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Interactive comment on "CALIPSO observations of the dependence of homo- and heterogeneous ice nucleation in cirrus clouds on latitude, season and surface condition" *by* David L. Mitchell et al.

Anonymous Referee #1

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Review of "CALIPSO observations of the dependence of homo- and heterogeneous ice nucleation in cirrus clouds on latitude, season and surface condition" by D. Mitchell et al.

This manuscript describes a new algorithm for retrieving cirrus ice concentration and effective radius from the CALIPSO satellite IIR and CALIOP measurements. The retrieved ice concentrations are used to derive regional and seasonal distributions of the fractions of cirrus formed by homogeneous and heterogeneous ice nucleation. If the retrievals are quantitatively accurate, the global distributions of cirrus ice concentrations and effective radii would be quite valuable in themselves, and the resulting conservative estimates of homogeneous freezing predominance would be a significant

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discovery. However, I do not believe the manuscript is suitable for publication in its current form. I strongly suspect the retrieved ice concentrations far exceed those indicated by recent in situ observations. The comparison measurements published by Krämer et al. (2009) included in the manuscript does not provide a clear assessment of the retrieved quantities, and readily-available recent in situ measurements based on improved instrumentation should be used instead. If the authors can provide a clear comparison between the retrieved quantities and the recent in situ observations, and if the retrieved ice concentrations do agree well with these measurements, then the revised manuscript would be a valuable contribution to ACP.

Note that, given the possibility of a fundamental overestimation of ice concentrations from the retrieval, I have not provided detailed comments on the results shown beyond Figure 13 since these results are entirely dependent on the accuracy of the retrievals.

General comments:

1. There seem to be a number of approximations, corrections, and estimates involved in the retrieval procedure, and it is difficult to evaluate the potential errors and uncertainties associated with each of these steps. As a result, it is difficult to evaluate the uncertainties in retrieved ice concentrations used to distinguish between heterogeneous and homogeneous nucleation. Therefore, I think it is essential to include a proper comparison between the retrieved ice concentrations and in situ measurements of ice concentrations. The 2D-S measurements, which are most likely to be relatively free of shattering artifacts, would be ideal for this comparison. I suggest that the authors need to include comparisons between retrieved and in situ-measured frequency distributions of ice concentrations for the time periods and geographical regions corresponding to recent field experiments using the 2D-S instrument (SPARTICUS, MACPEX, TC4, ATTREX, etc.). The in situ data can be subsetted to include appropriate extinction and temperature ranges for ideal comparison with the retrieved ice concentrations. My general impression is that the occurrence frequency of ice concentrations exceeding 500/L derived from the retrievals is far higher than indicated by in situ measurements, but a

proper comparison would clearly show whether such a discrepancy truly exists.

2. As shown by a number of recent modeling studies (e.g. Spichtinger and Gierens, 2009b, ACP; Jensen et al., 2012, JGR; Murphy, 2014, GRL), even though homogeneous freezing can produce high ice concentrations just after nucleation, sedimentation and entrainment tend to reduce ice concentrations as the clouds evolve, and even if the clouds are produced by homogeneous freezing, relatively low ice concentrations tend to dominate over the full depth and lifecycle of cirrus clouds. Therefore, the frequency of occurrence of ice concentrations exceeding 500/L is undoubtedly a vast underestimate of the fraction of clouds produced by homogeneous freezing. Some discussion of this issue should be included in the paper.

Specific comments:

1. Abstract: perhaps you could say something specific here about how the retrieval algorithm works?

2. Page 2, line 12: It would be clearer to say “such as an aqueous aerosol” than “such as a liquid drop” since you’ve already stated that the you’re focusing on the temperature regime where liquid can’t exist.

3. Page 4, line 5: It would be good to note that the ice concentration produced by hom is most sensitive to cooling rate.

4. Page 4, first paragraph: It would also be worth mentioning that het and hom can both contribute in air parcels that are cooling rapidly and/or have low IN concentrations. In this case, hom typically dominates the total ice concentration.

