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Atmospheric Measurement Techniques Discussions

Interactive comment on "Assessment of lidar depolarization uncertainty by means of a polarimetric lidar simulator" *by* J. A. Bravo-Aranda et al.

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In the elastic lidar technique, the assessment of the degree of depolarization induced by particle backscattering of polarized light, is a key factor to discriminate between different particle shapes. Unfortunately, its evaluation may suffer from inaccuracy, strongly depending on the lidar system setup. Thus, this interesting study on the sensitivity and accuracy of lidar depolarization measurements is contributing to define common procedures of lidar calibration, and surely deserve publication on the journal. Its aim is a quantification of the volume linear depolarization ratio (a common parameter measured by lidars) uncertainty due to systematic errors. It also presents a software tool (Polarimetric Lidar Simulator (PLS) applied to synthetic as well as real li-

dar systems to quantify the uncertainties and inaccuracies induced by the various lidar subsystems. The main outcome of the paper is the identification of which instrumental parameters need more accurate characterization and it then helps giving guidance in the development of new lidar systems with better performance in depolarization measurements, and in the standardization of their products. I recommend its publication. However, there are a some minor issues that I would like the author to look at.

In the following, (page, lines).

(3,13) To my knowledge, laser commonly used in lidar practice are often guaranteed with linear polarization not better than 100:1, so, as reported, sometimes a polarizing cube is used to further purify the laser light polarization before the transmission into the atmosphere. However, this does not prevent problems arising from possible misalignments between the laser polarizing plane and the polarizing splitter incident plane. Maybe it is worthwhile noting that the two effects are inherently different, and can in principle be corrected differently, in one case by further filtering the laser light to remove the unpolarized residuals, in the other, by a proper alignment of the two (polarization and incident) planes. However, the authors' formalism is correct, and general.

(7,5-6) This claim should be substantiated or referenced.

(7,18-20) What follows is the crucial point of my review. I think that the sentence reported in the text understates what, to my opinion, is one cause of concern about the accuracy of all the absolute calibration techniques of the lidar signal which have been proposed so far. The reference to Bravo-Aranda et al., refers to the stability of photomultiplier gains over long times, but I think it is not sufficient to guarantee that. I hope that, if I behave myself, after my departure I will find myself in a place where the sensors' responses to signals are linear along their entire dynamic range. Unfortunately, it is well known that photomultipliers are far from heaven, both when used in photon-counting mode, or in current mode. The lidar return may be a more or less significant part of the total signal detected by the photomultiplier, depending on the altitude where it originates and, more significantly, on the sky background that can vary over several orders of magnitude. This means that the whole lidar returns are located on different portions of the photomultiplier response curve, in dependence of the sky background. These different portions can be locally linear, or quasi-linear, but may not share the same linearity. In other words, depending on the sky background, the photoimpulse height spectrum of a photomultiplier can change substantially, thus affecting both photoncounting and current mode of detection. This effect may be dramatic or negligible, depending on the photomultiplier type, polarization, single realization of the device, and so on. Of course, if the effect is there, it has an impact on the absolute calibration, that became dependent on the sky background conditions. I am not aware of any study that focused on the dependency of the absolute calibration on sky background conditions, but in my experience as a researcher, I saw changes of volume depolarization values by few percents, simply induced by the sun rising or setting, so I am quite sure this effect can be present, even if it can be reduced or suppressed by an accurate choice of the photomultiplier type, polarization and amplification circuitry, lidar spectral bandwith and so on. I am not saying that this effect is spoiling the results of this study, or the whole absolute calibration procedures. What I am saying is that the assumption of a constant photomultiplier gain is quite a severe one, and should be acknowledged as that.

(9,5) The relationship between depolarization ratio and particle “asphericity” (whatever that means) is not so straightforward. Even under the simplified assumption of particles as oblate or prolate spheroids - unrealistic, but widely used because it allows analytical expression for the scattering equations solution - one could find greater depolarization ratio for aspect ratioes close to unity. The authors might drop that sentence, or quote some reference to T-matrix computations, as instance.

(12,6-7) “do not use laser emitting optics if possible” please rephrase, as in this form, it is not clear what it is meant, at first sight.

(16,3) It may be worthwhile to note here that the opposite result applies when a rela-

tive calibration approach is pursued, i.e. when the theoretical value of the molecular depolarization is imposed in a region of the lidar profile which is free of aerosol. In that case, the instrumental effects here discussed lead to an underestimation of the aerosol depolarization.

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