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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observations of boundary layer, mixed-phase and multi-layer Arctic clouds with different lidar systems during ASTAR 2007" *by* A. Lampert et al.

Anonymous Referee #1

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The paper by A. Lampert and co-authors provides four case studies of clouds observed with lidars during the ASTAR campaign at Svalbard. There are few measurements from high latitudes, and the authors should be commended for their efforts. The basic measurements seem pretty reasonable, but there are problems in the analysis and interpretation that need to be addressed. I recommend publication after the following revisions are made.

MAJOR

1) Conclusions: A major conclusion of the paper appears to be that lidars are good for observing clouds. This is already well known. The case studies are interesting

enough that other conclusions might be drawn. More attention to interpretation of the measurements in the context of earlier studies would make this a stronger paper.

2) Terminology:

a) Optical Density: There are statements in a few places that "optically thick" clouds were observed. The fact that the lidar can see right through these clouds means that they are not optically thick, by definition.

For Case B, the cloud optical thicknesses are said to be in the range of 11-17. However, the lidar sees right through this cloud, which is not possible for optical depths greater than 2 or 3 (depending on integration times).

b) Pre-condensed versus haze: The measurements presented in Case A are interpreted to be evidence of "pre-condensed liquid droplets" rather than haze aerosols. However, unless this is a case of homogeneous nucleation (very unlikely), I cannot see the difference between aerosol-nucleated water droplets and aerosol haze. While it is appropriate to distinguish between hydrated and dry aerosols, I don't think the distinction made in the paper is meaningful.

c) Mixed-phase: The term "mixed-phase clouds" in the Arctic usually refers to water clouds aloft with ice precipitation below. For Case B, the term is being used to describe a water cloud that later glaciates at the same altitude. It would be best to explain your use of the term to avoid confusion.

d) Cirrus: In Case C, clouds in the lower troposphere are referred to as "cirrus". This term is usually reserved for ice clouds in cold air near the tropopause. A more standard term should be used to describe the clouds in Case C.

3) Measurement techniques:

a) Klett inversion: The description of the Micro Pulser Lidar (Sec. 2.1) indicates that the Klett Inversion is used to obtain the backscatter ratio. However, that is not what the Klett Inversion does. It is used to obtain extinction coefficients or backscatter cross-sections, with an assumed lidar ratio (extinction to backscatter ratio). Perhaps the backscatter cross-section obtained using the Klett Inversion is used to determine backscatter ratios? If so, I would recommend using the more geophysically-relevant cross-sections the Klett Inversion provides. In any event, some clarification is needed.

The Klett Inversion is also used for the AMALi lidar. The text in Sec. 2.3 says "For the nadir measurements of optically thick boundary layer clouds, this value was set within the clouds and varied iteratively to obtain a reasonable backscatter ratio above the cloud". This approach seems strange to me. Normally one would normalize the Klett inversion in a region of clear air above the cloud and perform the retrieval in the downward direction. I am not sure how I would implement what is described. What is adjusted in the iteration? The initial extinction value? The lidar ratio? Note also that retrieving in the forward direction is unstable. Further explanation and description would be very helpful.

b) Use of backscatter ratio: In Figures 5 and 7, the backscatter ratio is used to profile the clouds. Are these true backscatter ratios, or the "attenuated" backscatter ratios that lidars normally provide? For attenuated backscatter ratios, the manuscript should point out that non-zero values below the cloud may not be evidence of particles there, but are elevated due to extinction in the cloud. For this reason, use of extinction profiles would have been a better choice.

c) Multiple-wavelength inversions:

i) For KARL, a multi-wavelength inversion scheme has been used to obtain particle size distributions, lidar ratios, and refractive indices. I have always been a little suspicious of the technique, given that the approach is not well-validated and will give results whether or not the input data is correct. In particular, I imagine that the mix of detector and signal acquisition techniques between the visible/UV and infrared measurements is very hard to deal with. Have you performed any validation of the KARL retrievals? Are there validation studies that demonstrate the technique actually works? For Case

