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Interactive comment on "Distinguishing cirrus cloud presence in autonomous lidar measurements" *by* J. R. Campbell et al.

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In the underlying manuscript the authors Campbell et al. utilize a full-year set of CALIOP data to evaluate the accuracy of different methods (temperature, depolarization ratio, height, optical depth) for the classification of cirrus clouds. The focus of the manuscript is hence not to introduce a new measurement approach or data analysis technique but to present a range of uncertainty in the classification of cirrus-type clouds when different methods are applied.

The abstract of the manuscript provides an adequate summary of the paper's content and it's conclusions. Also the results section is done nicely. The different classification techniques are evaluated against each other step by step. Writing style, spelling and

grammar are appropriate and a final proof-read of the co-authors should be sufficient to fix the last remaining pitfalls.

While reading, I noticed some major (and a few minor) issues which should be addressed and/or taken into account by the authors before publication. Those mainly deal with the information given in the exhaustive introduction that lacks some references to available literature that is relevant and can in part change the notion of the manuscript. When the major comments are taken into account the manuscript can be recommended for publication.

Major comments:

1) Section 1 highlights the problem of defining what a 'cirrus' cloud is. As the authors conclude there is not yet a consistent definition that separates 'cirrus' from whatever 'warm' ice formation process (P7213, L1). As the authors state, the typical cirrus clouds as they are identified by human observers are formed via either deposition freezing nucleation (during large-scale lifting of air or radiative cooling) or homogeneous nucleation (in deep convective clouds). If one would just stick to these nucleation processes as the ones related to cirrus formation there would be no need to speak of 'warm' ice/cirrus production. In turn, one could look into literature of ice-formation studies to see why cirrus is so frequent at low temperatures: Homogenous freezing nucleation will (at least for realistically small droplet sizes) not take place at temperatures far below -37°C. Deposition freezing efficiency, however, strongly depends on temperature and supersaturation but also on the type of IN (Hoose and Möhler, 2012). Thus, given appropriate saturation, temperature, or IN type, deposition freezing can occur also at rather high temperatures. There are two recent studies with coincidentally similar titles available from 2012 that statistically provide evidence that virtually all ice formation that occurs at T>-25°C is formed via the liquid phase (deBoer et al., 2012; Westbrook et al., 2012). Similar conditions were reported for the tropics (Ansmann et al., 2008). So, in conclusion, there is a remaining temperature range between -25°C and -37°C that resamples a transition region from liquid-dependent (mixed-phase) ice formation and

deposition-related ice formation. I, personally, don't see a reason why those clouds that formed entirely via deposition should not be denoted cirrus clouds. In Seifert et al., 2011, an example is given for a pure ice cloud that formed at cloud-top temperatures between -26 and -35°C. Ice formation in that case was affected by the presence of large amounts of aerosol particles from the plume of the Eyjafjallajökull volcano in 2010. In fact, that plume was also able to reduce ice production by the presence of high amounts of hygroscopic sulfur. I see a good chance and potential to have some more discussion added on the mixed-to-pure ice transition region between -25 and -37°C into the manuscript.

2) The manuscript misses a description of the flaws of lidar-only studies on cloud properties. As Zhang et al. 2010 note, the CALIOP-only approach misses quite a fraction of liquid-topped mixed-phase clouds because the lidar signal is already attenuated in the liquid layer before any signal can be returned from the ice below. The same is of course the case when a thick cirrus cloud layer is present above a lower one. The overall statistic (with it's impressive high number of cases) may not be affected too much but the reader should be informed that CALIOP may lack the detection of ice below liquid layers and of multiple cloud layers. Also the possible solution to combine lidar and radar (as done by Zhang 2010) should be mentioned.

Minor comments:

1) P7214, L21: There is a large number of publications available on the presence of relatively warm, long-lasting mixed-phase layers in the Arctic(e.g., Fridlind et al, 2007; de Boer et al, 2011). Hence, the authors should justify their statement given at this position in the text or it should be modified taking into account the two references above. The statement may eventually be right when only CALIOP-data is taken into account – in that case, however, only because of the fact mentioned in Major Comment #2 above.

2) P7221, L16: The optical-depth range from 0 to 3 is apparently not the real range of

cirrus optical depth. At least as long as also warm-frontal cirrus and deep-convective cirrus is included into the cirrus definition (http://isccp.giss.nasa.gov/cloudtypes.html , and as indicated in Fig. 6 in the manuscript). I assume in the case of SC2001 it is rather the OD-range that can be covered with standard lidar.

3) P7226, L21: SC2001 was already defined earlier.

4) I would suggest to use Kelvin as absolute unit for temperature intervals because °C is a relative unit. E.g., also time differences are given in absolute hours instead of relative 'o'clock'.

5) Acknowledgements: There is no author with initials 'R. J. H'...

References: Ansmann, A., M. Tesche, P. Seifert, D. Althausen, R. Engelmann, J. Fruntke, U. Wandinger, I. Mattis, and D. Müller (2009), Evolution of the ice phase in tropical altocumulus: SAMUM lidar observations over Cape Verde, J. Geophys. Res., 114, D17208, doi:10.1029/2008JD011659.

de Boer, G., H. Morrison, M. D. Shupe, and R. Hildner (2011), Evidence of liquid dependent ice nucleation in high-latitude stratiform clouds from surface remote sensors, Geophys. Res. Lett., 38, L01803, doi:10.1029/2010GL046016.

Fridlind, A. M., A. S. Ackerman, G. McFarquhar, G. Zhang, M. R. Poellot, P. J. DeMott, A. J. Prenni, and A. J. Heymsfield (2007), Ice properties of single-layer stratocumulus during the Mixed-Phase Arctic Cloud Experiment: 2. Model results, J. Geophys. Res., 112, D24202, doi:10.1029/2007JD008646.

Hoose, C. and Möhler, O.: Heterogeneous ice nucleation on atmospheric aerosols: a review of results from laboratory experiments, Atmos. Chem. Phys., 12, 9817-9854, doi:10.5194/acp-12-9817-2012, 2012.

Seifert, P., et al. (2011), Ice formation in ash-influenced clouds after the eruption of the Eyjafjallajökull volcano in April 2010, J. Geophys. Res., 116, D00U04, doi:10.1029/2011JD015702.

Westbrook, C. D., and A. J. Illingworth (2011), Evidence that ice forms primarily in supercooled liquid clouds at temperatures > $-27°C$, Geophys. Res. Lett., 38, L14808, doi:10.1029/2011GL048021.

Zhang, D., Z. Wang, and D. Liu (2010), A global view of midlevel liquid-layer topped stratiform cloud distribution and phase partition from CALIPSO and CloudSat measurements, J. Geophys. Res., 115, D00H13, doi:10.1029/2009JD012143.

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