Manuscript Review

Differences in microphysical properties of cirrus at high and mid-latitudes

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General comments

This study is a careful but detailed comparison between mid-latitude (ML) and high-latitude (HL) cirrus by evaluating the differences in three, microphysical bulk properties of these high altitude clouds – number concentration, median effective diameter and ice (liquid) water content . I commend the authors on the clarity of their presentation and the plausible explanations of why there are significant differences in the microphysical properties. I also believe that this is the first refereed publication on cirrus that has explicitly stratified the results according to the latitude of cloud formation and the latitude of cloud measurements. This additional analysis step has led to a better understanding of cirrus formation and evolution and will, no doubt, be used in future studies to better label cirrus clouds.

I do have three major concerns that I would like the authors to address, as I think that there are possibly two environmental factors tha could alter some of their conclusions, or at least open the discussion that could further explain some of the differences that are observed.

My first concern is related to where the clouds were measured with respect to the tropopause. The authors mention that the upper tropopause, in both the ML and HL, were frequently supersaturated but it appears that the analysis has not stratified the cirrus with respect to their altitude with respect to the location of the tropopause, nor is it stated whether any of these clouds were actually in the stratosphere. Given that it appears that there were either no aerosol measurements on the project, or that they are not being evaluated within the context of cloud formation, and given that many times stratospheric and tropospheric aerosol particles can be quite different in their composition, and hence activity as CCN or IN, I strongly recommend that this study add this factor into the analysis. Is it possible that some of these cirrus are polar stratospheric clouds? These are more common in the winter but cannot be completely ruled out in the summer and their formation at high latitudes typical are on a different type of aerosol than in the troposphere,

My second concern is related to more general synoptic features that not only are important in how they can lead to vertical motion, but also how they can lead to horizontal and vertical shear. Very little is said about mesoscale and synoptic scale features like the polar vortex that is weaker in summer than winter but can play a major role in determining the history of the air masses where cirrus are found.

My third concern is that it appears that a Cloud and Aerosol Spectrometer with Polarization Detection (CAS-POL) was also a part of the instrument complement but measurements of the polarization ratio of small ice crystals were not used. A great deal more information about the small crystal tail (Fig. 8) could be extracted by comparing the relative shapes of crystals < 50 µm in the four cirrus types shown in this figure. Rather than just speculating about possible contrail crystals, there is a high probability that this could be addressed quantitively.

Other comments, questions and suggestions.

Line 154: How are convective cirrus identified? Even though the paper says they are excluded from the analysis, at several points in the paper it appears that they have been included in at least some of the analysys (e.g. lines 344 and 487).

Line 167: Here and elsewhere, DMT Inc. should be changed to DMT LLC.

Line 175: Why is the CDP data being truncated at 37.5 um?

Line 199&200: The definitions for the shadowing levels are a bit nebulous. Is 0-25% being classified as unshadowed? Also, the importance of the grayscale is more because it helps constrain the depth of field and not so much shape analysis.

Line 207: Although the PADS data system uses PAS (Particle Air Speed) to denote the pitot measurement, it measures air speed not particle speed, so this should be changed in the manuscript from PAS to TAS.

Line 222,223: I am assuming that the 50µm lower threshold stated here is assuming that when only ½ a dide is shadowed it will be registered as "on", but probably using 100 µm as the lower threshold is a better estimate.

Line 292: "higher sample efficiencies" I think is meant to say "higher sample volumes".

Line 295: I do not agree with this statement that "maximum dimension diameter represents more accurately the spatial extent of ice crystals, which is key for radiative impact calculations." It is the cross-sectional area of crystals that is key to radiative impact calculations. To be sure, our measurement community has not reached a consensus on best dimensions to use in calculating various quantities but unless the authors can show that the end result of using maximum diameter as opposed to area equivalent diameter is marginally different, than I think that this justification should, at the least, be removed.

Line 362: For consistency with the other three categories, change "cirrus measured and formed at mid-latitudes" to "cirrus formed and measured at mid-latitudes".

Figure 8: Strongly recommend showing extinction or area vs size to highlight the radiative impact, on a linear scale. This would show even more clearly the differences in cirrus properties and since the impact on radiative forcing is mentioned numerous times, why not display the data in a way that emphasizes these differences?

This is just a suggestion, but I would ask the authors to consider looking at the particle by particle data from the imaging probes, plot the interarrival times and compare regions where they think there might be contrail cirrus with regions of similar concentration but where contrail like are not present. I think that this might highlight clusters of small contrail crystals embedded in the natural cirrus. Just a thought.