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Atmospheric Measurement Techniques
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Discussions

Interactive comment on "Cloud thermodynamic phase detection with polarimetrically sensitive passive sky radiometers" *by* K. Knobelspiesse et al.

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We thank the reviewer for this review. Indeed, we feel that the primary goal of this work is to show how simulations indicate the cloud phase determination potential of ground based, polarization sensitive measurements, but challenges remain for use of existing instruments.

Responses to minor suggestions follow

1) Page 11993, Line 20: It would be more accurate to change the text to "blue (or red) and near-infrared reflectances".

Correct, thanks for pointing this out.

2) Page 11996, Line 16: R_Q, R_U, and R_I should be defined here.

Done.

3) Page 11996, Line 21: The authors haven't talked about the orientation of ice particles. How will it affect the use of polarisation on cloud phase determination?

We state that the simulations are made for "randomly oriented" particles on page 11994, line 23. Current simulations are limited to randomly oriented particles. Crystals can be assumed randomly oriented in most clouds globally and for clouds that contain oriented particles, their numbers are estimated to generally make up below 3% of the total ice crystal number [Noel and Chepfer 2010, Zhou et al., JAMC 2012]. Oriented plate-like particles lead to bright optical phenomena such as 22-degree parhelia (sundogs). However, polarization from these parhelia was found to be relatively weak compared to that associated with the 22-degree halo feature [McDowell, 1974; Zhou et al., 2012]. Moreover, such features are not observed at zenith. Based on these facts, effects of orientation on our technique are assumed to be minimal.

McDowell, R.S. 1974, "The Formation of Parhelia at Higher Solar Elevations", Journal of Atmospheric Sciences, 31;

Chen Zhou, Ping Yang, Andrew E. Dessler, Yongxiang Hu, and Bryan A. Baum, 2012: Study of Horizontally Oriented Ice Crystals with CALIPSO Observations and Comparison with Monte Carlo Radiative Transfer Simulations. J. Appl. Meteor. Climatol., 51, 1426–1439.

Noel, V., and H. Chepfer, 2010: A global view of horizontally oriented crystals in ice clouds from Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observation (CALIPSO). J. Geophys. Res., 115, D00H23, doi:10.1029/2009JD012365.

4) Page 11998, Line 27–28: I am not sure what the authors mean. If pristine ice crystals are present, they should be identified as well. I am not sure why the authors

said that they would interfere "with with" cloud phase determination. Could the authors please clarify? I understand it is related to Figure 6 and the discussions later on, but this sentence needs to be re-written.

We removed the double "with" typo, and added the sentence "Below we will further investigate sensitivity to these factors."

5) Page 11999, Line 16–29: This paragraph is a bit disorganised; the key points are not as clear as they should be. I would suggest re-working on it. Also, I am not sure why the authors said that "P12 is primarily dependent upon AR, and then a few sentences later, say "so size has a minimal impact on P12". I understand AR and size are not the same things, but they are not completely independent in reality. Could the authors please clarify?

We modified portions of that paragraph to say: "Figure 6 shows P12 of drops and ice crystals with varying aspect ratios. Here, variations in ice size are not considered since ice crystals are generally large enough for their scattering properties to be simulated with geometric optics (Bi et al., 2014) and thus size has a minimal impact on P12. Note, however, that aspect ratio could depend on size [e.g., Auer and Veal 1972; Um et al., 2015]."

And added these references:

Um et al. "Dimensions and aspect ratios of natural ice crystals", Atmos. Chem. Phys. Discuss., 14, 31111–31167, 2014

Auer, A. H. and Veal, D. L.: The dimension of ice crystals in natural clouds, J. Atmos. Sci., 27, 919–926, 1970.

6) Page 12001, Line 14: I wouldn't say it is proper to categorise Turner et al. (2003) into "microwave radiometer". Please correct it.

Indeed the reference was not correct.

We included "hyper spectral infrared measurements" in the list of techniques to determine phase and refer to the Turner et al. paper.

7) Page 12002: Could the authors give a brief review/status of AERONET polarisation measurements, for example, where they have been collected, and how many of sites/measurements can be potentially used/modified.

The number of polarization sensitive instruments (both single channel and multichannel) is evolving – roughly 10% of the 500 or so currently deployed field instruments.

8) Page 12002, Line 10–20: this is a very minor suggestion about presentation. Perhaps the authors can link discussions back to figure numbers in Section 2 so readers can review key figures/points quickly again.

Done

9) I would suggest changing line styles used in Figure 4, because it is quite hard to see the difference. Also, could the authors please explain, for example, at a given COD =7.5, why Q values don't change monotonically with increasing AR?

