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Atmospheric Measurement Techniques Discussions

Interactive comment on "Scanning Polarization Lidar LOSA-M3: Opportunity for Research of Crystalline Particle Orientation in the Clouds of Upper Layers" *by* Grigorii P. Kokhanenko et al.

Anonymous Referee #1

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The authors describe a two wavelength (532 and 1064 nm) depolarization (circular and linear) scanning lidar designed for detecting and characterizing oriented ice crystals. The instrument design is provided along with some information on the calibration processes. They then present two example cases where oriented ice crystals are observed. Finally they show some analysis on the width of specular reflections of (presumably) plates which is slightly different between the two wavelength channels.

The work presented here is sound and novel. The manuscript represents a complete and well rounded effort. The results will certainly make unique and interesting contributions to the field of atmospheric science. I believe this work should be published after

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Discussion paper

revisions. A few of these revisions are minor but absolutely mandatory. Others I believe will improve the quality of the presentation but are at the discretion of the authors and editor. Finally I have included several points that I think should be clarified.

Mandatory Edits:

Pg 2, Line 51 “Another effect caused by the horizontally oriented columns is the corner reflection when the lidar is tilted at 30 deg. . .” The 30 deg corner reflection comes from plates, not columns. Columns also have a corner reflection but it is closer to 60 degrees. This is noted in A. Borovoi, I. Grishin, E. Naats, and U. Oppel, “Backscattering peak of hexagonal ice columns and plates,” Opt. Lett. 25(18), 1388–1390 (2000).

Pg 2, Line 54. In this paragraph the authors cite several works stating that these works observed both horizontal orientation and azimuthal orientation. This is not true. Most of the references make no mention of observing azimuthally oriented ice crystals which implicitly seems to suggest they didn’t observe any. There are a few works (such as Kaul 2004 and Balin 2011) that do mention observing this effect. Beyond that I happen to know number of the researchers cited are very skeptical about the existence of of azimuthally oriented ice crystals outside of thunderstorms. I doubt they would appreciate being cited in support of this claim. The authors need to accurately represent the results of prior work and most of the citations used here do not support the statement or even contradict the statement.

Discretionary Edits:

At a number of points the authors draw conclusions about their observations (for example the paragraph on Pg. 8 starting on line 235). The authors should be careful about overasserting what their observations definitively prove about the scattering volume. Hayman 2014 notes in three bullets the difference between what is physically present in the cloud and what is actually observable. Similar conditions would apply to this system. For example, the lack of variation in backscatter with angle does not rule out HOIC. There are cases where HOIC might not generate this signal. For ex-

ample, HOIC columns probably would not generate the same variation in backscatter, especially over such a narrow range of scan angles. Instead, it seems to rule out the presence of pristine plates. In that context, the authors later assert that the depolarization values are indicators for the relative mass of oriented and randomly oriented ice (Pg. 10 line 284). This is certainly one possibility, but it is also possible that there are crystals that are oriented but have different habits or surface roughness. Also, the assertion that this ratio is connected to mass seems very tenuous.

It is very challenging to determine anything definitive from most oriented ice crystal observations. Presumably one would need a sophisticated microphysical model (to capture temporal evolution of ice habit, roughness, size, etc) coupled with an advanced scattering model. By themselves the observations are underconstrained.

My suggestion is that the authors use a few words to reduce the certainty of their statements. For example: "It is possible that this is because. . ." They might also include listing some other possible explanations for what is happening. In my opinion this would enhance the value of the paper because it would keep readers engaged with the challenges that oriented ice crystals pose.

Pg 11, Line 316 "... including exploring the azimuthal orientation of particles." I suggest being clear that this was not explored in the current work and that looking for azimuthal orientation of particles would be in future work.

Points for clarification:

Pg 3, Line 62-64 It is not clear how the authors come to the conclusion that m12=-0.22 +/ -0.2 means that in 30% of the observational cases, the depolarization depends on the lidar reference plane. The value of m12 certainly does not dictate this. Is the assumption that PDF of m12 is Gaussian?

Pg 3, Line 64 "In other words. . ." This statement isn't totally clear. I think to clarify you want to say "...when the lidar's linear polarization rotates around. . ."

With regard to the near range channel, I'm a little confused about what its purpose is. Doesn't the fiber scramble the polarization modes and therefore prevent measurement of the depolarization ratio with this channel? What is this channel being used for? What ranges are the near and far range channels used for?

Pg. 5 Line 145 Just to be clear, this analysis of error in polarization angle does not translate into a baseline system error. The authors don't mention this explicitly, but there are almost certainly some effects unaccounted for (e.g. waveplate behavior, mechanical and optic axis misalignments, etc) in any system. It doesn't necessarily have to be published, but the authors should have some idea about the accuracy limits of their system and based on the data presented here, it is probably larger than 0.016%. One useful test is to look at how close the depolarization comes to zero in liquid clouds (before multiple scattering contaminates the signal).

Pg. 10 Line 278 The authors state that the signal variations with angle are smaller than the measurement errors. This really depends on what the authors mean by "measurement errors" because one can clearly see a trend in the data, so the limiting factor does not appear to be random error. If they mean this is less than the systematic error of the instrument, this is certainly a valid point. It would be good to clarify which type of errors they are referring to.

Also with regard to Figure 10 and the perpendicular measurements, I wonder if this angle dependence is the result of cross talk between the channels. Perhaps this is what the authors are referring to as "measurement error". If so it would be helpful to simply state that explicitly.

Pg. 10, Line 300 The authors describe that the depolarization measurements at 532 nm are only made for linear polarizations. This needs to be better explained in section 2 Lidar Description. I had assumed (incorrectly) that the authors were using a dual wavelength wave plate. Make it clear what wavelengths the polarization optics are designed for and please be clear throughout the manuscript that this instrument performs

the two polarization measurements only at 1064.

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