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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice supersaturations exceeding 100% at the cold tropical tropopause: implications for cirrus formation and dehydration" *by* E. Jensen et al.

E. Jensen et al.

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1. Interpretation of laboratory measurements of homogeneous freezing

We have clarified that the Koop et al. [2000] parameterization is based on laboratory measurements with a wide variety of aerosol compositions.

We should indeed have included non-equilibrium effects in the calculations shown in Figures 2-4, and we have done so in the revised manuscript. Even with an accommodation coefficient of unity, the peak supersaturations are increased by 5-10

We have added discussion of the kinetic effects on peak supersaturations earlier in the

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paper. Specifically, we discuss this issue in reference to the Koop threshold curves shown in Figure 1.

2. Chemical composition of supercooled aerosols

As the reviewer points out, the available measurements are insufficient to allow definitive statements about the aerosol composition. As clarified in the revised manuscript, only a eleven particles were sampled by PALMS, and these were not in the layer with highest supersaturation. We have stated in the revised manuscript that we cannot constrain the chemical composition of aerosols in the supersaturated layer.

We agree that the simulations shown in Figures 5 and 6 are just confirming the results shown in Karcher and Koop [2004], and we should have specifically stated this with a proper reference.

3. Water saturation and plausibility of relative humidity measurements

Based on the vapor pressure expressions given by Murphy and Koop [2004], the peak ice supersaturations observed do indeed exceed saturation with respect to liquid water. In fact, at the minimum temperature observed (187.13 K), water saturation occurs at an ice saturation ratio of 2.05. We fully agree that significant supersaturation with respect to liquid water is unlikely, and we have added some discussion of this point in the section dealing with the plausibility of the measurements. We also note that, give the uncertainty in supercooled water vapor pressures, the peak ice saturation ratio of about 2.3 does not necessarily imply supersaturation with respect to liquid water.

In the simulations shown in Figure 3 with peak ice saturation ratios greater than 2.05, droplets actually did activate in the model and subsequently freeze, but they were insufficiently numerous to prevent buildup of significant supersaturation with respect to liquid water. This point is discussed in the revised manuscript.

4. Frequency of occurrence of high ice supersaturations

In both the conclusions and abstract, we have highlighted the expected rarity of ice

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supersaturations as large as those reported in this manuscript.

5. Other comments

We agree with and have implemented all of the suggested changes except the following:

We have left the paragraph starting p.7435 line 28 ending p.7436 line 6 where it was since it served as a segue into the following paragraph in which we describe the use of in situ measurements in this paper.

p.7438, lines 1319: We have discussed the issue of increased peak supersaturation due to non-equilibrium aerosol composition effects elsewhere in the revised manuscript.

p.7447, line 17 p.7448, line 3 Since the decreased accommodation coefficient has a very different physical effect (increased nucleation threshold) than heterogeneous IN (decreased threshold), we do not think discussing the IN effect is appropriate here.

Interactive comment on Atmos. Chem. Phys. Discuss., 4, 7433, 2004.