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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Rare temperature histories and cirrus ice number density in a parcel and one-dimensional model" *by* D. M. Murphy

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Response to reviewers

I thank both reviewers for their detailed reviews. In addition to the changes they requested, the mass shading in Figure 4 has been corrected. The draft version had erroneous color shading. The model calculations were not affected.

Reviewer 1 (E. Jensen):

1) Emphasize the importance of small departures from the Koop et al. approximation.

This has been added to the abstract and to the top panel of Figure 1.

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2) Provide more detail about the temperature fluctuations in the model.

This has been done.

3) Figure 2 is plotted versus frostpoint temperature – add nucleation temperature?

A sentence has been added to the figure caption.

4) Page 10707, do newer laboratory measurements provide evidence for variations in supersaturation threshold with aerosol composition?

I've looked and haven't found anything that directly addresses this question. I did add a reference to the Zobrist et al. (2003) paper on freezing of high molecular weight polymers that indicates a variation in water freezing activity with molecular weight.

5) Page 10708, the Kraemer study included outflow cirrus and used FSSP instruments susceptible to shatter. Also, the sampling of cold cirrus is very sparse. . . the most one can conclude from the measurements is that there is a lack of compelling evidence for a strong increase of ice concentration with decreasing temperature.

The outflow cirrus possibility was already mentioned; I've added a mention of shatter. As requested I've removed the limits from Figure 2. Statements about the observed temperature dependence have been softened: the abstract has been changed to say "not observed" instead of the stronger "opposite of observed."

6) There seem to be two mechanisms for generation of fallstreaks discussed in the manuscript: (1) the diversity of ice concentrations produced by nucleation at different altitudes because of the differences in temperature histories, including layers with low ice concentrations that permit growth of large crystals that lead to fallstreaks; and (2) the "mother cloud" effect whereby ice crystals diffuse out of the high concentration layer into supersaturated air below where growth rates are rapid and large crystals result.

I appreciate this clarification from the reviewer. The abstract and discussion have been changed to more clearly differentiate these mechanisms and two more (entrainment

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and the classic heterogeneous nucleation).

7) Page 10710. In ATTREX, the layers with high ice concentrations were embedded within deep layers with low ice concentrations.

Comment added.

8) Page 10711: Thin layers in ATTREX were remarkable; this model provides an explanation

I agree but don't want to push the agreement too far because the 1-dimensional model has imposed cooling profiles that are inherently arbitrary. I think that it would take a 3-D model with dynamically consistent temperature fluctuations to prove this agreement.

9) Page 10711: Not convinced that curved fall streaks show that shear cannot be simulated with a 1-D model.

I partly agree. Curved fall streaks show that shear is present. The point made by Spichtinger and Gierens (2009) is not that wind shear per se cannot be simulated in one dimension but that the shear interacts with and modifies the small-scale dynamics. This is another aspect to my reply to point 8 above about needing a three-dimensional model for some questions.

Citations:

The suggested citations have been added to the paper. I made some extra model runs with an accommodation coefficient that varied with the saturation ratio as suggested by Zhang and Harrington (2014) and it made almost difference to the initial ice number; this is now noted in section 3.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 10701, 2014.