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Discussions

Interactive comment on "Experimental techniques for the calibration of lidar depolarization channels in EARLINET" *by* Livio Belegante et al.

Anonymous Referee #2

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The paper surveys different procedures for calibration of lidar systems to improve the accuracy of depolarization measurements, which are of primary importance to infer aerosol particle shape, hence typology. It briefly introduces the formalism to be used in light polarization measurements and reviews the current calibration techniques, describing relative merits and drawbacks. Then present results from the application of such calibration techniques on some case studies.

There are many paper dealing with the calibration of polarization diversity lidars, but the originality of the present one resides in its cut, more oriented to the description and practical implementation of the calibration systems, than in their theoretical description. This, in addition to a praiseworthy review of the theoretical assumptions of existing calibrations, assures it of the interest of the community, and for this reason I believe

that the work deserves the publication.

There are however a few issues the authors may want to further clarify, which I detail below, together with some minor items.

(2,8) typo "... about to..."

(2,20) "... distinguish between rather spherical particles with low depolarization ratio, and non-spherical particles with higher depolarization ratios." I would here drop the adverb "rather", as the whole sentence tends to suggest (at least to me) a univocal relationship between polarization and a "degree of asphericity", which is misleading, as it has been proved, for instance with theoretical T-matrix computations, that particles that are "rather" but not exactly spherical (i.e. prolate or oblate spheroid with aspect ratio close to unity) may have values of depolarization higher than considerably "more aspherical" (i.e. with aspect ratio much different than one) particles.

(2,22) "...low depolarizing (e. g. local aerosol) ...", well this claims depends on where you lidar is placed, that in turn dictates what is to be considered "local". I guess that a scientist working in Tamanrasset would have different views on what to consider "local". So you may consider to change "local", to "urban aerosol", as instance?

(4,19) A polarization purity of 95% is definitely a problem, and this should be stressed (actually is quite pessimistic, but even a more common 100:1 polarization purity still is a problem). Here you can quote that the residual non polarized laser light can be easily filtered out. It is said thereafter but I think the best place to pose that remark is here.

(7,12) please use "responsivities" instead of "quantum efficiencies", as the latter is only a factor of the former. This has an impact in what follows.

Formula (19): this is basically the ratio of the overall photodetector responsivities for the two channels. What follow is my crucial remark, and I would like the authors to discuss it in some more length. The responsivity, or the "gain" of a detector, is the ratio between the power input (in our case the photon flux) and output, (the current, or photoelectron

rate). One would like this gain to be constant, i.e. the idealized detector should have an output which is linearly related to the input. Unfortunately, this is seldom the case for the PMTs and APDs, as the gain may be dependent from the level of the input (this claim is straightforward in the photoncounting acquisition mode, due to "dead time" counting effects, but it is also true in current acquisition mode). This makes the ratio in (19) possibly dependent on the measurement conditions, i.e the altitude at which this ratio is computed (this is somehow implicitly – too much implicitly - addressed in fig 7 a-b) and, in some cases more important, on the level of sky background. It may well be that for "EARLINET-like" systems, which often use high power, low pulse repetition rate lasers, and very narrow interferential filters to reduce background to levels much lower than the actual signal, this effect is not apparent; but in general, and especially for systems with larger spectral bandwidth and low power, high pulse repetition rate lasers, this may be an issue. This is an issue which, to my knowledge, has never been addressed in any study putting forward the merit of calibrations others than the "0° calibration", and I think it is worthwhile to mention it.

(8,18-19) The sentence is unclear and should be rephrased.

(16,18-20) I am somehow uncomfortable with the whole sec. 4.4. I guess everyone is already well aware of "... the importance of calibrated depolarization lidar products...". This is not the main goal of the paper, but rather to discuss at length the different calibrations; hence it would have been much more interesting to show what is the effect of these calibration procedures, i.e. to show uncorrected vs corrected profiles, which I think is a display much more in line with the rest of the paper. Therefore, I would ask the authors to do that.

(16,24) I understand that the presentation of particle depolarization is functional to the aim of showing "... the importance of..." (see above), but again I think this is not the main message the paper is delivering. Moreover, as correctly stated, the computation of particle depolarization is affected by uncertainties on the aerosol backscatter coefficient and this is especially true in the case of low aerosol loading, as correctly stated in

(17, 14-16). This should open a completely new and wide discussion which is clearly beyond the scope of the article, which is on calibration procedures: the effect of these may be heavily masked by other sources of inaccuracies in the computation of particle depolarization.

In synthesis, I would ask to rewrite sec. 4.4, presenting calibrated and uncalibrated profiles on selected case studies, or to drop it entirely.

(18,24-25) This is a very nice result which I think is understated, as the comparison of the observations in regions supposedly free of aerosol, with the theoretical values of the molecular depolarization is the key factor to assess the goodness of the calibration procedures hereby described. The authors may consider to add a table reporting the values of "low aerosol height range values", vs the molecular depolarization as expected from theory. The bandwidth of the interferential filter should of course be also quoted, as it impacts that value. Incidentally, it might be quoted (18,27) that also the presence of small amount of liquid aerosol may impact the profile, in a different direction and to a smaller extent.

I think this is a very nice paper, and I would like to see it published.

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Discussion paper