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Interactive Comment

Interactive comment on "Ice supersaturations and cirrus cloud crystal numbers" *by* M. Krämer et al.

Anonymous Referee #1

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GENERAL COMMENTS: As the scientific community wrestles with the key uncertainties in climate prediction (especially clouds), there remain fundamental questions about the accuracy of humidity measurements in the upper troposphere (e.g. Peter et al., 2006) and the mechanisms of cirrus cloud formation. This paper addresses both issues through analysis of supersaturation and particles both in and out of cirrus clouds. A climatology of upper tropospheric relative humidity is presented for polar, midlatitude, and tropical regions at temperatures of 183 to 250 K. The following findings are reported: 1) Supersaturations are observed as high as the homogeneous freezing threshold (Koop et al., 2000) over the temperature range 183-250 K. 2) At temperatures less than 200 K, a few observations are made where the supersaturation is greater than the homogeneous freezing threshold, but less than liquid water saturation. In fact, the quality-checked relative humidities never exceed liquid water saturation in any of the observations reported here (in contrast to some previous publications). 3) Persistent

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high supersaturations (less than liquid water saturation) are confirmed both in ice cloud and out of cloud. Conclusions include: Supersaturations never exceed liquid water saturation, therefore severe freezing suppression was never encountered in any of the aircraft flights. However, the supersaturation is often greater than the homogeneous freezing threshold, possibly indicating the effect of organic nuclei. The concentration of ice particles is lower than expected, and the authors claim that this low number of ice crystals explains the persistent high supersaturation within cirrus clouds.

GENERAL EVALUATION: This paper presents new, quality-controlled measurements of relative humidity in the upper troposphere. The paper is a novel contribution by extending previous climatologies of RH and particles to lower temperatures. An important and robust conclusion is that (1) persistent high supersaturations are confirmed by accurate water measurements both in ice clouds and out of clouds, and (2) supersaturation is never observed above liquid water saturation in the quality-checked data. These findings are important and should be published. The main weakness of this paper is in the interpretation of the particle measurements. The authors try to explain the measurements with a cloud microphysics model, but there is a flaw. The particle instruments did not detect the largest particles and were susceptible to measurement artifacts due to ice shattering. Both of these effects lead to inaccurate data. The authors' observation of unexpectedly low ice crystal numbers and models with low relaxation times are both erroneous because the particle measurements are incomplete. We request that the authors remove the cloud microphysics from this paper, and focus on the water measurements. The microphysical argument is based on incomplete and misinterpreted particle measurements.

SPECIFIC COMMENTS: As described above in general comments, the main weakness of this paper is in the interpretation of the particle measurements. 1) A significant fraction of the particles are not measured. The authors state on page 21097 that the particle are measured by an FSSP 100 or FSSP 300, which sample particles at size ranges of 1.5-30 micrometers and 0.3-20 micrometers, respectively. These instru-

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ments miss a significant fraction of larger particles. For perspective, Fig. 1 of Field et al. (JAS, December 2007) shows ice concentrations greater than 100 micrometers in (maximum) diameter for temperatures down to 218K. In both midlatitude and tropical clouds, there is a high probability of large particles $>$ 30 micrometers at temperatures down to 218 K.

In section 3.5.2, one has to wonder whether the unexpectedly low ice particle concentrations at low temperatures ($<$205 K) is caused by not sampling the large particles that presumably occur.

2) The authors note that ice shattering can occur, but the flaw is that they suggest that there were no large particles that would cause shattering - even though no instruments flew that were capable of measuring large particles. Shattering will alter the concentrations of particles, particularly the small particles, so the calculations given by equations 1 and 2 are not correct.

In section 3.5.1, the largest concentrations of ice particles are observed at higher temperatures (225-240 K). However, this may simply be due to increased shattering at higher temperatures.

Another specific point is that the precision and detection limits of the water vapor instruments are not mentioned in this paper. This is one reason why flights are classified 'bad' for volume mixing ratios below 5 ppmv. This leads me to suspect that the reason for broader distributions of relative humidity at low temperature (e.g. the blue curve in Figure 8) is limited precision of the water vapor measurements. After all, the water vapor mixing ratios at temperatures $<$ 205 K may be an order of magnitude lower than the mixing ratios at higher temperatures. Can the authors quantify the precision of the relative humidity?

TECHNICAL CORRECTIONS: Section 3.1, page 21098, line 5: change 20.8 to "20.8 h"

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Section 3.2, page 21100, line 7: there is a large range of water vapor in the upper troposphere (1.5 to 100 ppmv), with the low end of the range near the tropical tropopause. Change "upper tropospheric range" to "typical values near the tropical tropopause".

Section 3.4 (page 21103), Figure 5, and Figure 9: what is the "middle" curve? Is it the mean or the median? Please specify. At any rate, the "middle" curve does not accurately fit the data.

Section 3.4, page 21103, line 23: change "supersaturations" to "supersaturation".

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 21089, 2008.