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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. The reviewer suggested we provide a few more details about the cloud model simulations. We now specify that we are using a 1-D model. The original manuscript described the cloud simulation technique of tracking numerous individual ice crystals, which is different from either bulk or bin methods.

2. The reviewer requested clarification of the supercooled water vapor pressure issue. We have added a sentence in the revised manuscript pointing out that the uncertainties in the vapor pressure over supercooled water are large enough to account for the discrepancy between the ice supersaturations reported here and those inferred

from laboratory measurements of the aerosol activity at which sulfate freezing occurs. This explanation would indeed be invalid if the AIDA cloud-chamber measurements are correct. However, as mentioned in the manuscript, we feel that additional laboratory measurements are needed to confirm the AIDA results, and we believe it is still worth discussing the vapor pressure uncertainty issue.

Note that explaining the large observed supersaturations by assuming the vapor pressure expressions are in error does not require assumption of large vertical velocities. The only model simulation shown using modified vapor pressure is Figure 3d with a wind speed of 10 cm/s.

3. The reviewer requested clarification of the discussion concerning Figure 4 and aerosol composition. As we have attempted to clarify in the revised manuscript, the simulations shown in Figure 4 simply show the effect of varying the number of aerosols available for homogeneous freezing. Since we don't have information about how the threshold for freezing depends on aerosol composition, we do not think going into further detail is warranted.

As specified in the revised manuscript, PALMS only sampled 11 particles in the descent, and these were not actually at the temperature minimum. In the revised manuscript, we have qualified the discussion of PALMS measurements indicating that we cannot really constrain the aerosol composition with these limited measurements. As described in the original manuscript, NMASS and FCAS measurements indicated at the size distribution of aerosols during the descent was typical. For such a size distribution, more than 10/cc aerosols would have frozen at the measured supersaturation if they had thresholds for homogeneous freezing indicated by laboratory experiments conducted using sulfuric acid aerosols. The point of Figure 4 and the associated discussion is to show that the vast majority of available aerosols would need to have compositions other than sulfuric acid, thus preventing ice nucleation in order to allow the high supersaturations to build up.



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As clarified in the revised manuscript, no ice was detected anywhere during the descent.

4. Regarding Fig. 6: We have now included points from simulations with moderate cooling rates and and lower accommodation coefficients such that the peak supersaturations are consistent with the measurements. We agree that the 40 K/hour simulation is probably unrealistic for the PreAVE case, and have added a comment stating this. However, we feel it is worth including to show the range of possible results for the full range of cooling rates that frequently occur in the atmosphere.

The peak ice saturations shown in Figure 6 occur just after the onset of ice nucleation. Once ice nucleation begins, numerous crystals nucleate, and the ice surface area rapidly builds up sufficiently to halt the increase of supersaturation. The peak supersaturation achieved just before ice nucleation is only about 2% lower than the peak supersaturation occurring just after nucleation. Our assumption is that the observed supersaturation was not quite high enough to trigger ice nucleation in this case. We have attempted to clarify this issue in the revised manuscript.

5. As requested, we have clarified how the 1-D simulations were used to simulate the global distribution of water vapor. We specifically state that the trajectory simulations end at latitudes and longitudes throughout the tropics.

6. We have increased the size of text on Figure 7.

7. The reviewer suggested that we indicate which of the mechanisms listed is the most plausible explanation for the large supersaturations. We feel that the potential mechanisms explaining the large supersaturations are all simply hypotheses at this point, and we do not think it is appropriate to put one hypothesis forward as the most plausible. As we have emphasized in the manuscript, additional laboratory and field measurements are required before definitive explanations are possible.

8. We have corrected the spelling error noted and defined the symbols.



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

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