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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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Review of "Rare temperature histories and cirrus ice number density in a parcel and one-dimensional model" by D. Murphy

This manuscript provides an analysis of important physical interactions and sensitivities that emerge from detailed microphysical models of ice nucleation, growth, and sedimentation in cold cirrus. The results are clearly described and have important implications for our understanding of cold cirrus properties and representations of cirrus processes in models. The manuscript should be suitable for publication in ACP, but I would like the author to consider the points discussed below.

1. **Page 10703, lines 8-11:** The point made here (and later) about the importance

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of even small departures from the Koop et al. approximation seems important, and perhaps it should be mentioned in the abstract.

2. Section 2: In the simulation setup discussion, it would help to provide a little more detail on the specification of temperature fluctuations. For example, what is the amplitude of the temperature fluctuations and how was this chosen? Are the temperature fluctuations imposed consistent with observations? The strong sensitivity of ice concentration to temperature fluctuation amplitude has been shown in numerous previous studies as well as this manuscript (Fig. 2), and this is clearly an important factor. In particular, it may well be the amplitudes of the higher frequency waves that are most important for ice nucleation.

3. Fig. 2: The temperature dependence is shown by plotting results as a function of frostpoint temperature, which seems reasonable. However, particularly for homogeneous freezing, the ice nucleation occurs at a somewhat colder temperature. It might be worth mentioning this in the manuscript and providing the temperature difference (about 3 K) for a representative frostpoint temperature.

4. Page 10707, lines 10-21: My impression is that a plethora of laboratory nucleation rate measurements have been published since the 2000 Koop paper. Is there any evidence from the body of experimental work for variations in the supersaturation threshold with changes in aerosol composition? I suspect that comparing results from different laboratory experimental techniques would be fruitless because the differences associated with aerosol composition are likely small. Perhaps the differences required to significantly affect ice concentration are so small that they wouldn't even by discernible in laboratory experiments that vary aerosol composition but use the same experimental setup. This point would be worth stating.

5. Page 10708, lines 10-13: It is my understanding that the Krämer et al. (2009) study did include all cirrus data, with no attempt to remove anvil (outflow) cirrus. Also, Krämer et al. measurements were made with FSSP instruments, which are highly susceptible

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to shattering. Particularly for anvil cirrus with large crystals present, the very high ice concentrations reported may well be attributed to shattering artifacts. Further, the lower limit reported by Krämer et al. may just be determined by the FSSP sample volume and the integration time used for distinguishing clear and cloudy samples. I would recommend omitting the lower and upper limits.

A related issue is the limited sampling of cold cirrus. Prior to the ATTREX measurements reported by (Jensen et al., PNAS, 2013), TTL cirrus measurements (from WB-57 and Geophysica campaigns) showed no evidence of narrow layers with abundant crystals. However, even in the ATTREX dataset, these high-concentration layers are quite rare. I do not think it is reasonable to argue that the nucleation processes were fundamentally different in the different campaigns. Rather, it is more likely that different stages of the cirrus lifecycle were sampled. In my opinion, 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.

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. My impression is that the author is suggesting that the first mechanism likely dominates under realistic conditions, and I would tend to agree with this suggestion. This issue might be worth a bit of additional discussion.

7. Page 10710, lines 14-16: It might be worth noting here that in the ATTREX observations the layers with high ice concentrations were embedded within deep layers with low ice concentration (Jensen et al., 2013). In other words the regions with low ice concentrations occurred both above and below the layer with high concentrations. This observation is consistent with the hypothesis of fall streaks initiated by freezing events

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that produce low concentrations, but it is inconsistent with the mother-cloud hypothesis.

8. Page 10711, lines 4-14: One of the most remarkable features in the ATTREX observations of TTL cirrus is the very narrow layers (vertical thicknesses as small as a few m!) with high ice concentrations. It might be worth pointing out that the simulations presented here provide an explanation for the formation of such narrow layers.

9. Page 10711, lines 20-23: I'm not convinced that the curvature of fallstreaks caused by wind shear necessarily implies that one-dimensional models cannot simulate the microphysical properties in fallstreaks. As alluded to in the manuscript, small-scale convection driven by shear and/or radiation-driven thermal instabilities are the potentially important processes that 1-D models can't treat.

Citations:

1. Page 10702, line 25: The author might want to cite Stackhouse and Stevens (JAS, 1991) who showed the sensitivity of cirrus radiative effects to ice crystal size (and number) prior to the papers already cited.

2. Page 10703, lines 14-16: The author cites the central conclusion (dominance of heterogeneous nucleation) of the Cziczo et al. (2013) paper. Since the focus here is on cold cirrus, it might be worth also mentioning the result of the Froyd et al. (ACP, 2010) study that for cirrus in the uppermost tropical troposphere, the ice crystal residuals were very similar to the ambient aerosol, with no evidence for enhancement of particle compositions like mineral dust that might suggest heterogeneous nucleation.

3. Page 10703, lines 20-21: The author might consider citing DeMott et al. (JGR, 1997) who explored the competition between heterogeneous and homogeneous nucleation through the parameter space of IN concentration and vertical wind speed.

4. Page 10703, lines 28-29: The author might consider citing Jensen and Toon (GRL, 1994) who showed the sensitivity of ice number concentration produced by homogeneous freezing to cooling rate.

6. At various points in the manuscript, the sensitivity to accommodation coefficient is mentioned. It might be worth discussing the results of Zhang and Harrington (JAS, 2014) who investigate surface kinetic effects and their impacts on deposition coefficient in detail.

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