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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the representation of immersion and condensation freezing in cloud models using different nucleation schemes" *by* B. Ervens and G. Feingold

Anonymous Referee #2

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In this work the authors use parcel model simulations to study the sensitivity of the cloud ice crystal concentration and the ice water content to the ice nucleation scheme in immersion and condensation freezing. The authors conclude that the different ice nucleation schemes are likely to disagree when extrapolated outside the conditions used to retrieve their parameters. This may lead to significant variation in ice crystal number and ice water content during the cloud development. This is an interesting study, relevant for the scientific community, however some clarifications are needed before it can be published.

1 General Comments

1- In the study the authors set the maximum IN concentration and use the parameterizations to calculate a relative freezing fraction. Most of the simulations are run with $N_{IN} = 4$ L^{-1}. Although a sensible choice, using a different value will lead to a different feedback on the supersaturation, and to different conclusions in the the parcel model simulations.

2- In their description of the different schemes the authors focus on the representation of the distribution of contact angles on the immersed IN. It is know that droplet volume also has an impact on nucleation rates and it would be appropriate to discuss how this may fit into each of the parameterizations. If the impact of the droplet volume on nucleation rates is neglected then this must be explicitly stated.

3- The expression used for the "deterministic" approach neglects the effect of supersaturation on ice crystal production and should not be used for water-subsaturated conditions. Even if a deterministic approach is used, it is known that supersaturation is an important factor in condensation freezing. In fact, the authors mention several empirical parameterizations that include supersaturation but end up using a temperature-only dependent parameterization, which is an error.

4- It seems that the only distinction between the internal vs. externally mixed "soccer ball" cases is the sampling of contact angles from the overall distribution, which is assumed to be the same in both cases. The authors sample 20 subdistributions from the overall contact angle distribution for the externally mixed case. It seems that if they would use a larger sample the internally and externally cases would converge. A more consistent approach would assume several independent contact angle distributions for the externally mixed case.

5- It is not clear what assumptions about the water activity around the IN (i.e., inside the droplet) are used to calculate immersion and condensation freezing rates.

2 Specific Comments

Page 7169. Line 24. Please reference at least some of the studies showing temporal dependency at constant T.

Page 7172. Line 5. Please specify what model is used to calculate r_{germ} in condensation and immersion.

Page 7172. Equation (3). This expression is valid only if J is constant (box model calculations). Please describe what approximation is taken to calculate $\int J\mathrm{dt}$ for the parcel model calculations.

Page 7172. Eq. (4). Please explain how this equation is derived. I also find confusing what the authors mean by the “overall freezing probability”. Is this the fraction of frozen droplets? Or is it the expectancy of finding a droplet frozen in the population?

Page 7175. Eq. (6). This expression is suitable only for activated droplets. For subsaturated regimes any credible expression must include the effect of supersaturation.

Page 7175. Line 19. Do the droplets grow during the simulation? What are their sizes? Are they assumed to be in equilibrium?

Page 7177. Line 15. The expression used to calculate the nucleation rate assumes equilibrium which would not be the case once the droplets grow beyond their critical diameter. Please explain how the water activity inside each droplet is calculated.

Page 7178. Eq. (9). Is this an approximation or an equality?

Page 7179. Line 5. This may not be so obvious. One can always find a value such that $J_{1\theta} = \sum S_j J_j$.

Page 7179. Line 18. This sentence is confusing. How is increasing J with increasing D_{IN} a test for time-independency?

Page 7182. Line 24. Using a model that does not depend on supersaturation, even

if empirical, for water-subsaturated regimes is not correct. The authors are looking at the impact of neglecting the effect of supersaturation rather than the impact of using a deterministic approach.

Page 7182. Line 27. The described behavior results from the specific conditions and assumptions used in a single simulation and must not be generalized.

Page 7184. Line 8. It must be mentioned that the feedback on supersaturation only affects condensation freezing. Once activated the droplets are no longer in equilibrium and the water activity is not determined by S_i.

Page 7185. Line 8-10. These are a very narrow temperature ranges. How do they compare to observations? Would this indicate that the contact angle distributions are too narrow?

Page 7188. Line 25. It is not clear what is meant by stability in this context.

Page 7190. Lines 1-4 and Lines 20-25. These conclusions refer only to the specific assumptions of the simulations. For example, setting a larger maximum IN concentration would lead to stronger feedbacks and likely to larger differences in IWC between nucleation schemes.

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