Atmos. Meas. Tech. Discuss., 7, C4722–C4725, 2015
www.atmos-meas-tech-discuss.net/7/C4722/2015/

Atmospheric Measurement Techniques Discussions

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

Anonymous Referee #2

Received and published: 24 January 2015

Using examples from the Aerosol Robotic Network (AERONET), this manuscript investigates whether ground-based polarisation measurements can be used to distinguish cloud thermodynamic phase, with an aim to improve cloud properties retrieved from shortwave zenith radiances. The manuscript is well structured and well written. Although the manuscript reports results that are somewhat expected (because multiple scattering in optically thick clouds would wash out polarisation signature, and thus polarisation is less useful in cloud mode retrievals that tend to observe optically thick clouds due to the operation strategy), the authors are candid about their finding, and have done a great job to systematically investigate/discuss their sensitivity tests. More

importantly, the authors point out some possible pathways to improve ground-based polarisation measurements and their use, which is invaluable for the cloud community.

Some minor suggested changes are listed below:

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

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

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?

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.

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?

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

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.

8) Page 12002, Line 10–20: this is a very minor suggestion about presentation. Per-

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haps the authors can link discussions back to figure numbers in Section 2 so readers can review key figures/points quickly again.

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?

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?

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?

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.

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.

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?

15) Page 12005, Line 13-15: Could the authors please confirm that there was no polar-

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isation 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.

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.

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