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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Aerosol particle depolarization ratio at 1565 nm measured with a Halo Doppler lidar" *by* Ville Vakkari et al.

Anonymous Referee #1

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The authors present a new technique that enables depolarization measurements from a Halo Doppler lidar. The new product falls at 1565nm, an unexplored wavelength until now. Consequently, the scientific impact is rather high, while possible spread of the technique may increase the information from the Halo systems operating worldwide. The manuscript is also written in a clear way. My main criticism lies in the cloud calibration method that has been selected for the analysis as it poses many risks that are not properly addressed in the manuscript. I would recommend this manuscript for publication after some revisions.

————General Comments————

Lines 128-141: Cloud calibration has the following risks:

1) The co polar signal can be saturated, especially for low clouds. Then, the ratio in the cloud base is not reliable anymore. 2) Vertical filtering/smoothing usually generates artifacts near the cloud base, especially for sharp low clouds. Are the profiles that were used for calibration smoothed? If yes, what kind of filter has been applied 3) There is always the multiple scattering issue that is discussed in the manuscript. Reducing the FOV by e.g. reducing the field stop radius if possible can reduce multiple scattering effects. 4) The measurements are not simultaneous! For aerosol variability 7 seconds are not important but for clouds a lot can change. This could negatively affect the ratios. Some information is provided in lines 136-138. What time scales are applied in the high time resolution data.

How did the authors deal with these issues?

Concerning the calibration, as stated above, using the same detector for both co and cross signals has the benefit that the ratio of the co and cross attenuated backscatter profiles gives the volume linear depolarization ratio (VLDR) directly. However, it seems that the authors use the SNR instead of the signals. In that case, to my understanding, the ratio of the 2 SNR is no longer the VLDR. Why not use the attenuated backscatter ratio directly for the volume linear depolarization ratio calculation?

There is always the case that depolarizing effects are introduced by the receiver and by the laser (depending on the emission purity) and the emission optics, but these can be accounted for with the GHK formalism introduced by Freudenthaler et al 2016.

The polarizer bleed through can be calculated in the laboratory. In addition, such values are usually provided by the manufacturers. Then, this effect can be taken into account into the K parameter of the GHK formalism (see Freudenthaler et al. 2016).

Finally, is the molecular atmosphere observable with HALO? If yes, the VLDR ratio in the molecular region should agree with the theoretical molecular VLDR at 1565nm with the respect to the FWHM around the mainline that is collected by the detector (see Behrend et al. 2002). Is it possible to perform such a test? The GHK correction is then applied to correct any offsets.

————Comments————

Lines 54: It has been recently shown (Gialitaki et al 2020) that soot aggregates can assume the near spherical shape. In their study they present the depolarization ratio values that are expected per wavelength.

Line 91: Strong H2O absorption takes place in the spectral region near 1565nm. Does this specific wavelength fall in a low absorption region? Are there any H2O vertical extinction corrections required?

Lines 95-96: A single channel has been used for the depolarization measurements. The benefit of this setup is that it does not require a calibration factor. However, is is difficult to achieve similar order of magnitude co and cross polarized signals. This can result to non linear amplification in the detection since the detector might not operate optimally for such a demanding dynamic range. In 2 channel setups a neutral density filter is used in front of the co polarized channel to bring its naturally higher intensity to the crossed polarized levels. Have the authors checked for effects in their setup (e.g. saturation and/or clipping in the co signal or increased noise due to a weak cross signal).

Lines 112-115: The authors should provide more information and references on how the SNR is calculated. Is it the raw signal divided by a constant noise level? Is the noise vertically resolved?

Line 115 'by averaging the SNR': Is the SNR averaged or the signal in order to reduce noise and increase the SNR?

Lines 117-120: A US standard atmosphere model is preferred here. This can introduce uncertainties in the retrievals The use of a dedicated radiosonde or a meteorological model is a much safer approach. Did the author compare the retrievals using the US standard atmosphere with a radiosonde?

Figure 2: A figure with the SNR (or signal) with the same vertical scale as the VLDR plot is missing here. The vertical scale of the left part of figure 2 makes it difficult to compare the two plots.

General figure comment: Please specify whether the depolarization ratio is the particle linear depolarization ratio or the volume linear depolarization ratio.

————Recommendations————

In order to optimize the VLDR retrievals a GHK correction can be applied. Optimally, the authors should measure the purity of their emission and also perform a Delta 90 calibration with a rotator or a rotating linear polarizer in front of their receiver box, if possible, to measure any diattenuation and/or retardation effects coming from the receiver. Then they can apply the measured properties to calculate the GHK parameters and then, obtain the corrected VLDR profile from the signal ratio.

Section 3.4 It would be interesting to take also into account the modeled wavelength dependence of such aerosols, at least for dust. The latest version of the OPAC database (Koepke et al. 2015) includes non spherical dust particles in three modes (ultra fine, coagulation, and coarse). The wavelength dependence is different depending on the size mode. This could be taken into account in the discussion here. Similar databases like MOPSMAP (Gasteiger et al. 2018) could be also taken into account.

————References————

– Behrendt, A. & Nakamura, T. Calculation of the calibration constant of polarization lidar and its dependency on atmospheric temperature Opt. Express, OSA, 2002, 10, 805-817

– Freudenthaler, V. About the effects of polarising optics on lidar signals and the Δ 90Âăcalibration Atmospheric Measurement Techniques, 2016, 9, 4181-4255

– Koepke, P.; Gasteiger, J. & Hess, M. Technical Note: Optical properties of desert aerosol with non-spherical mineral particles: data incorporated to OPAC Atmospheric

Chemistry and Physics, 2015, 15, 5947-5956

– Gasteiger, J. & Wiegner, M. MOPSMAP v1.0: a versatile tool for the modeling of aerosol optical properties Geoscientific Model Development, 2018, 11, 2739-2762

– Gialitaki, A.; Tsekeri, A.; Amiridis, V.; Ceolato, R.; Paulien, L.; Kampouri, A.; Gkikas, A.; Solomos, S.; Marinou, E.; Haarig, M.; Baars, H.; Ansmann, A.; Lapyonok, T.; Lopatin, A.; Dubovik, O.; Groß, S.; Wirth, M.; Tsichla, M.; Tsikoudi, I. & Balis, D. Is the near-spherical shape the "new black" for smoke? Atmospheric Chemistry and Physics, 2020, 20, 14005-14021

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