

Interactive comment on “Freezing thresholds and cirrus cloud formation mechanisms inferred from in situ measurements of relative humidity” by W. Haag et al.

Anonymous Referee #2

Received and published: 28 July 2003

GENERAL COMMENTS

In general I found this an interesting paper. It raises some interesting points, but leaves several questions only half answered. This paper should be revised to answer them better. This paper may be acceptable for ACP with appropriate revisions. There is perhaps too much emphasis on the qualitative statistics (shapes of distributions) and too little analysis of the microphysics behind the statistics. I also find the focus on relative humidity less than compelling. Relative humidity is a derived quantity from water vapor and temperature. Some of the 'features' of the distributions (a specific 'slope' with increasing RHI) may simply be a product of the temperature distribution. the paper would be stronger with less emphasis on these distributions and more discussion

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of physics.

Perhaps the most interesting kernel in this paper, which the authors could highlight more than just at the end, is that the distribution of Relative Humidity may help discern the nucleation process and its variability. More detailed attention to the microphysics would help make this nice result better, and it could be better expressed.

SPECIFIC COMMENTS

Several specific things would improve the manuscript.

First, I believe a little more concentration on the physics and less on distributions of unconstrained quantities (Relative Humidity) would add to the understanding. Particularly when the model does not have sedimentation for water vapor, and must obviously permit too many clouds to form because they simply can also evaporate. I would be very interested to see what the model results would look like if some simple sedimentation was proscribed (Heymsfield and Iaquinta 2000, or Gettelman et al 2002).

A. J. Heymsfield and J. Iaquinta. Cirrus crystal terminal velocities. J. Atmos. Sci., 57:916-938, 2000.

Gettelman, A., W. J. Randel, F. Wu and S. T. Massie, Transport of water vapor in the tropical tropopause layer Geophysical Research Letters, 29:1, 10.1029/2001GL013818, 2002

This relates to the first conclusion, that transport is dominant. I think this result is partially a product of the model, and basically is linked to the Clausius-Clayperon equation: a cloud forms in the model every time it gets cold, and if water vapor does not fall out, then these clouds will continue to form. The microphysics also controls the size distribution of ice particles and hence the sedimentation of water. Your model should include many of these processes? If so, they should be discussed.

Second, the paper highlights the difference between homogenous and heterogenous nucleation in a model. Yet the heterogenous nucleation is simply an imposed constraint

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to match the observations (critical RHI of 130%). No difference in aerosol or CN between the NH and SH is demonstrated in the paper. So what is the physical cause? Some references to the heterogeneous nucleation thresholds of various possible CN would be very useful in this regard.

Continuing along these lines, the authors postulate that a few big ice nuclei in the northern hemisphere could account for the difference in the peak out of cloud RHI and peak in cloud RHI between the model and observations. Can a detailed microphysical model produce this? From the INCA observations, can you determine if there are at least 10-3 per cm³ of these IN to make clouds in the NH but not in the SH?

Finally, no conclusions are drawn from the global model results. I am not entirely sure of the point of including the model results. Agreement with observations is good at 250hPa, but given the logarithmic vertical axis, I do not think that agreement is good at 200 hPa. Why the difference? The model also has RHI of well over 170%, which is not observed during INCA. Is this realistic?

TECHNICAL CORRECTIONS

p3268, l23: 'necessary prerequisite FOR developing the ability...' would sound better.

p3269, l12: 'thermodynamic' not thermodynamical

p3269, l24-25: 'We thereby...' this is awkward and confusing. What is the 'methodology' you are referring to?

pg 3270, l15: awkward (the sentence doesn't make sense).

pg 3271, l11: Actually, you are comparing a Eulerian model (Gettelman et al 2000) to Lagrangian trajectory calculations. Also, you are not focusing on the water vapor distribution, but on the relative humidity. More relevant to the present work are other recent Lagrangian trajectories such as Pfister et al (2001) or Gettelman et al (2002-reference above).

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L. Pfister et al. Aircraft observations of thin cirrus clouds near the tropical tropopause. J. Geophys. Res., 106(D9):9765-9786, 2001.

pg 3271, l21: please provide a reference to the model here (perhaps Haag et al 2003).

pg 3272, l3: why is this is main 'area of interest'. Is it the location of the INCA NH campaign? What happens if you use the SH as well??

pg 3272, 19: what else would interact with the particle phase other than vapor? are you referring to the aerosols interacting with the particle phase? (unclear).

pg 3272, l24: what happens if you change the freezing threshold? it would be nice to see a sensitivity test to a different threshold (e.g.: 120% or 140%).

pg 3274, l2: why dont you just sediment some particles. Heymsfield & Iaquinta 2000 and Gettelman 2002 have provided various simple methods for sedimenting particles based on size distributions or just total bulk ice water that might apply here. Again, it would be nice to see a sensitivity experiment here.

pg 3274, l9: That the results are insensitive to these changes perhaps leads me to suspect that the Clausius-Clayperon equation fixes the slope.

pg 3275, l23: how were the trajectories selected? Location? regular grid?

pg 3276, l1: what happens to the water vapor? does it just all go into clouds? does the size distribution evolve (coalescence, coagulation)?

pg 3278, l21: in HET, the cutoff is specified to match these observations is it not? So the agreement is proscribed a priori.

pg 3280, l28: I don't see where this is really addressed appropriately in sec 3.2.2. I would like to see (as noted in general comments) some discussion of what types of particles or composition might cause the difference. You speak of 'pollution': are we in the stratosphere or troposphere? Is it just a larger concentration of land producing mineral dust or organic aerosols? Or is it combustion products (soot, hydrocarbons)?

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Or is it aircraft emissions (more likely if only in the stratosphere)? There are interesting questions you might be able to answer here with a greater focus on the microphysics.

pg 2385, l15: the difference is hard for me to see in fig 7. I see differences that just look random. Also, does the GCM have sedimentation (I assume it must).

pg 3285, l27: I do not think the data and model agree well at 250 hPa, given the log scale.

Interactive comment on Atmos. Chem. Phys. Discuss., 3, 3267, 2003.

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