We made some modifications to the figure to try to make it more legible. Q values appear to have the least amount of polarization for the most compact AR (1.0), and monotonically greater (negative) polarization magnitudes for more extreme AR's (0.05 and 20.0).

10) Page 12003, Line 20-25: For Figure 10, the authors expect a wider range in Q than U, but the range of Q also likely depends on type of ice clouds. Could the authors provide some information/literature on what type of ice clouds (e.g., stratiform ice clouds, convective anvils, wave cirrus) likely occurs at Cabauw?

As explained in line 12, the point that we are trying to make is that U is expected be near 0, while Q is expected to show a substantial range. As we demonstrate, the value of Q depends on cloud optical thickness, ice crystal aspect ratio and crystal roughness.

Indeed, these properties could be related to cloud type, but these relationships are still poorly understood.

Both stratiform and convective clouds can be expected and we have changed the sentence at line 25, page 12002, to read:

"This site was selected because of the variety and frequency of cloudy conditions, both stratiform and convective, encountered in the Netherlands. . ."

We think that the remark that Q depends on ice aspect ratio, etc., is more relevant in the discussion of Fig. 12, where we added the sentence (12004, line 19):

"Although the value of Q depends on cloud optical thickness, ice crystal aspect ratio and crystal roughness and solar geometry, generally Q is negative for ice clouds"

11) Page 12004, Line 6-7: Why selecting 0.0001 as the threshold to distinguish between liquid and ice clouds? Could the authors please provide justifications?

We inserted the sentence to Page 12004, line 8: "The 0.0001 threshold was chosen based upon measurement uncertainty assessments described in Appendix A."

12) Figure 10: Perhaps for the figure at right, the authors need to avoid labelling y-axis as "smooth", when data points were not exactly "smoothed", rather than just being rotated with an angle? It is a bit confusing, because data points in Fig. 11 were indeed smoothed.

"Smoothed" is a typo and was removed. No data in this figure were smoothed.

13) Explanations for Figure 12 are not quite right. Since the dataset includes both winter and summer, freezing level heights could be quite different and thus clouds with the same cloud base height could correspond to either water or ice cloud, depending on the season. That's perhaps why the histogram does not show a clear correlation as expected. I would suggest identifying cloud phase using sounding data for each point, and then plotting histograms of Q to see how distributions of Q vary in both water and

ice clouds.

The following sentences were added to the third paragraph on page 12004: Seasonal differences within the cloud base height dataset were also examined. Using NASA MERRA reanalysis, we found for the Netherlands in December, 0C is at roughly 200m, -20C is at roughly 4km and -40C is at roughly 7km, while in July 0C is at roughly 2.5km, -20C is at roughly 6km and -40 is at roughly 8.5km. These numbers indicate that the level above which ice clouds can be expected increases with height and above 4-6km, most clouds are expected to be glaciated. Based on these facts, we would expect an increasingly skewed distribution of Q with increasing height.

14) Could the authors elaborate on the sources of noises in Q measurements? The authors mention the knowledge on reference frame, but is there any other important source?

We added the following to the last paragraph on page 12004.

Appendix A is an assessment of two types of measurement uncertainty (filter placement/frame of reference knowledge and filter polarization efficiency). Presumably, error due to these sources is fixed. Random, highly temporally variable sources of uncertainty are most likely electronic/detector related, but difficult for us to characterize without more information about the instrument. Because the Q and U elements of the Stokes vector are determined by differences between channels (see equation 2), uncertainty due to this type of noise is magnified.

15) Page 12005, Line 13-15: Could the authors please confirm that there was no polarisation data available from the MPL at GSFC? Otherwise, depolarisation ratio would be more informative than cloud-base height, although it remains limited for optically thick clouds.

The depolarization ratio would indeed be more informative than cloud base height. MPLNET is in the process of upgrading their instruments and algorithms to include polarization sensitivity, but these data are not yet available.

16) Page 12006, Line 18-19: Perhaps that the authors can be more precise/specific about this, because one can use cloud boundaries defined by radar and then classify cloud phase from cloud boundaries, but I don't think that's what the authors have in mind. I guess that the authors meant polarisation information from radar observations here.

What we had in mind was cloud phase retrieval techniques that use a combination of multiple sensors, such as the one developed by Shupe et al. (2007). This reference was added to the sentence as example.

Shupe, M. D. (2007), A ground-based multisensor cloud phase classifier, Geophys. Res. Lett., 34, L22809, doi:10.1029/2007GL031008.

Additional changes:

We modified an incorrect acronym usage (MPLnet to MPLNET) and changed the Acknowledgements section to properly thank the MPLNET team.

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 11991, 2014.