Atmos. Chem. Phys. Discuss., 8, S4943–S4948, 2008
www.atmos-chem-phys-discuss.net/8/S4943/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling of cirrus clouds – Part 2: Competition of different nucleation mechanisms" *by* P. Spichtinger and K. M. Gierens

Anonymous Referee #2

Received and published: 16 July 2008

In this paper the authors use a simplified approach to describe heterogeneous ice nucleation within a two-dimensional cloud parcel model, which includes sedimentation of ice crystals, to study the competition between homogeneous and heterogeneous ice nucleation during cirrus formation and evolution. They demonstrate that, under certain circumstances, a two-layer structure may form within the cirrus cloud, which results from the continuous sedimentation of ice crystals that allows supersaturation built up in the upper layer of the cloud, leading to continuous homogeneous nucleation. The authors perform, within a limited range, sensitivity analysis on temperature and updraft velocity, and discuss the effect of velocity and temperature fluctuations on the resulting relative humidity distribution. The authors describe their results with considerable detail. The subject of this work is within the scope of ACP, and the conclusions are insightful and of interest to the scientific community. However some general issues should be addressed before publication in ACP.

General comments:

The authors discuss three different cloud regimes: homogeneously dominated, heterogeneously dominated, and equal competition. These regimes however are only defined based on a qualitative description of Figs. 4 and 5. Thus, it is not clear what "dominance" in this context refers to (e.g., crystal number, area, volume, or cloud depth occupied). The only quantitative criteria for the existence of such regimes is given in terms of the critical concentration predicted by Gierens (2003), which is based on box models simulations and it is not clear whether it can be extended to the case including sedimentation. As shown by several authors [e.g., DeMott, et al., 1997; Kärcher, et al., 2006] the critical concentration of ice nuclei is a strong function of the cloud forming conditions (which is also evident in the equation of Gierens (2003)). Therefore the use of the words like low, medium, and high ice nuclei concentrations to define the "dominant" regime is not appropriate. Thus, it is not clear what the authors mean with the homogeneously or heterogeneously dominated regimes, and may be better to describe them in terms of measurable quantities, like for example, crystal number concentration and relative humidity, which I think can be extracted from the results of their simulations.

It is also noticeable that the number concentration of homogeneously frozen crystals in the upper layer of the cloud (as presented in Figs. 2 and 4) seems to be constant in time (around 50 L-1), even after several hours of depletion by sedimentation. Is the concentration of background liquid aerosol assumed to be constant? If so, why to assume an infinite source of liquid aerosol, but a finite source of ice nuclei? Is the aerosol being continuously provided by an entrainment processes? To clarify this point it would be appropriate to calculate the total number of frozen droplets during the 5 hours period and compare it to the total number of liquid aerosol initially present in the

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parcel.

The authors did not consider aggregation in their model arguing that cirrus clouds at temperatures below -38 C are mainly composed of small crystals with low terminal velocities. This statement, however, seems to contradict the results of their work as it is the terminal velocities of large crystals what ultimately produces the layered cloud structure. From this, it does not seem plausible that a cloud with a structure like the one presented in the middle panels of Figure 4 would be mainly composed of small crystals, and that aggregation would be negligible. A comparison of the timescales for aggregation and sedimentation would clarify this issue. This also brings up an important point regarding the effect of the competition between homogeneous and heterogeneous nucleation on the overall crystal size distribution of the cloud. What are the typical sizes encountered? I see a lack of discussion on this regard in the paper, which is important to clarify the above mentioned issues regarding the presence of small crystals.

Specific Comments:

Page (9063) line (9) This is not completely accurate; the cited references specify a threshold for the ice nuclei concentration not for the vertical velocity.

Page (9065) line (6) This assumption would be valid only in the case in which there is only one ice nuclei population with a RHihet that is quite below the homogeneous freezing threshold. It would be good to briefly discuss the implications and assumptions behind the simple parameterization used.

Page (9065) line (5) The word "purpose" is misspelled.

Page (9065) line (10) Is the deposition coefficient still 0.5? Is it the same for all crystals (heterogeneous and homogeneous)? Please specify its value.

Page (9066) line (15) The size distribution of the ice nuclei is not irrelevant, it is just assumed to be monodisperse and chemically homogeneous (so all particles have the

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same freezing threshold).

