

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

Anonymous Referee #2

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Review of “Ice supersaturations and cirrus cloud crystal numbers”, By M. Kramer et al., for publication in Atmos. Chem. Phys.

Summary: The primary motivation of this paper is to explore physical reasons for the observations of frequent supersaturation in the upper troposphere within cirrus clouds. This is a very interesting paper that utilizes in situ measurements of ice microphysics (number concentration, particle size, etc.) and thermodynamics (temperature, water vapor, and derived humidity) of clear sky and in-cloud scenes, spanning several field campaigns from the Tropics to the high latitudes. Both warm and cold cirrus clouds are studied and treated separately in the statistics. The authors carefully quality control the data to retain only the most robust data samples for analysis. The authors find that conventional ice microphysical arguments can be invoked to explain the observed distributions of supersaturation. They do not rule out other (more exotic) explanations, such as NAT-coated ice crystals, cubic ice formation, the presence of highly polluted air

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(aerosols composed of organics), or overestimates of the deposition coefficient. But, observations of low ice crystal number concentrations offer a robust explanation (given the assumption that the parcel model calculations are representative of observed reality) for high-observed in-cloud humidity for the set of measurements the authors made over multiple field campaigns. This article should be published and will be a useful addition to the scientific literature on upper tropospheric humidity and ice cloud processes. One major weakness of the article is its use of English, which in a few places, impacts the scientific meaning of some particular statements. These are addressed in the specific comments below. But the authors are encouraged to thoroughly revise the manuscript for scientific meaning and clarity to the reader since the review only covers some of these instances.

Specific comments:

p. 21090, lines 2-5: This first sentence is long and contains two distinct thoughts. Suggest separating like “...sometimes exceed water saturation. Up to and more than 200%...”

line 5: change to “...discussion continues on whether...”

line 8: change strictly to strict

line 17: “could hardly be explained” is colloquial, probably better said with “are not explained”

p. 21091, lines 6-7: “form not as soon as” is awkward

line 10: “In the case...”

lines 11, 14 and 15: hyphens are not necessary

Same general place: another useful reference on modeling RH distributions is a recently published paper by Comstock et al. (2008), JGR. They make the argument that fewer large ice crystals can greatly impact RH distributions.

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Line 23: change interacts to interact

p. 21092, line 23: artifacts

line 27: “in the frame of this discussion” is awkward, how about “in this context”?

p. 21093, line 10: change frame to framework

line 20: how about “Experiments”? Experimentals just doesn’t work.

p. 21096, line 27: is the uncertainty a bias or a root-mean squared value? Please clarify

p. 21097, line 8: delete “without such.”, and add “Otherwise,”, to the beginning of the sentence

line 18: change to “we estimate”

Regarding ice crystal shattering effects in the particle probes, will this impact the interpretation of the parcel modeling results to follow? Can these effects be “estimated” and then “propagated” through the calculations to see if the conclusions are generally similar? This would help address Reviewer #1s concerns about the model calculations with regard to the observations. Also, perhaps another useful paper has been published on ice shattering by S. Davis et al. (2009), JGR.

p. 21098, lines 7-9: both warm and cold cirrus are partitioned in this study. Has a similar analysis been performed for “cloud type”, i.e., frontal, lee wave, convective detrainment, and in situ formed thin cirrus in the TTL? Or are the results more dependent on air temperature rather than cloud type?

line 12: were these spirals or stacked flight tracks?

Line 25: delete “firstly”

p. 21099, line 1: which temperature range? INCA? C-F? Or cold vs. warm cirrus? Not clear.

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p. 21100, lines 8-14: Regarding the Jensen et al. observations, did they fly through an extraordinarily dirty air mass near land, or was the air transported from fires to the flight level? Probably should go back to paper to see how dirty the air really was, could be a “freak” sampling case because of really dirty air.

p. 21101, line 17: “They may either portrait the...” is very awkward. Not sure what authors are trying to say here.

p. 21103, line 8: change to “By knowing the minimum...” Furthermore, “in dependence” is not clear. Should it be “that depends on temperature”?

lines 19-20: supersaturation

line 23: same as above

p. 21104, line 1: change to “...in case fewer ice...”

line 5: “and thin ice clouds needs and...” unclear

p. 21105, line 22: are the authors referring to the “simulated” number here? Unclear.

p. 21106, line 24: “temperatures”

p. 21107, line 16: change to simultaneous

line 20: change “maybe” to “may be”

line 25: change to “(around or lower than 1 cm/s)”

p. 21108, line 1: To be consistent with use of words, shouldn't it be “Scenario (ii)”?

lines 6-7: not sure what authors mean by “general mechanism”.

Lines 14-15: “Here, we presented...”. And change “strongly” to “thoroughly”

Line 18: “explicable” is not clear

p. 21109, were there any observations of vertical velocity in this field campaign to

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validate “Scenario (i)?

p. 21110, line 4: Acknowledgments. Also, “giving” is better than “leaving”

Another reference on the deposition coefficient that may be of use (at least for the warm clouds in this study) is a paper by Magee et al. (2006), GRL. They found lower values than previously assumed.

A paper by Strom et al. (2003), ACP shows a nice analysis of RH as a function of IWC, similar to other studies referenced, but may also provide an unique perspective based on how tenuous/thick the ice cloud is.

p. 21117, Table 1: What about papers that use Microwave Limb Sounder retrievals? Several papers by W. Read et al. that may be worth considering.

Fig. 3 (lower left) and Fig. 7: Why is it that the processed data in Fig. 3 show some points down to RH = 0%, but yet, in Fig. 7, they are not there? Is that because there were so few points, that in the PDF plot in Fig. 7, they don't show up because the binning starts above some minimal value of frequency of occurrence?

Fig. 8: A suggestion, lots of papers present RH PDFs on a log-scale. Can the authors consider making two sub-panels, one with the same coordinates at preent, and another on a log scale, for easier comparison to papers that have log-scale PDFs?

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

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