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Interactive comment on "Supersaturation, dehydration, and denitrification in Arctic cirrus" *by* B. Kärcher

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General Comments:

This manuscript presents an idealized simulation of Arctic cirrus formation driven by synoptic-scale motions. The calculations of HNO_3 uptake include the potentially important process of diffusional burial in growing ice crystals. The conclusions concerning supersaturation within the cloud and dehydration could be generalized by showing the sensitivity to vertical wind speed and imposing descent at the end of the simulation such that all cloud ice is sublimated. Additional references to recent papers discussing these effects should be added. The HNO_3 uptake and denitrification calculations are novel, and perhaps more detail could be provided describing the uncertainties and

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sensitivities in these calculations.

Specific Comments:

1. The author states that he is simulating a case study of a cirrostratus cloud sampled in a field experiment. Yet nowhere in the manuscript are any measurements shown or discussed. Section 3.1 implies that the initial conditions are based on measurements, but the linear temperature and potential temperature profiles lead one to believe that these are actually idealized profiles. It should be made clear that the simulation is idealized and in no way constrained by (or confirmed by) observations.

2. A recent publication [Jensen et al., 2005] presented simulations and observations of large supersaturations within a cirrus layer under similar conditions near the tropical tropopause. As discussed in that paper, the buildup of ice supersaturation depends on how large the cloud ice surface area density is. It would be useful for readers interested in the topic if this manuscript relates the current results to those presented by Jensen et al., and it would be helpful in this regard if the surface area densities in the simulated cloud were presented.

3. The calculations of dehydration and denitrification would be more satisfying if the column was driven first by slow ascent (cooling) and then by descent (heating) such that the cloud ice was entirely sublimated by the end of the simulation. This would ensure that the final H_2O and HNO_3 profiles represent irreversible dehydration and denitrification. Also, it would be interesting to see how varying vertical wind speed (with the corresponding changes in ice crystal concentration, size, and fallspeed) affects the magnitude of dehydration and denitrification. I believe a figure showing these sensitivities would be a useful addition.

4. The finding that an Arctic cirrostratus cloud can significantly dehydrate the upper troposphere is not particularly surprising. Furthermore, a single idealized simulation does not really address the issue of whether cirrus have a strong impact on the upper

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tropospheric water vapor budget. Quantification of the effects of cirrus requires either large numbers of such semi-Lagrangian simulations driven by analyzed meteorological fields or three-dimensional simulations. The manuscript should clearly state that the results presented just suggest the potential for important dehydration effects.

5. The finding that the cirrostratus cloud can essentially completely denitrify the upper troposphere is very interesting, but I was left with several questions about the calculations: How important is the HNO_3 burial to the denitrification? It would be useful to indicate how large the trapping efficiency is in the simulated cloud. Would you still get significant denitrification without burial? A related question is whether the ice crystals take up more than a monolayer of HNO_3. If they don't, then presumably burial is unimportant. How well constrained is the trapping efficiency? My assumption is that it is poorly constrained. In general, since the plausibility of HNO_3 burial in ice is disputed, I believe the paper would greatly benefit from further discussion of these issues.

6. Page 1848, lines 13-15. The author states that the strong sensitivity results from the fact that uptake is neither fully controlled by diffusional burial nor by the escape rates at the ice surface. Can more detail be provided here? Why does the balance of effects result in such strong sensitivity?

7. Page 1844, lines 20-24. The author argues that the presence of small crystals (down to 0.5-1 microns) is consistent with measurements. However, current instrumentation is very nearly useless for measurement of crystal size distributions in the 1-10 micron range. The morass of problems includes ambiguities associated with Mie oscillations in FSSP-type measurements and shattering of crystals impacting the instrument inlets. The statement in the manuscript should at least be qualified to indicate that the in situ measurements are problematic.

Editorial Comments:

1. The author should add a reference to Kelly et al. [1991]. This paper hypothesized

that dehydration by cirrus at high latitudes was responsible for the observed asymmetry in northern/southern hemisphere upper tropospheric water vapor concentrations.

2. The fonts in Figures 2 and 5 need to be larger. It is difficult to discern the features discussed in the text. For example, in the upper right panel of Figure 2, I can't tell that ice crystal concentrations increase with height. Perhaps simply enlarging these figures in the typesetting process will solve the problems.

References:

Jensen, E. J., L. Pfister, T.-P. Bui, A. Weinheimer, E. Weinstock, J. Smith, J. Pittman, D. Baumgardner, M. J. McGill, Formation of a Tropopause Cirrus Layer Observed over Florida during CRYSTAL-FACE, *J. Geophys. Res., 110*, doi:10.1029/2004JD004671, 2005.

Kelly, K. K., A. F. Tuck, and T. Davies, Wintertime asymmetry of upper tropospheric water vapor between the northern and southern hemispheres, *Nature, 353*, 244-247, 1991.

Interactive comment on Atmos. Chem. Phys. Discuss., 5, 1829, 2005.