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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice water content of arctic, midlatitude, and tropical cirrus – Part 2: Extension of the database and new statistical analysis" *by* A. E. Luebke et al.

Anonymous Referee #1

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This manuscript represents a potentially valuable contribution by two groups who have collaborated to produce one of the most extensive in situ ice water content (IWC) data sets to date. Measuring bulk IWC in situ is a challenging undertaking. While most reports of IWC are derived from optical array probes (OAP) that estimate particle numbers within size bins and then apply various uncertain mass-size relationships to produce IWC. The measurements reported in this manuscript take a different approach by ingesting the bulk PSD, vaporizing all particles, and then measuring total water with laser hygrometers. Such measurements are rare and highly complimentary to data produced by OAPs.

While I strongly recommend that this manuscript be published at some point, I feel the present version of the manuscript can be significantly improved. I have two primary criticisms. First, the authors choose to explore the variability in IWC as a function of temperature. There have been many such parameterizations since Heymsfield and Platt's seminal 1984 publication. While illustrating temperature relationships is valuable, calling it a parameterization implies that it should be used verbatim in models. I don't think this makes sense at this point when models are moving toward much more explicit microphysics. The authors slice their data by latitude, ignore anvil cirrus, and then attempt to show how the IWC-temperature relationships vary. All of this is done without any mention of the statistical significance of their results. Essentially, the authors argue that their sparsely sampled data sets accurately represents the statistics of the full atmosphere in a particular latitude belt. While this may be true, it is incumbent on the authors to defend this position. For instance, while there may be some 10's of thousands of points in each PDF in Figure 6, how many independent samples are there? Clearly, data points collected from one day to the next are likely independent, how independent are data points collected a few seconds apart in a given cirrus cloud?

Second, the authors seem not to be aware of the argument that has gone on in the ice crystal measurement community in the last number of years regarding the problem of ice crystal shattering with OAP measurements. This issue has been largely resolved and it has been shown that number concentrations of ice crystals in cirrus greater than 1/cc are exceedingly rare and very transient. Yet, the authors show just the opposite in Figure 8. They then proceed to use this relationship to draw their primary conclusion that IWC is mostly dependent on particle number and not on particle size. Basically, the independence on particle size in figure 8 and the dependence on particle number arises from the fact that the particles measured by OAPs have all been ground down to a similar size by shattering.

My specific comments are as follows:

Page 29445, Line 5: The statement that the "microphysical properties of an individual cirrus cloud ... determine whether absorption or reflection will dominate for a particular cloud" is not true. The macroscale properties (IWP and temperature of the top) are the zeroth order determinants of whether an ice layer is going to have a net warming or cooling effect on the atmospheric column. After this, forward scattering becomes important - i.e. microphysics.

Page 29449, Paragraph starting at line 5: Need citations for these field programs where available.

Page 29449, Line18: Should explain the implications of the FISH IWC using saturation mixing ratio. Does this mean that no ambient vapor measurement was made and ice saturation was assumed to difference the total water to get IWC?

Page 29451, Line 20: How much data are rejected? What are the criteria for rejection?

Page 29451, Line 26: What does the ratio of eRH-ice to RH-ce give in circumstances where a cirrus fall streak may be sedimenting through subsaturated air? Unlie liquid cloud droplets, Ice crystals can exist in such subsaturated air for quite some time. See Heymsfield and Donner (1990) for instance. Some data suggest that a sizable fraction of all ice in cirrus reside in subsaturated air. This id likely not well represented by the in situ data because the aricraft tend to spend most of their time near cloud top attempting to measure the interesting dynamics where particles are formed.

Page 29453, Line 25: extinction is really 2 times the cross sectional area of the particle ensemble for these particle sizes and wavelength combinations. Number and effective radius are just different moments of the particle size distribution.

Page 29454, Line 5: Where does this relationship come from? What is the uncertainty in the relationship? the parameters a and b are going to be functions of particle habit and will vary widely.

Page 29454, Line 10: Citation(s) are needed here.

Page 29454, line 20: Declarative statements like this with no citation should not be

made. Where does 12% come from? Is it a theoretical value or derived by comparison to in situ data like the FISH? Perhaps both? The error is a constant percentage over 5 orders of magnitude? Does it account for the lidar ratio uncertainty? For the multiple scattering correction uncertainty? For the uncertainty relating area to mass in a particle size distribution?

Figure 2: I'm not sure what this plot is supposed to be telling the reader. What is agreeing? I suppose it says that the lidar and in situ probes are generating quantities that lie within several orders of magnitude of each other and agree that IWC varies as a weak function of temperature. Beyond that, it is difficult to draw much from this plot.

Page 29455, Line 14: I don't think these conclusions can be drawn from this comparison. The relationship between lidar attenuated backscatter and iwc is complicated by all the uncertainties raised in my earlier comment.

Why use this particlular lidar? There are longer data sets at various locations from millimeter radar. IWC and radar reflectivity are much more directly related than attenuated backscatter and iwc.

These in situ instruments are generally flown with particle probes are they not? Why is it not reasonable to "validate" the hydrometeor occurrence thresholds with the particle probe counts? The particle probes may not be perfect but the presence of hydrometeors is registered with accuracy and that data set has the necessary requirement of being concurrent with the FISH and CLH.

Page 29456, Line 13: The community, I think, has moved beyond T-IWC parameterizations. The value of illustrating these relationships with this dataset is that modelers can create similar relationships from model output and see if their more sophisticated representations of cirrus produce IWC as a function of T as a function of latitude in a manner similar to these measurements.

Page 29456, Line 21: Isn't geography just indicative of different formation mechanisms and the processes that maintain cirrus? Even these processes are not exclusive to a region but a function of the underlying spectrum of large scale dynamics and turbulence. It would be most interesting to investigate how the large-scale dynamics within which cirrus exist vary as a function of latitude.

Page 29456, line 25: The statistical significance of these fits need to be quantified. Are the differences meaningful given the sparsity of the data? I'm not at all convinced that they are meaningful.

Page 29459, Line 5: Without more thoroughly establishing the statisticsal significance of the bimodality, the features and their change with temperature is an intriguing curiosity.

Figure 8: Recent analyses of data sets collected with particle probes that can filter for shattering artifacts from TC4, SPartICus, and MACPEX show very strongly that >1/cc concentrations are extremely rare in cirrus (occur less than 1% of the time). This would argue strongly that the number and size plots in figure 8 have not been filtered for shattering and are erroneous. Any inferences drawn from these plots should be removed from the manuscript.

Page 29461, conclusion 1: This conclusion arises purely from the fact that shattering artifacts have not been accounted for the particle probe data. The conclusion is wrong and has been shown to be wrong in recent analyses of data collected with probes that can account for shattering artifacts.

Page 29461, conclusion 2: The validity of the bimodality needs to be established with significance testing before this conclusion is justified.

Page 29461, conclusion 3: I have no idea how this conclusion emerges from the data presented here. Just because the number concentration is small?

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 29443, 2012.