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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Parameterizing the competition between homogeneous and heterogeneous freezing in cirrus cloud formation – monodisperse ice nuclei" *by* D. Barahona and A. Nenes

D. Barahona and A. Nenes

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Responses to Reviewer #1

We thank the reviewer for their positive feedback and comments. Our responses to the comments are given below.

1. The authors state that Kärcher et al. (2006) consider either pure heterogeneous or pure homogeneous freezing. This is not correct. Also Kärcher et al. consider the competition between the two mechanisms and explicitly calculate the effect of heterogeneous freezing on the occurrence and microphysical prop-

erties (number, size) of ice crystals frozen homogeneously. The authors should clarify this in the manuscript. They should motivate their study by discussing the new aspects compared to the parameterization by Kärcher et al. In the present version of the manuscript the improvements or different approaches with regard to Kärcher et al. are not clearly highlighted.

Indeed, Kärcher, et al. (2006) (referred to as K06) can describe simultaneous homogeneous and heterogeneous nucleation. However, numerical integration is required to account for the effect of previously frozen crystals on homogeneous nucleation by modifying $\frac{dS}{dt}$ (i.e., the "freezing time scale", t_{fr}, using K06 terminology). Given that the t_{fr} in K06 is fixed (assumed to be known), its dependency on heterogeneous freezing thresholds and IN number concentration cannot be resolved explicitly. As a result, the effect of previously frozen particles manifests through the redistribution of water vapor between homogeneously and heterogeneously frozen crystals at the peak supersaturation, but neglects the change in freezing probability and size distribution of the homogeneously frozen crystals. K06 recognize this limitation, and argue that adjusting the t_{fr} can account for freezing pulse changes; this however requires numerical integration, as the size of heterogeneously frozen crystals at the homogeneous freezing threshold is not known.

Our approach is fundamentally different from K06, and completely eliminates the need for prescribing a freezing timescale and numerical integration, as the freezing probability is explicitly dependent on cloud formation conditions (T, V, p), aerosol and IN characteristics, as described in section 2.

The above discussion will be included in the revised version of the paper.

2. The authors extensively discuss the calculation of the limiting number of ice nuclei that would prevent homogeneous nucleation. What is the intention? The parameterization includes not only cases where homogeneous nucleation is suppressed. Cases where homogeneous nucleation occurs with the number

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of homogeneously frozen crystals reduced due to the presence of ice nuclei are also relevant. The authors should balance the discussion in order to include also the other cases.

Previous work, using numerical parcel simulations, determine "heterogeneously-dominated" and "homogeneously-dominated" regimes and correlate them with cloud formation conditions (V, T, and, P). As we show, the contribution of each freezing mechanism to crystal number concentration, *depends only on* $\frac{N_{IN}}{N_{\rm lim}}$. This is a significant theoretical advancement; by providing an expression for N_{lim}, we eliminate the need for defining "dominant" regimes of ice formation, and unravel most of the physics of ice formation within our simple parameterization. To illustrate this, we demonstrate that at high V and low T (conditions where homogeneous freezing typically dominates), ice crystal formation can be affected by IN, provided that N_{IN} is high enough. Thus, $V$and T cannot uniquely define heterogeneously and homogeneously-dominated regimes, and, parameterizations using such approaches are inherently limited.

We have now included the discussion above, as well as additional comparisons of IN effects on N_c when homogeneous nucleation is not surpressed.

3. The authors compare their results with the outcome of other studies, particularly Gierens (2003) and Liu and Penner (2005). Nevertheless, comparisons with the Kärcher et al. (2006) study are missing. The work of Kärcher et al. and the present study have very similar objectives. Therefore, the authors should also include discussions of the results of Kärcher et al. in the manuscript.

Good point. We have now included a comparison with K06.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 15665, 2008.

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Interactive Comment