

Interactive
Comment

Interactive comment on “Formation of solid particles in synoptic-scale Arctic PSCs in early winter 2002/2003” by N. Larsen et al.

Anonymous Referee #2

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General comments

This paper presents and discusses balloonborne measurements of PSC size distribution, optical backscatter, and chemical composition obtained in Kiruna, Sweden, and Sodankyla, Finland in early December 2002. The data were collected under quiescent dynamic conditions, i.e. they are not influenced by mountain lee waves that occur quite often in these locations. The data show that a small number of solid particles were almost always present, even in PSC layers whose optical properties were dominated by those of liquid STS droplets. Calculated values of extinction and color index were shown to be consistent with data from the SAGE III satellite instrument. Theoretical microphysical simulations were performed to check the consistency of the data with a surface-based homogeneous freezing scheme for forming solid particles from liquid STS droplets. A good match was found between calculated and observed size distri-

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butions and particle HNO_3 uptake if the theoretical nucleation rate was reduced by a factor of 20. The paper is well written, the data and calculations are presented clearly, and there are liberal references to other work in the field. I think this will be a very useful addition to the literature. A major comment (to follow) is that the surface-based homogeneous freezing nucleation scheme seems to have been accepted as correct, and there is little discussion of the possibility of heterogeneous nucleation of solid particles.

Specific comments

1. I find it a little odd that the authors have adopted the surface-based homogeneous freezing nucleation scheme as the 'standard' for producing solid PSC particles, even though they had to reduce the nucleation rate by a large factor (20) to get agreement between model and measurements. I would like to see a little more discussion about the possibility of heterogeneous nucleation. Would the model vs. measurements comparison be similar for a plausible heterogeneous mechanism, or would the results look totally wrong?
2. Please explain the discrepancy between the SSA size distribution parameters observed on Dec. 6 at 606 K (from Figure 2, median radius = 0.034 micron, $\sigma = 1.7$) and those used for the theoretical calculations (median radius = 0.07 micron, $\sigma = 1.4$), which supposedly were derived from heated inlet OPC data on the same day.
3. Consider moving the discussion that 12 ppbv HNO_3 is consistent with MIPAS observations from section 4 to earlier in the paper. The 12 ppbv value is first used to compute TNAT in Figure 1, and appears in several other places as well.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 2485, 2004.

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