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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simulating mixed-phase Arctic stratus clouds: sensitivity to ice initiation mechanisms" *by* I. Sednev et al.

Anonymous Referee #3

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GENERAL

The paper "Simulating mixed-phased Arctic stratus clouds: sensitivity to ice initiation mechanisms" by Sednev et al, focus on the problem of modeling longevity of arctic mixed phased clouds. They look at different microphysical processes (both liquid phase and ice phase cloud processes) to determine the important factors that contribute to the longevity of arctic mixed phased clouds. They specifically look at different types of heterogeneous ice nucleation processes and how different newly created ice crystal size influences the cloud development. It is also pointed out the significance of explicitly treating the Bergeron Findeisen Process in larger scale models. In addition they discuss the importance of how to use, or define, effective radii of ice crystals, in order to compare modeling results to measurements.

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Interactive Discussion
Discussion Paper

The paper is a contribution in furthering understanding the topic and modeling of Arctic mixed phased cloud. However, I have some concerns on the focus on ice initiation mechanisms in this paper for the following reasons:

The authors are conducting sensitivity modeling studies, based on MPACE observations, assuming two different types of ice initiation processes. For the nucleation process of ice crystals from water vapor the Meyer's formula is used to calculate the number of ice crystals. On page 11761, line 14, it is state that "if liquid phase is not involved, ice initiation is parameterized with the Meyer's formula" (i.e freezing mode must therefore be deposition freezing). However, it should be noted that the Meyer's formula was developed based on measurements with a Continuous Flow Diffusion Chamber (CFDC) that measures freezing from both deposition and condensation freezing (freezing where CCN that has activated a droplet also serve as an ice nuclei (IN), i.e. from liquid phase). Therefore, when the Meyer's formula is used as it is, more than one basic ice initiation process is accounted for. The explicit ice freezing modes that the Meyer's formula is based on (deposition and condensation) should therefore be stated in the paper.

Another issue is that IN measurements, with the same type of CFDC as used to develop the Meyer's formula, was conducted during the MPACE study [Prenni et al., 2007]. Prenni et al., determined that the Meyer's formula overpredict IN concentrations by a factor of 26. Note that the Meyer's formula is developed from measurements at mid-latitudes levels where the concentration of dust (one of the main contributors to the IN population) typically is higher than at Arctic latitudes. Prenni et al. also found that measured IN concentrations during MPACE had a smaller temperature dependence than is in the Meyer's formulation. Hence they developed a new formulation, but with the same functional form as the Meyer's equation. And, again, this formula is for both condensation and deposition mode freezing. I suggest including a reference to Prenni et al. and a small discussion on how the sensitivity study might compare with the IN measurements. Even, if possible, use the parameterization developed from the specific

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MPACE measurements.

Further, some of the basis for the main conclusions in the paper is not described well enough (see specific comments below).

SPESIFIC COMMENTS:

I would suggest using a different word for newborn droplets (for example newly activated)

Page 11759, line 20: I suggest using the term deposition/condensation freezing when describing the ice initiation process from water vapor.

Page 11761, line 13: Suggest changing "...some of which (IIP and BFP) are of special ..', to "...for where IIP and BFP are of special ....

References related to Equation (2) should be included in the main text and not only in the appendix. I also suggest that the discussion of ice initiation mechanisms in the appendix could be moved to the main text, since this aspect is one of the main focuses in the paper. The effective radius definitions in appendix could also be moved to the main text.

Page 11763: A_t, B_t, C_t and P_t have wrong subscripts. It should be "l" and not "t".

Page 11763: It is more likely that the second mode of the AP distribution (the larger sizes with median radius of ˜1 micrometer) would be comprised of dust/soil like particles, and not only pure ammonium sulfate. The particles in this mode (if assumed to be dust) can still act as CCN though [e.g. Mahowald and Kiehl, 2003], since the sizes are large (giant CCN). In addition, Koehler et al. [2007] show that dust with only slight soluble material on them is hygroscopic and can serve as CCN. This can later then also lead to condensation freezing.

Page 11766, line 13: Can you explain why precipitation flux is lower and droplet concentration is higher in the W3 case compared to W4. Intuitive, I would think precipi-

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tation would increase with coagulation and droplet concentration would decrease with coagulation.

Page 11767, paragraph starting on line 3: Statements in this paragraph are not well founded based on the information given in the paper. It is not clearly shown or discussed why and how the CCN spectrum shape is more important than the process of coagulation. Simulations with a different (monomodal or less broad?) distribution could maybe support this statement? In addition, the next sentence is somewhat out of place. It is new for the reader that it is assumed that "water-water, ice-water and ice-ice interactions may be relatively minor for the MPACE single-layer mixed phased clouds." No simulations showing ice-water and ice-ice interactions are shown yet, thus I cannot see how only the warm cloud simulations can validate the abovementioned assumption. This also brings up a new question. Is coagulation included in the "ice" microphysics sensitivity runs? This is not clearly stated in the paper.

Page 11769, line 8: It is not clear for me, looking at Figure 12 that I3 predict significant higher crystal concentrations than I2. I suggest reducing the maximum range in color table to better illustrate differences in Figure 12 (and Figure 11).

Discussion on page 11773 and 11774: It is stated that the second ice initiation process "crucially depends on the shape of the AP distribution and not only on the concentration of cloud droplets". First, it is not clearly shown how the broadness of the AP distribution affects the ice initiation process compared to a lesser broad AP distribution. Second, what about IN concentrations in the droplets? For a droplet to freeze, an IN must be present. A larger droplet will have a larger freezing probability due to higher concentration of IN in the droplet (could be acquired for example through coagulation with droplets containing IN, or scavenging of IN). Would there be a difference in freezing probability from a large droplet originating from a broad CCN distribution, or a large droplet grown due to coagulation?

Page 11779, line 8 and 9: What is the difference in meaning of "activation of CCN",

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and "nucleation of droplets"?

There should be more references to newer studies of ice initiation processes. There has been a lot of work on this the last 14 years (since Deshler and Vali, 1992). Among some of the newer ones are for example DeMott et al. [2003], Archuleta et al. [2005], Mangold et al. [2005], Field et al. [2006], Mohler et al. [2006], Koehler et al. [2007] and Marcolli et al. [2007].

Page 11781, line 18: Two main mechanisms for ice generation by supercooled droplets are mentioned. What about condensation freezing? (see Vali, [1985])

FIGUERS AND TABLES:

Figure 11 and 12: I suggest reducing the maximum range in color table to better illustrate differences in the figures.

Table 8: In table heading, include equation number. In table caption, use correct subscript for plates and dendrites.

TECHNICAL COMMENTS:

"simplier" on page 11758, line 12 should be spelled simpler

Page 11767, line 13, CN should be CCN.

Page 11777, line 10, an "l" is missing in complexity.

Page 11785, "Combining" is spelled wrong.

Use a different word for newborn (for example newly activated....)

Acronyms are defined unnecessary multiple times.

Replace all occurrences of undersaturated with subsaturated

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Interactive Comment