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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation of large ($\simeq$100 μm) ice crystals near the tropical tropopause" *by* E. J. Jensen et al.

Anonymous Referee #2

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General Comments/Suggestions:

This paper represents an appropriate combination of observations and modeling focused on the origins of large ice crystals in the upper troposphere. The research described here is technically strong and leads to the conclusion that such ice crystals, though rare, do form in or near the tropical tropopause. The calculations suggest that growth of pristine ice crystals to ˜100 um in the time available requires relatively large supersaturations. Unfortunately, given the disparity in measured water vapor concentrations, it is not possible to determine the actual supersaturation experienced by the crystals with any reasonable certainty. The authors have explored the various options thoroughly, but one is still left with nagging questions about the origins and growth of the ice crystals in the upper troposphere. The comments below highlight a couple of

the issues that the authors, indeed the scientific community as whole, may wish to consider.

Specific Comments:

a. The measurement of small amounts of water vapor in the atmosphere is clearly an issue that must find resolution. The discrepancies in humidity values resulting from balloon-borne and aircraft measurements is not new, and this paper offers little in the ways to overcome the problem. The last paragraph in Section 2, in particular, leaves a reader hanging with little hope of a resolution. As a person not directly involved in the research program, I may not be aware of current efforts in the community and so I may be raising naïve questions. I am surprised that little attempt was made to install a frost-point device on the aircraft, if for no other reason than to eliminate the differences in measurement platform from the equation. Have no head-to-head comparisons been done in a laboratory, where one can be sure that each instrument is measuring the same air, and where the time constraints of field programs can be avoided? It is not my purpose here to suggest methods, rather to remind the authors of this paper that a purpose of research publications is to disseminate information to a broad scientific audience. Could the authors at least speculate on how future research programs might be structured to reduce the uncertainties in the measurement of water vapor concentration in the upper troposphere? Without better measurements, we are not likely to resolve issues about ice crystal growth from field measurements.

b. The growth of single crystals of ice at low atmospheric temperatures and pressures, discussed in Section 3, is of concern for the current paper, as it is for the community at large. Aside from the uncertainties associated with degree of supersaturation in which the crystals are embedded, we also know little about the growth mechanisms and how to model them under such conditions. The capacitance model serves at best as an approximation to the growth of faceted crystals. Of even greater concern is assigning faceted crystals deposition coefficients of unity (Table 1). The very existence on facets implies growth limitations due to surface kinetics. I understand that the goal

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of the modeling exercise was to push the growth to an extreme and show that problems rectifying the observations still exist. Still, it is not fair to violate fundamental principles of crystal growth while discounting alternative explanations. Aircraft measurements yield at best instantaneous snapshots; no knowledge of the growth history can be determined directly. Until the observations are better constrained, it makes little sense to draw conclusions about the growth of the crystals. For instance, on page 6303 (Lines 14, 15) it is stated, with little real justification, that water molecules must readily diffuse across the crystal surface. I suggest deleting that entire paragraph (for being too great an extrapolation, unwarranted by the data available).

Technical Corrections:

Page 6296: The acronym TTL needs to be defined. A logical place to do so is inside the parenthetical explanation about tropopause on lines 2 and 3 of this page.

Page 6298, Lines 7 and 10: Air may be cold, but temperatures are then “low”.

Same page, Line 13: The comma after “relatively large ($>$65 um)” should be deleted.

Page 6300, Line 1: Define RHI.

Same page, Lines 13 and 15: The acronym HWV is used as an adjective. Thus, move the word “instrument” to behind the parentheses on line 13, and insert this word after HWV on line 15.

Same page, last line: Clarify what is meant by the term “quasi-equilibrium”. In the presence of large RHI values, ice crystals are certainly not in equilibrium. What purpose does adding the prefix “quasi” serve? Please explain or change the terminology.

Page 6309, Lines 16-18: The intent of this sentence is unclear. Mass accommodation coefficient is a kinetic concept relevant during growth or evaporation. How, then, is it justified to jump to the conclusion that the aerosol is in equilibrium with the environment? Please clarify.

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References: Please proofread these carefully. Microphysical is spelled incorrectly in the title of the paper by Lawson et al. (2007), and I suspect a couple of names are spelled incorrectly as well in the reference list.

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Interactive Comment

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