A, you could compare with the sun photometer which usually outputs these same parameters as standard data products (for AERONET, anyway). If the data are available, perhaps the aircraft measurements described in the paper could be used. In any event, references to where in the literature such comparisons can be found are needed.

ii) In Sec 3.2.2 (Analysis of Case A), it says "... the backscatter and extinction coefficients are clearly decreasing with wavelengths indicating that the main part of the particles was smaller than 1.25 [microns]." However, decreasing extinction and backscatter occurs even in the Mie regime, where the scattering efficiency is roughly constant. Given that the inversion technique requires there to be no large particles, this unsupported assumption poses a serious problem. What is the sensitivity of the analysis technique to large particles?

d) Depolarization: Is the depolarization provided for particles only, or is it particles+molecules? There is a substantial difference when the molecular contribution is not subtracted. I also wonder if the laser beam is tilted to avoid specular reflections from horizontally-aligned platelets? Finally, it should be specified as "linear depolarization ratio" to differentiate it from the circular depolarization measured by some lidars.

e) Lidar ratio: Measurements of the lidar ratio are given in several places, but there is no description of how these were measured.

4) Multiple Scattering: There is repeated reference to multiple scattering effects in the paper. This is surprising given that the clouds observed were, in essence, optically thin. I wouldn't expect multiple scattering to be a factor in these measurements at all. As you are aware, multiple scattering can be observed using multiple fields-of-view. You have referred to the work of Luc Bissonnette, who used a lidar specially designed to rotate through different fields-of-view many times per second, which is needed because of rapid changes in the cloud itself. The instruments used in this study are not designed to perform such observations. The attribution to multiple scattering is instead frequently made on the basis of variations in depolarization with altitude.

However, there are very real variations in depolarization in clouds that aren't at all related to multiple scattering. In Section 4, "an afterglow effect behind the cloud" is also attributed to multiple scattering. I have never heard of such a thing from lidars before, and suggest that this may be due instead to photomultiplier tube ringing from over-exposure (highlighted elsewhere in the manuscript as a problem).

Unless more solid evidence for multiple scattering effects can be found, I recommend removing such distracting discussion from the paper.

6) Introduction: There is a discussion of the radiative impact of clouds, and a reference to the paper of Ehrlich (2009) claiming a surface cooling of -160 W/mˆ2. This result seems pretty extraordinary, since a net surface warming from clouds would normally be expected at this time of year. I would be hesitant to refer to that result until it has been reviewed. In any event, the conditions under which such an unusual result was possible should be described.

Also, there is a lot of background on mixed-phase clouds, but none on Arctic haze, diamond dust or blowing snow. It seems to me that these are relevant to the discussion, particularly Case A which deals with boundary-layer observations.

MINOR

1) HYSPLIT analysis: The trajectory analysis described on page 15141 is not very convincing. A more sophisticated model is needed to assess the impact of precipitation on the transported aerosol burden.

2) Case D: I don't think there is very much of interest in this Case. I recommend removing it.

3) Figure 1: The top panel isn't very useful, and I recommend eliminating it. The bottom panels should have the height intervals labeled rather than numbered, and the measurement dates printed on the plot.

4) One-sentence paragraphs: There are several one-sentence paragraphs, and these

should be expanded, joined to the surrounding paragraphs, or removed.

5) AMALi description (Sec 2.3): The first paragraph is very hard to read. It needs to be broken up into multiple paragraphs and clarified. Similarly, the first paragraph in Sec 3.5.1 (Case D) should be broken up into multiple paragraphs and clarified.

6) MPL cloud detection: The paragraph beginning with "Using different thresholds..." is really unclear.

7) Spelling errors: a) Sec 3.1.2: "lewel" -> level; "perseverate" -> ??? b) Sec 3.2.1: "radio sonde" -> radiosonde; "week" -> weak c) Sec 3.5.2: "through" -> trough d) Sec 4: "backscatter efficient" -> ???

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 15125, 2009.