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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The origin of midlatitude ice clouds and the resulting influence on their microphysical properties" *by* A. E. Luebke et al.

Anonymous Referee #1

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Review of "The origin of midlatitude ice clouds and the resulting influence on their microphysical properties" by Luebke et al.

Recommendation: Should be acceptable for publication after revision

This paper proposes that there are two types of cirrus clouds, classified as in-situ and liquid origin. Data about particle mass contents, concentrations and particle sizes obtained during the 2014 ML-CIRRUS campaign are then analyzed and segregated into these two classifications, and the microphysical properties between the two types of classified cirrus are compared. As the paper presents a new way of separating cirrus populations and presents analysis of a new set of ice cloud data collected during a campaign, I think that the paper would be of interest to the readers of ACP. However, there are some features about the paper and its presentation that should be improved

before it is regarded as acceptable for publication.

First, I think that they should considerably improve the comparison of their results and methodology with that presented in previous manuscripts. Currently, they mainly compare their approach to approaches that have previously been used within their group, both in terms of comparing the calculated microphysical properties and in terms of comparing their classification mechanism against the homogeneous/heterogeneous classification mechanisms that their own group previously used. But, there are copious ice crystal data sets that have been collected in other field projects and other methodologies for classifying cirrus used by other groups that also merit comparison. For example, there are extensive papers by Heymsfield where ice cloud properties are classified according to origin (convective or stratiform) or papers where cirrus properties are presented for one specific classification of cirrus. How do the properties observed between the in-situ and liquid origin clouds compare against some of the microphysical properties that were measured in these previous experiments. Further, recent observations collected in ice clouds during the Small Particles in Cirrus (SPARTICUS) campaign have been analyzed by Muhlbauer et al. (2014) and Jackson et al. (2015), with different schemes used to classify cirrus according to their origin. How do the classification systems presented in this study compare to the classification systems that were used in those previous studies? Is there any reason why their classification system is advantageous? If so, it should be explicitly stated what these reasons are, or at the very least, the advantages and disadvantages of their new scheme discussed. I think some discussion about the relative merits of all the different classification systems is sorely needed. Some more detailed comparison with prior observations would be useful to place their study within the appropriate context.

Second, when discussing the in-situ and liquid origins of cirrus clouds, I think it would be very advantageous to discuss which of the different previously hypothesized ice nucleation mechanisms apply to each case. For example, wouldn't homogeneous nucleation be only associated with liquid origin clouds because such liquid origin particles would freeze while being lifted to cirrus temperatures. Some of the heterogeneous mechanisms that involve a transient occurrence of liquid could also be associated with liquid clouds? I'm not sure how the in-situ clouds could be forming from homogeneous nucleation? I think some more discussion in relation to nucleation mechanisms could clarify some of the confusion I experienced here.

The third major critique of the current manuscript I have is that some of the comments are overly speculative and not fully justified according to the data that are presented in the manuscript. For example, there are a lot of comments about the role of secondary ice nucleation and secondary homogeneous ice nucleation in the later parts of the manuscript. There needs to be better justification of these comments. The manuscript should restrict itself to statements that can be definitely shown, rather than saying certain observations are likely indicators of the operation of some process.

Fourth, with regards to the microphysics probes it would seem that the combination of the CIP and CAS is not sufficient for detecting any larger particles if they are present. Particle reconstruction techniques would only work up to a certain extent to give concentrations of particles that are larger than the widths of the photodiode array. How were larger particles handled? What did the mass distribution functions look like? Does the absence of direct measurements of large particles affect the derivation of the microphysical quantities? In addition, some estimates of the uncertainties associated with the derived products would be beneficial.

The authors apply the mass-diameter relationship of Mitchell et al. (2010) to the results of their study. But, past studies have shown that there is a lot of variation in the mass-diameter relationships depending on the meteorological situation and the location of the measurements. How can the authors be confident that this relationship derived for tropical clouds apply to data collected in a different geographical location? How much of au uncertainty might be induced by the use of this relationship? I am also having trouble reconciling this statement with the statement on line 17 on page 34252 that the mass in each size bin is calculated using a simplified assumption that all crystals are

spheres. Assuming all particles is spheres is a huge error, so I can't understand why the mass-diameter relationships are not being used here.

The authors state that they are able to use the CLaMS-ice model to classify the flight segments by origin type. I would have expected to have seen more information about the validation of the model if the model is playing such a critical role in the classification procedures being used in the paper.

The authors state on page 34259 that cirrus typically have small ice at the top, larger ice crystals at the bottom, and that the smallest crystals are found where nucleation is occurring. But, isn't this a contradiction to statement that for liquid origin cloud that nucleation occurs when liquid particles are ascending to some temperature? Also with regards to in-situ clouds it is stated that they form by heterogeneous and homogeneous ice nucleation whereby an air parcel rises and cools to a point at which a freezing threshold is crossed and ice crystals can form and continue to grow as conditions allow. I think the dynamical activity of cirrus and the role of sedimentation also have to be considered in order to understand the observed structure of cirrus.

Detailed Comments:

Page 34250, line 7: Did the probe have anti-shatter tips?

Page 34250, line 14: Given the lack of sphericity of ice particles, it would appear that Mie theory would not apply for sizing particles because this only refers to spheres.

Page 34252, line 5: Were these particles truly spherical, or was simply there not enough photodiodes shadowed to resolve their shape?

Page 34252, Eq (2): Note that equations are not unit specific. The equation should work regardless of what units are used for different variables and will be reflected in the final units. Remove conversion terms in equation and statement that certain parameters must be in certain units.

Page 34253, line 10: Are any ensembles run to give any indication about the uncertainty of these trajectories. This would seem to be important given the role of the model in determining the origin of the cirrus.

Page 34257, line 2: Most papers present IWC in units of g/m3. I would recommend converting to these units for more easy comparison with past studies.

Page 34259: There are many other studies that have measured PSDs in addition to the few that are referenced here. I would recommend also comparing results with many of the prior other studies of cirrus PSDs to better understand the context of these new observations from CIRRUS.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 34243, 2015.