Thank you to each of the reviewers for their reading of the manuscript. Many important changes have come from your comments. All changes in the revised manuscript have been made in red font to distinguish them from the original narrative.

Please note that, due to a coding error, Figure 6 has been recalculated. The new version shows effectively no significant departures from the original, however. The new figure has been included in this response as an attachment.

Reviewer #1

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.

Thank you for your detailed reading of our manuscript. Furthermore, thank you for signing your review, Dr. Seifert. We greatly appreciate your considerate comments and insight.

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.

Thank you for this insightful point. We have a couple of responses and thoughts here.

First, at/about P7212 L26, we introduce the potential bias of "glaciated liquid cloud remnants". In this single sentence, we have implied the existence of many of the processes that you have described above. We weren't trying to downplay the significance of these processes. Suffice to say, however, we weren't necessarily trying to open the Pandora's Box that they represent, either. The apparent lack of clarity to you, though, necessitates we expand the thought. Therefore, after "remnants", we have added the following passage

> "SC2001 acknowledge that glaciated liquid water clouds [i.e., heterogenous freezing of liquid water droplets induced by aerosol particles, like mineral dusts (DeMott et al., 2009) or even other ice crystals (Campbell and Shiobara, 2009), for example, as well as volcanic residues (e.g., Seifert et al., 2011)]".

And again at P7225 L9, in the conclusions, after "remnant" as

> "(i.e., the heterogenous freezing of liquid water droplets induced by aerosol particles)"

Furthermore, we've added a fourth question to the conclusions as

> 4. "What other factors (i.e., nucleation mechanism through homogeneous vs. heterogenous freezing, supersaturation rates, etc …) drive first-order differences in ice-cloud macrophysical, microphysical and radiative properties, which ultimately may require resolving in the long run to reach closure on autonomous cirrus cloud identification for climate study?"

Aside from those additions, however, we are unsure how to adapt the discussion much further. That is, whereas we recognize that there are likely fundamental differences in ice physical properties as a function of the dominant nucleation mechanism, we know that, as Dr. Seifert states, "warm" ice is overwhelmingly frozen through heterogeneous processes involving the liquid state. However, we still anticipate measuring significant differences in ice physical characteristics generated from (to borrow an example from Dr. Seifert's 2011 paper) a glaciated altocumulus cloud versus an Arctic sulfur-seeded ice cloud, despite the common predominance of heterogeneous freezing. Thus, there is still a wide range of potential micro/macrophysical/radiative properties that may result in such clouds, and we've still not even

addressed the fundamental question of the paper as to whether or not the clouds are "phenomenologically" cirrus. We further can't…

a) differentiate them operationally with autonomous algorithms (which we point out at P7212 L9), or
b) reconcile differences in nucleation with phenomenological traits observed by ground and/or digital characteristics measured with autonomous observations.

In summary, beyond the additions we have already suggested, at this time we don't believe we can reasonably add "more discussion [...] on the mixed-to-pure ice transition region". The reason for our reticence is simply because the currently available remote sensing instrumentation does not give us the insight into ice microphysics that would be needed to differentiate ice clouds according to nucleation mechanism.

So, we hope that we've addressed the point as thoroughly as the narrative, as designed, allows, and that we've thus acknowledged its significance even though there isn't much that we can really do to further resolve it with current ground/satellite observational capabilities.

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.

Thanks for highlighting this point. We agree that this additional context is fundamental to the case that we are trying to make. We have changed some of Section 2 to address this point. In particular, there is now a new second (of now three) paragraph, which reads as

"Any generic interpretation of sample counts and relative cloud frequencies described in the narrative for anything but the stated intention of each test applied should be considered with these caveats in mind. Further, the reader must also consider the influence of the nadir-viewing lidar geometry, and how signal attenuation through optically-thick clouds can limit the vertical extent of CALIOP profiling. Zhang et al. (2010), for instance, describe the synergy necessary between CALIOP and the CloudSat millimeter cloud radar (Stephens et al., 2003), also flown within the A-Train, for profiling mixed-phase liquid-layer topped stratiform clouds. Though the CALIOP geometry is obviously far more preferable to ground-based zenith-profiling lidar application, with respect to attenuation effects, the data are not immune to some degree of sampling bias. The results below, again, are presented in mostly relative context, except where specifically denoted."

