at all three wavelength. Profiles showing the depol ratio and the lidar ratio together provide the essential basis of all the aerosol typing

studies (which includes your own paper Burton et al, AMT,2012, but also Gross et al., 2013 and 2015, even in Illingworth et al., EarthCARE paper in BAMS, 2015, such a plot with lidar ratio versus depol ratio is given).

Page 24763, Sect. 3.2

Line 17, The AOT of the layer on 8 Feb is just 0.02 at 532 nm, and you can still have good particle depol ratios? The 1064nm backscatter and the 532-1064nm Angstrom exponent should at least be rather uncertain. Sakai et al (Appl. Opt 2010) found values close to 39% depol for 'pure' coarse-mode dust. So your results seem to be in agreement with these measurements.

Page 24765, line 22, please mention here also Sakai et al. (2010)

Page 24766, last two paragraphs of Sect.3.2:

Simulation studies are at all not just trustworthy and should therefore be interpreted with caution The unknown shape characteristics does not allow proper conclusions from simulations of the wavelength dependence of particle linear depol ratio.

Page 24767, Sect 4.:

You observed a smoke plume at 8 km, and the aircraft was a bit higher. Then you should have an impact of the overlap problem on your measurements. Can you exclude such an impact caused by slightly different beam pathes from the telescope to the photomultipliers of the cross and co-polarized channels. Maybe you made tests at ground and found an almost height –independent depolarization ratio throughout a well mixed PBL and this down to very small ranges to your lidar?

I do not believe this value of 25% depol at 355nm when, at the same time, the other wavelengths show depol ratios below of less than 10 or even less than 3%. There must be something wrong with the 355 nm signals. Are you sure that the signals were well aligned, no overlap problems. Such a strong wavelength dependence between 355 and 532 nm has never been observed). And there are several 355/532nm lidars available now and produced a lot of 355/532 nm depol ratio observations.

Simulation studies do not help because of the always unknown shape characteristics.

The layer is about 300m in depth and the AOT was estimated to be 0.05 in the green. So it should be possible to compute extinction coefficients and lidar ratios for 355 and 532nm. Based on this optical data set one may be in a better position to discuss this strange observation. . .

Page 24769, line 11-28, please include in this discussion (soil dust injection during fire events) the paper of Nisantzi et al., (ACP, 2014).

Nisantzi, A., Mamouri, R. E., Ansmann, A., and Hadjimitsis, D.: Injection of mineral dust into the free troposphere during fire events observed with polarization lidar at Limassol, Cyprus, Atmos. Chem. Phys., 14, 12155-12165, doi:10.5194/acp-14-12155-2014, 2014.

Page 24770, simulations: All the simulations with a spheroidal dust shape model are not trustworthy. The results must be handled with care. Especially the spectral dependence of the depol ratio is rather erroneous.

Page 24771, I am not a friend of speculations as given on this page. Figure 15 is simply useless. I would just remove all this, but leave it open to the authors what to do with this strange case study and all the simulation-based discussions.

Sect. 5, Summary, second half of this Sect. 5: You make a dangerous conclusion concerning the 355 nm depolarization ratio (EarthCARE) . When looking into the literature (for example Illingworth, 2015, depol vs lidar ratio figure) then there is no doubt that 355 nm depol observations can indeed by used for aerosol typing. Dangerous means here: Your statement is based on the fact that you only observed depol values from 20-25% at 355 nm with your HSRL, disregarding what type of aerosol was present. So these measurements are at least to some extend questionable (at least for me) so that such severe conclusions are not justified, . . . is my opinion. Sure if this is found by

many groups in future, yes then we have to change our mind, but at the moment, one should better leave out such statement regarding EarthCARE. This brings me to the question: Did you ever observe particle depolarization ratios (in aerosol layers) at 355 nm clearly below 10%?

Final remark: Appenix, Page 24776. line 14 Do you observe any wavelength dependence in the ellipticity angle? If yes, please provide the numbers for the different wavelengths.

Table 1: Lidar ratios at 355 and 532 nm in addition would be fine.

Figure 3: There is space to the right. . ... for two more color plots (355nm and 532nm lidar ratios). At least for this excellent dust observation, we need to bring together all the information, we have and on which all the aerosol-typing papers are based on.

As mentioned I would like to have an additional four-panel figure: height profiles of backscatter (three wavelengths), extinction (two wavelengths), lidar ratio (two wavelengths), depolarization ratio (three wavelengths).

Figure 10: smoke is practically invisble, can you use arrows or something else to point to the smoke fields. . ..?

Figure 13: I am still wondering what the reason for the strange observation is ? Overload in the co-polarized 355nm channel, could be an explanation?

All in all, the paper is very good and it was fun to study it! I know the group and know that the lidar is excellent, and the data analysis is carefully done by an experienced scientist (professional). So my comments are just to trigger to re-think and to re-check some of the results and to keep the discussion on the very save side . . ..

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 24751, 2015.