5. Page 4, second paragraph: Barahona and Nenes (2009) used a parcel model that did not include sedimentation. As shown by a number of recent studies (see general comment above), sedimentation can produce large regions below the level of nucleation with relatively low ice concentrations. In other words, sedimentation from a layer produced by hom with high ice concentrations can feed a deep layer with low ice concentrations. Furthermore, modeling studies show that entrainment and differential sedimentation will tend to wash out the high ice concentrations produced hom, such that aged clouds originally produced by hom will have low ice concentrations and might look very similar to clouds produced by het.

6. Page 6, lines 19–22: Why don't you use MACPEX data in addition to SPARTICUS data (same instruments lots of cloud measurements)? Additional references describing the missions, flights, and data should be included?

7. Page 6, line 26: So, is the first 2D-S size bin included in this analysis?

8. Page 6, line 28–29: It would be helpful to have some specific information about the method used to derive β_{eff} from the 2D-S PSDs.

9. Page 7, lines 14–15: Was satellite imagery used to distinguish TC-4 "fresh" (attached) from "aged" (detached) cirrus? It would be more appropriate to consider the time since detrainment from convective updrafts. In any case, there doesn't seem to be any distinction between the fresh and aged cases in the N/IWC vs β_{eff} dependence shown in Figure 2.

10. Page 7, line 27: It looks like most of the TC4 data had N/IWC $<$ 10^7/g. Does this imply the method described here cannot be used for most of the TC4 data? Perhaps these are just lumped into the het category based on relatively low N/IWC?

11. Figure 3: It would be helpful to show the D_e values on the right axis corresponding to the (D_e^{-1}) on the left axis.

12. Figure 4: Do satellite- and in situ-derived retrievals of β_{eff} agree for SPARTICUS cirrus colder than about -54 C, or is the method just not applicable at the colder temperatures? This would imply that the method is only applicable for a narrow temperature range between about 220 and 235 K. Were MODIS-retrieved β_{eff} values for SPARTICUS similar to those in TC4?

13. Page 7: SPARTICUS included both convective and in situ cirrus. Are the regres-

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sion curves shown in Figures 3–5 significantly different for these two types of cirrus?

14. Page 9, line 9: Presumably, the authors mean Figures 2 and 3 here instead of Figures 4 and 5.

15. Page 12, lines 1–2: A brief description for how the uncertainty in retrieved ice concentration should be included in the body of the paper.

16. Page 12, lines 15-17: I disagree with the authors' dismissal of the shattering problem for cirrus colder than 240 K. A number of papers have shown that ice crystal shattering is a major problem for FSSP-type instruments even in colder cirrus and even with the shroud removed whenever relatively large ice crystals are present (e.g., McFarquhar et al., 2007, GRL; Jensen et al., 2009, ACP). The authors should compare their results with more recent measurements using the 2D-S instrument with post-processing based on inter-arrival times to remove shattering artifacts (e.g., from SPARTICUS, MACPEX, TC4, and ATTREX). An appropriate comparison would be to show the frequency distributions of ice concentrations from the retrievals with frequency distributions of in situ-measured ice concentrations for appropriate extinction and temperature ranges.

17. Figures 7 and 8: The comparison between in situ-observed and retrieved ice concentrations is obscured by showing six orders of magnitude, but it appears that the most common retrieved ice concentrations are generally considerably higher than those measured in situ, particularly over land. The discrepancy would likely be more glaring if the 2D-S measurements were used (see general comment above).

18. Page 15, lines 3-5: As noted here, the selection criteria used result in a small sample of the total cirrus present being included in the analysis. Perhaps some discussion of whether the sampling bias tends to select lower or higher ice-concentration cirrus could be included.

19. Figure 13: The retrievals indicate very high $N > 500$/L fractions. Over land, the fractions typically exceed 50% in the extratropics. Published statistics of ice concentration from TC4 (Jensen et al., 2009, ACP) and SPARTICUS/MACPEX (Jensen et al., 2013, JGR) show that the 2D-S in situ-measured ice concentrations very rarely exceed 500/L.

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