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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.

Anonymous Referee #2

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Review of "Ice supersaturations exceeding 100% at the cold tropical tropopause: implications for cirrus formation and dehydration" by E. Jensen and coauthors.

General Comments:

This manuscript presents measurements of high ice supersaturation (>2.0) near the tropical tropopause and uses a cloud model to evaluate the validity of these measurements. The authors present a series of cloud simulations and physical reasoning to explain the presence of high ice supersaturation and also discuss the implications of these measurements to cloud formation and dehydration in the tropical atmosphere.

High ice supersaturation has been measured previously both in and out of cloud, but never have such high values been reported in the atmosphere or laboratory. My first response is to question whether these measurements are physically possible. The authors do a fairly good job at convincing the reader that the measurements are accurate and give several explanations as to how these high supersaturated conditions could exist in the atmosphere. I feel this manuscript should be published based on its relevance to current scientific issues concerning the tropical tropopause region and sound scientific method. However, there are several points listed below that should be addressed before final publication of the manuscript.

Specific comments:

Section 3: Implications for ice nucleation

Model simulations: it may be helpful to the reader to give a few more details about the cloud model simulations. For example, what kind of model (1D, 2D?), bulk or explicit microphysics, basic model assumptions? Just a few sentences would be enough.

In reference to Fig. 3: Through a series of model simulations, the authors show that using the usual model assumptions and increasing the vertical velocity it is difficult to achieve the observed supersaturation (Si) values. The authors then show that the only way to reach the peak values of Si is to increase the ice nucleation threshold by increasing the vapor pressure over supercooled water using a temperature dependent factor. To reach the high Si observed, an unrealistic vertical velocity was required. Then the authors say that this assumption is likely wrong anyways because AIDA laboratory measurements show that the error in the supercooled water vapor pressure is not that large. Can the authors explain more clearly why the uncertainty in vapor pressure is so important? How uncertain is it? Otherwise, the relevance of adding results of unrealistic model simulations (Fig. 3d) to the discussion is unclear.

Aerosol composition/concentration: This discussion is interesting and may have significant implications for modeling cloud formation. Most cloud models assume that the

ACPD

4, S2635–S2638, 2004

ice forms as a result of nucleation of sulfate drops because there is little information available for other constituents. As you point out, there is evidence from the limited in situ data near the tropical tropopause that organic/sulfate compounds exist in this region. You simulate the change in aerosol composition by reducing the number of aerosols available for nucleation. Aren't you really just decreasing the aerosol number concentration? This doesn't really show the effects of composition and impurities on ice formation. The PALMS measurements indicate that there were only few particles detected but not in the layer of interest. How few? Could it be that there simply weren't any ice nuclei to initiate formation in the layer where the high Si was observed? Was cloud detected in the region where the PALMS detected particles?

Figure 6: In this figure you show that reduced aerosol accommodation coefficient increases the peak saturation ratio. But at the same time, the ice concentration is increasing. The 40 K/hour simulation is unreasonable for the PreAve observations, and it seems that the other simulations do not predict high enough Si. So I am left wondering how important this coefficient is for this particular case. It seems that if you reduced the accommodation coefficient further, then more ice would form, but no ice was detected in the observations. Please clarify the connection with the measurements.

Section 4 Implications for TTL cirrus formation

Please provide more information on how the authors used a 1D model to simulate global distribution of water vapor (Fig. 7). These results are interesting and surprising, but it is difficult to understand them without understanding the modeling methods.

Figure 7: The font on Figure 7 is too small and the contours are difficult to read. Since most of the discussion in this paper is about ice supersaturation, it might also be interesting to plot the global distribution or frequency of Si.

Section 5 Conclusions

The authors provide 3 different sets of modeling simulations that try to explain high ice

ACPD
4, S2635–S2638, 2004

supersaturation measurements, but do not leave the reader with a good sense of which is the most plausible explanation. It seems that the reduced accommodation coefficient is the most likely, but there still seems to be some discrepancy there. Naturally it is difficult to draw definitive conclusions based on only one profile, but some speculation would be appropriate. You might want to emphasize the need for more laboratory measurement on realistic atmospheric particles and in situ measurements near the tropical tropopause region

Technical Corrections

Page 7438, Line 6. “Uncertainty” is misspelled. Page 7440, Line 23. Define symbols N, rm, and sigma.

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