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Atmospheric Chemistry and Physics Discussions

Interactive comment on "A numerical study of aerosol influence on mixed-phase stratiform clouds through modulation of the liquid phase" *by* G. de Boer et al.

Anonymous Referee #2

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The paper explores how variations in CCN properties can affect the persistence of mixed-phase clouds through alteration of the intensity of ice nucleation via immersion mode. The study is based exclusively on the numerical simulations from a 2D cloud-resolving model and the sensitivity of simulated cloud is tested with respect to the soluble mass fraction of the CCN as well as the composition of the insoluble component. The latter affects the ice nucleating properties of aerosol immersed in cloud droplets. The ice nucleating efficiencies of various insoluble components, such as soot and various dust types, are taken from an independent study. The paper follows the footsteps of dozens of studies from the last few years that used model sensitivity studies to gain insight into various processes in mixed-phase clouds. For the approach to yield useful

results, however, this study needs to do a better job in justifying the range of explored parameters and analyzing the simulations, as indicated in comments below.

1. If I understand the simulation setup correctly, in each model run a soluble mass fraction and insoluble mass type are assumed to be the same for CCN of all sizes. Noting that nearly all of the CCN are of sub-micron sizes, with about half of them having radius smaller than 0.1 micron, and knowing that dust particles tend to fall into the coarse-mode aerosol, is it realistic to assume that every aerosol particle contains a dust particle? Furthermore, the majority of "laboratory-derived parameters" used here for dust were obtained for particle sizes of hundreds of microns and extrapolating them to particles 1000 times smaller calls for providing some justification. I wonder if it will be more realistic to assign dust fractions only to coarse mode CCN (called "large" mode in this paper) and what effect that change would have on the results?

2. I like the attempt to analyze the sensitivity of the immersion freezing parameterization (Eq 1) to various parameters, but I find much of the discussion in section 2.2 to be confusing. It would be more instructive to illustrate how the freezing rate changes (in relative terms) when uncertain (e.g., Bhi) or variable (e.g., Vd, Ta) parameters are altered within a probable range. For example, the freezing point depression contribution for aerosol with a dry size of 6 microns is practically irrelevant here since there are virtually no such large particles in the presented simulations. For the vast majority of considered CCNs, the freezing point depression for droplets larger than 1-2 micron would be very close to zero. On the other hand, varying Bhi by five orders of magnitude or so would be comparable to changing the droplet temperature (deltaTa) by about 10 degrees. This brings up a question: Is a series of 2D model simulations needed to demonstrate, for example, that the insoluble aerosol type affects the cloud structure more that accounting for freezing point depression? The study currently considers 17 different simulations, many of which are not discussed in any details. Dropping a few simulations from consideration would also can help to make the discussion of others more structured and systematic.

3. The presentation and the analysis of the results can be improved. Surprisingly for a study on aerosol effects on clouds, the paper does not present any results on simulated droplet or ice number concentrations. (A caption to figures 6,8, and 11 list ice number concentration but the corresponding panels are not found in these figures.) Providing this information would help to put the widely varying properties of the simulated clouds in perspective. For example, does any combination of IN and CCN properties result in a more realistic simulation than others? Do some aerosol properties lead to completely unreasonable cloud microphysics? Figures, in general, need some work. Many green and red lines are hard to distinguish, a number of 2D plots are too small, and captions often do not correspond to the content shown.

4. Is an overcrowded flowchart in fig 14 really needed to make a point that the system is complex and that many interacting processes operate in a mixed-phase cloud? There are many other processes that could be included and more arrows could be drawn as well. What is not clear is how this study contributes to the refinement of this conceptual model. The authors are upfront that they do not quantify the strength of the indicated feedbacks. So what is the purpose for this diagram? Does this study identify any new processes or feedbacks not previously reported? It is not clear that it does, but if so, the paper should make a stronger case on what these processes are, how they change our understanding of the mixed-phase cloud system, and when they would be most important to account for. Basically, the paper should tell readers why they should care about these effects.

Minor comments:

p.22060, ln5: remove “the”

p.22060, ln13-16: Sentence is not clear.

p.22061, ln13: Findeisen

p.22064, ln15-16: Be more specific on how you set your size grid, or just say that you

use 40 bins. Saying that you split them in two groups of 20 does not add much.

p.22068, ln7: “from 10 to 35”

p.22070, ln9: relative to what simulation?

Table 1, figures: use consistent notation in text, table, and figures. E.g., it is “A” in table, “a” in text (p22066); there is a mixture of dT and deltaT in text and figures, etc.

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