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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of particle size and shape on the backscattering linear depolarisation ratio of small ice crystals – cloud chamber measurements in the context of contrail and cirrus microphysics" *by* M. Schnaiter et al.

Anonymous Referee #2

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The paper is on measurements of the particle linear depolarization ratio δp of ice crystals performed with SIMONE as part of the AIDA cloud chamber. This quantity is derived for the backscatter direction (as close to 180 degrees as possible) to provide information for lidar remote sensing. The corresponding δp were measured for ice particles having undergone different nucleation processes (different condensation nuclei, different temperatures). The paper describes the measurement setup, the theoretical basis of the description of depolarization, and the evaluation/interpretation of the data. The paper is well written, clearly structured and the results are interesting. From this

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point of view the paper can be published.

However, minor revisions might improve the paper; a few suggestions are given below. As a general comment I want to state that the paper is sort of lengthy – there are information provided, that are well known and must not be repeated. This is in particular true for the part describing the formalism of the depolarization ratio: in the present state it is more confusing than helping because of the inconsistent nomenclature (a lot of different "depolarization ratios" are introduced, which is neither necessary not as exact as it should be; by the way, lidar researchers should know about this stuff). Moreover, the extensive description of the conduction of the nucleation experiments can be explained once (for the lidar people), but maybe not three times.

Few more comments:

15454/4: 488 nm is not a lidar wavelength. Thus, the results must be extrapolated to 532 nm, 355 nm or even 1064 nm. A comment on this fact and the consequences should be added. In case of aerosols there is a wavelength dependence.

15454/8 and throughout the paper: LIDAR should not be capitalized (in the 21. Century it can be treated as a word, not as an acronym).

15456/8: what is the linear depolarization ratio of CALIPSO: the “particle linear depolarization ratio” or the “volume linear depolarization ratio”? In case of cirrus clouds the difference certainly is small nevertheless, the manuscript must be precise. This applies to the whole text, especially when measurements are compared that concern different “depolarization ratios”!

15456/20ff: the examples mentioned here all show very large values inconsistent with the results from SIMONE shown later in this study. So, why are these references cited, or what is the reason for the discrepancy?

15456/24: “the knowledge of the link. . .”: this is indeed a crucial point, note that the variety of “real” ice crystals is much larger than discussed in this manuscript. Moreover,

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the orientation of the crystals influences the lidar signals. Thus, this paper can give some (useful) information but will not provide the missing link.

15459/23: δp δH δV how are they related? What is the relevance of the latter two for lidar measurements?

15461/15: Figure 1 is explained after Figs. 2-5; thus, the order of the figures should be changed; or rearrange the text.

15464: δLIDAR: this is the fourth or fifth "depolarization ratio" in the text. Sij and k are not explained here (only two pages later). Please check, how the whole section 3.1 can be reduced to those parts that are really required for the understanding of the data evaluation and the link to the lidar measurements.

15465/10: the authors assume (among others) cylindrical particles. A comment why hexagonal forms are not considered must be added.

15466/14: If virtually all details are explicitly written as equations, a formula for "b" should be given for reasons of "homogeneity". Or reduce the whole formalism (see above).

15468/3ff and rest of the paper: I am not sure whether it is necessary to explain in detail how the freezing-experiment was done – at least from the lidar-point of view, this is of minor importance. This is in particular true as this type of information is repeated for each subsection.

15475/22ff: It should be stated, that this fact is well known (see 15477/9ff); not a new finding of this paper. It is not necessary to repeat this a third time on 15481/25ff.

15477/28: "ellipsoidal scattering pattern": this cannot be understood here. The reference to the subsequent section is correct but does not help the reader. I recommend to skip this sentence here; it is sufficient to mention the scattering pattern where it is discussed (the authors often mention something which is only explained much later – this could confuse the reader).

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15480/15: “This means. . .”. I don’t understand this conclusion, in particular, as most (or all) lidars do not change the polarization of the emitted radiation (as radars do). So, what is the message?

15480/11ff: in Fig 13 (lower right) the differences of S22/S11 for 178 and 180 degrees is discussed. As the quantity of interest is δp the authors should show this difference (can easily be calculated from Eq. 15). Only then it is possible to directly see the possible errors of δp due to the angular extrapolation towards 180 degrees, if the value at 178 is used (according to Fig. 13 this difference can be large). Why is the small difference between δH and δV (see 15480/15) an indication that the depolarization ratio at 178 can be used for 180 degrees? This issue is relevant for any lidar application; thus, the arguments should be convincing.

15480/22: When discussing real lidar measurements, the presence of large ice crystals and the implications for δp should be briefly addressed.

15481/28: The relevance of hexagonal particles and the modeling of their optical properties have already been shown many years ago, e.g. Hess and Wiegner (1994; Applied Optics), who provided a data base.

15482/12: “absolute backscattering linear depolarization ratio”: one more depolarization ratio. Please homogenize the wording and the nomenclature.

15483/7: What is the reason for choosing the FDTD and not DDA. Are the particles too small for the Geometrical Optics Approximation?

Conclusions/results: The authors should think about a summarizing table including the most relevant findings (lidar relevant optical properties): nucleation process/particle size/temperature/δp. This would be more helpful than the information given in Tab. 1.

Fig 13: the figure caption of the lower right panel and the legend do not agree.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 15453, 2012.