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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aircraft measurements of microphysical properties of subvisible cirrus in the tropical tropopause layer" *by* R. P. Lawson et al.

Anonymous Referee #3

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This paper reports measurements made in tropical thin cirrus using optical particle spectrometers. One of these, a 2D-S, is an modestly improved version of technology that has been used for more than 35 years. Two pieces of information come out of this submission: 1) 18 particles larger than 100 um were found in 1800 km worth of samples and 2) quasi-spherical particles were measured. The primary relevance of the large crystals and quasi-spherical shapes is that these contrast with what was measured from the same type of aircraft, at similar latitudes and temperatures, 33 years previously. In the previous measurements they didn't find particles larger than about 50 um and the crystal habits were trigonal, not quasi-spherical. It could be concluded that, given the dearth of measurements and the difference in the instrumentation, there cannot be any special significance attached to the differing results. It only highlights the general lack of measurements and accompanying lack of understanding of the microphysics that is unlikely to be resolved until many more measurements are made at other tropical regions.

The abstract states that “Subvisible cirrus (SVC) clouds Ěhave been shown to have a significant impact on the earth radiation budget”. Later, in the introduction, “Although the radiative forcing of optically thin SVC is relatively small, the clouds generally cover a large horizontal extent and are considered to be radiatively significant”. I think that the authors are overstating the importance of SVC somewhat since the use of the word “significant” is subjective unless attached to a relevant comparison. Statements like “observed in the central Pacific Tropics 29 percent of the time” and “calculate observed heating rates of up to 1.0 K day-1, principally in the infrared, and cloud radiative forcing of up to 1.2 W m-2” are misleading because they say nothing about horizontal extent, how large of an area is affected by the heating rate and over what extent of the globe is the forcing 1.2 Wm-2. It is also rather strange to use the term “calculate observed heating rates” since it implies that these heating rates have been measured and the calculations are confirming them. I don’t think this is at all what McFarquhar et al. and Comstock et al. were implying. I believe that it does the scientific community a disservice to propagate these types of numbers, out of context, when it is more than sufficient to express the importance of the new measurements in the context of the limited data that exists on the microphysical characteristics of the SVC and lack of understanding of how they form and evolve.

The WB57 photo is labeled incorrectly as it says the photo is of the aircraft in transit from Houston to Costa Rica. This can’t be correct as there were instruments mounted on the tops of both wings during this project and they were there during the transit. This photo shows no probes mounted on the tops of the wings. There also does not seem to be any reason to show this photo as it is irrelevant to the discussion of the paper. The same is true of the photos of two of the instruments as there is no relevant

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information imparted other than advertising for the manufacturer.

There is no discussion of the significant uncertainty in the sample volume for the small particles. The authors don't explain that the original idea of the 2D-S was to decrease that uncertainty by simultaneously imaging particles in the same volume. Had it worked, this would have eliminated the "donuts" since these would not have been accepted by the "S" (stereo") arrangement. Without this function, the only improvement that has been realized is faster response at higher resolution. The CPI actually does this better than the 2D-S because, as the authors state, the CPI only measures in-focus particles and with four times the resolution of the 2D-S. What then is the improvement? Apparently, the only advantage is a larger sample volume than the CPI. The region of the laser beam where a particle is in focus scales with the diameter squared of the particle and is inversely proportional to the wavelength of the light. Knollenberg originally used a factor of 6 times the radius of the particle squared divided by the wavelength to define the "DOF". This relationship is very uncertain, however, as has been detailed in the publications by Korolev, and may vary by factors of two or more. There needs to be a more quantitative estimate of the errors. Just based on the sample volume of the 2D-S, what are the sampling statistics?

The Korolev algorithm corrects for the size but it also provide the distance of the "donut" from the center of focus, i.e. "donuts" corrected to a diameter of 50 um has a different sample volume than 50 um in-focus particles. Were the concentrations calculated taking this into account? If not, then the average concentrations that are tabulated are probably as a much as a factor of two too high.

How are SVC defined in these measurements? Sassen has a definition based on optical depth derived from lidar but how are they being defined in the current study? The vertical profiles in Figure 4 don't give any information about the optical thickness, only the physical depth and this depth is based on what threshold measured by what instrument? At a later point in the manuscript there are estimates made of the optical depth but how was the extinction calculated?

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Page 10, “The obvious differences between the mid-latitude cirrus data and our observations are the colder temperatures and unusual aerosol chemistry in the upper TTL”. What is unusual about the particle chemistry? The PALMS instrument has certainly measured sulfate and organic carbon in mid-latitude ice crystals. The authors reiterate this chemistry question in the summary but it seems irrelevant given that similar compositions have been measured at higher latitudes.

In the end, the only information that comes from this paper are two things: 1) a very small fraction of the particles are two times larger than previous measurements have shown and 2) there are a lot of quasi-spherical particles compared to trigonal particles found previously. This is mildly interesting, but other than the fact that it requires very large supersaturations to grow the large crystals, not much new light has been shed on the microphysical processes that produce these clouds or how they can be so persistent given the very small crystal size.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6255, 2007.