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.

Thank you for rightfully identifying this admittedly clumsy sentence. We were not trying to say anything so profound here. We were simply trying to describe for the reader the impact of choosing the 5-km cloud product from the Level 2 CALIOP archive, where clouds identified at 333 m resolution alone are excluded. As we don't believe it likely that such exclusion would bias our eventual cirrus cloud sample, this statement was simply for clarity and consistency.

The sentence has been changed to

> "These clouds are resolved from single CALIOP signal profiles at native 0.333 km along-track resolution, and are generally unlikely, even in Polar Regions, to correspond with cirrus cloud presence."

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.

It's a subtle point relative to the intended context of the sentence. But, we agree that it should be more accurately stated, and thus qualified. The sentence has been changed to

> "Cirrus clouds that are readily apparent to the ground observer typically correspond with optical depths between 0 and approximately 3 (Sassen and Cho, 1992), whereas nearly all liquid water cloud genera exhibit significantly greater values."

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

Thanks. This is a stylistic choice to redefine all acronyms in the Conclusions, in the event the reader skips ahead to the final section without reading the rest of the narrative.

No change was made.

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

Thank you for these suggestions. We do not believe that there are any references to time in the manuscript. We trust that this is an error. With respect to Kelvin, though, we appreciate this thought. However, for consistency with the literature, and discussion relating specifically to homogeneous freezing in particular, degrees Celsius are almost exclusively used (e.g., Fig. 7.6 in

Pruppacher and Klett…their classic plot of freezing rate vs. supercooling). Therefore, to maintain this construct, we would prefer leaving units in the discussion as is.

However, to clarify this reasoning for the reader, we have added to P7212, L6,

> "Note that for consistency with discussion relating to the homogeneous freezing of liquid water in the literature, we maintain use of degrees Celsius throughout the manuscript."

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

Indeed. This has been rightfully changed to R.E.H. Thanks.

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.

Note that the Seifert et al. reference has been added, as well as

Campbell, J. R. and M. Shiobara, 2008: Glaciation of a mixed-phase boundary layer cloud at a coastal Arctic site as depicted in continuous lidar measurements, *Polar Sci., 2*, 121, doi:10.1016/j.polar.2008.04.004.

DeMott, P. J., K. Sassen, M. R. Poellot, D. Baumgardner, D. C. Rogers, S. D. Brooks, A. J. Prenni, and S. M. Kreidenweis: Correction to ''African dust aerosols as atmospheric ice nuclei,'' Geophys. Res. Lett., 36, L07808, doi:10.1029/2009GL037639, 2009.

and

Zhang, D., Z. Wang, and D. Liu: 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, 2010.

Thank you again, Dr. Seifert, for your constructive comments.

Reviewer #2

Review of "Distinguishing cirrus cloud presence in autonomous lidar measurements" by Cambell et al.

I found this to be a well written and thought provoking contribution. After considering the review due to reviewer #1, I only have a very few minor points too add in addition.

Thank you for considering our manuscript, and for your supportive comments. We sincerely hope that this paper will in fact prove thought provoking within the community.

1. Page 7210, Line 11: "..are each.." => "..are all."

Thanks. Changed as suggested.

2. Page 7222. Line 24: Perhaps it is useful to point out here that also an a priori multiple-scattering correction is also applied. I realize this is discussed later but it still seems appropriate to mention it here.

Agreed. Changed to:

> "Note that the majority of the optical depth retrievals in this analysis were derived using "unconstrained" retrievals where *a-priori* values for the lidar extinction-to-backscatter ratio and multiple scattering correction have been applied."

3. Page 7225,Line 25: "Progress is made. Ultimately.." ==> Progress is made ultimately...'

Agreed. Changed to:

> "Progress is made, by documenting the difficulties surrounding the task, and outlining the sensitivity to global and regional cloud properties derived under varying constraints for cirrus presence. It is ultimately hoped, however, that this paper will motivate a discussion within the community that helps resolve lingering questions that would improve such analysis further."

Thanks again.