Page (9067) line (8) Please define ISSR somewhere in the paper.

Page (9068) line (24) This description seems a little ambiguous and is repeatedly used in this section. The authors interpret the reduced number of crystals as a consequence of a smaller "overshooting" over a homogeneous freezing threshold. This threshold, however, is not defined in the paper (which may be ambiguous since the nucleation rate of crystals changes with supersaturation, making it difficult to define a unique threshold). It is better to explain this as the reduction of the peak nucleation rate due to the lower maximum supersaturation achieved in each freezing event.

Page (9069) line (10) The word "enough" is misspelled.

Page (9070) line (5) The word "significantly" is misspelled.

Figure 4. There is no disussion on the case with na,2=50 L-1 in the text. If there is something significant about this particular case it should be pointed out in the text, or else be removed from the figure.

Figure 5. Right panel. There is a "kink" in the crystal concentration isolines, at about 60 minutes (which is evident in all panels of Figure 7) which is not present in the left panel of Figure 5 or in Figure 4. Please explain what is the source of the discontinuity.

Page (9071) line (20) The authors argue that the maximum concentration of crystals at the top of the cloud is similar in all cases as it changes from 200 to 250 L-1; however they also argue that it differs in the middle of the cloud where it decreases from 180 to 110 L-1. It seems that both changes are comparable but little attention is given to the top of the cloud (where most crystals are produced).

Page (9072) line (6) The word "homogeneous" is misspelled.

Page (9072) line (9) I find it ambiguous to describe the onset of homogenenous nucleation as the "touching" of two lines in the figure. Please make the connection to the

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physical process, describe which value of the nucleation rate is assumed to define the homogeneous threshold, and the implications of this assumption.

Page (9073) line (6) What does it mean "further evolution" in this context? Are the authors referring to the effect of heterogeneously frozen crystals on ice crystal concentration after the initial pulse? Gierens (2003) formula only describes crystal nucleation rather than cloud evolution. Please clarify.

Page (9073) line (23) The word "nucleation" is misspelled.

Page (9075) line (25) The authors imply that "polluted" means a higher concentration of heterogeneously frozen crystals; such connection however is not been made in the paper.

Figure 8. Caption Please specify whether total crystal concentration or only homogeneously frozen crystal concentration is presented, and also the concentration of ice nuclei.

Page (9076) line (15) Can the heterogeneous threshold be surpassed in each grid box at different times due to temperature or velocity fluctuations?

I disagree with the statement of the authors that a more sophisticated parameterization would be insensitive to fluctuations as it may be dependent of the cooling rate in the same fashion as for the pure homogeneous case.

Page (9076) line (25) Is the variability given with respect to the mean or to the value without fluctuations? please specify.

Page (9077) line (21) This statement is confusing. Does it mean that the temperature fluctuations become stronger or that their effect is more noticeable?

Page (9078) line (13) The word "homogeneous" is misspelled.

Page (9078) line (23) What kind of "exotic phenomena" are the authors referring to? Please be more specific.

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Page (9080) line (9) It should me mentioned that this conclusion is consistent with several published works, i.e. [Kärcher and Lohmann, 2002; Ren and Mackenzie, 2005; Kärcher, et al., 2006; Barahona and Nenes, 2008].

Page (9081) line (23) The word "layer" is misspelled.

References

Barahona, D., and A. Nenes (2008), Parameterization of cirrus formation in large scale models: Homogenous nucleation, J. Geophys. Res., 113, doi:10.1029/2007JD009355. DeMott, P. J., D. C. Rogers, and S. M. Kreidenweis (1997), The susceptibility of ice formation in upper tropospheric clouds to insoluble aerosol components, J. Geophys. Res., 102, 19575-19584. Kärcher, B., J. Hendricks, and U. Lohmann (2006), Physically based parameterization of cirrus cloud formation for use in global atmospheric models, J. Geophys. Res., 111, D01205. Kärcher, B., and U. Lohmann (2002), A parameterization of cirrus cloud formation: homogeneous freezing of supercooled aerosols, J. Geophys. Res., 107, 4010, doi:4010.1029/2001JD000470. Ren, C., and A. R. Mackenzie (2005), Cirrus parameterization and the role of ice nuclei, Q. J. R. Meteorol. Soc., 131, 1585-1605.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9061, 2008.