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Atmospheric Chemistry and Physics Discussions

Interactive comment on "New particle-dependent parameterizations of heterogeneous freezing processes: sensitivity studies of convective clouds with an air parcel model" *by* K. Diehl and S. K. Mitra

Anonymous Referee #3

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The authors improve an adiabatic air parcel model by implementing new or updated parameterizations for various heterogeneous freezing modes. These parameterizations are based on laboratory studies, many of which were performed within the INUIT project. Improvements in the description of ice formation parameterizations are a welcome contribution to the current literature on mixed-phase and ice cloud models. However, the current manuscript should be improved in order to represent a more useful contribution to the current literature.

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Interactive Discussion

Discussion Paper

Major comments

1) Comparison to previous studies

a) Several previous studies compared the effect of various freezing modes and other parameters (e.g. IN composition) on mixed-phase cloud properties. Only a few of them are mentioned in the introduction; however, in the result section, the current results are not discussed in the context of previous findings. Such studies include but are not limited to (Fridlind et al., 2007; Eidhammer et al., 2009; Ervens et al., 2011; Kulkarni et al., 2012; Hiron and Flossmann, 2015)

b) How realistic are the assumptions of 1% or 10% of all particles being IN? How are such ratios treated in previous models and justified based on observations?

c) How do the findings compare to observations? For example, several studies have discussed a likely predominance of immersion and/or contact freezing in the atmosphere (de Boer et al., 2010; Lance et al., 2011 and others)

d) Your study seems to imply that contact freezing is likely not important. How do these findings agree with previous studies (Ladino et al., 2013, and references therein)?

2) Novelty and uncertainty of results

Some of the conclusions of the current manuscript are similar to those in studies mentioned in Comment 1. What are new results in the current study? What are the most uncertain and sensitive parameters that affect cloud properties?

3) Mass- vs. surface –based parameterizations

Several recent laboratory and model studies describe ice nucleation based on surface area of ice nuclei (IN) (Hoose and Möhler, 2012, and references therein). In the current study, some of the data used in the previous study have been converted into mass-based parameterizations. I think a surface based parameterization is more intuitive since the number of surface sites determines the IN efficiency. What is the justification

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of a mass-based parameterization? In particular, in the case of coalesced particles the surface area might change but the mass remains the same which leads then to ambiguous interpretation of the IN activity of the resulting particles. This discrepancy should be discussed and better justified. Assumptions on deposition freezing (p. 16418, l. 16; p. 16430, l. 7) even contradict a surface-based approach. This contradiction should also be better explained.

4) Application of the parcel model

a) I agree with the authors that a parcel model is a very useful tool to learn about microphysical processes and feedbacks. However, it should be clearly stated which limitations such a model presents. For example, does the presence of growing ice particles feed back on the supersaturation? How realistic are such conditions in the atmosphere when particles might fall out of the parcel?

b) Some of the details of the parcel model should be better explained. I got confused in the introduction when it was stated that 'dry air is mixed into the parcel' (p. 16403, l. 24; p. 16406, l. 14). By definition, an adiabatic air parcel does not have any entrainment of surrounding air. How are these mixing effects described within the model?

c) On p. 16420, l. 8, precipitation from convective clouds is mentioned. This is confusing, if indeed no precipitation is simulated in the model.

5) Presentation of the figures Almost none of the figures can be understood solely based on their captions. All figure captions should be improved and details should be explained which freezing mode results etc shown.

Minor comments

p. 16406, l. 22: Why does the ice spectrum have the same bins as the aerosol particle/liquid drop one? Aren't ice particles much larger? Or do you mean that the size-spacing (i.e. doubling in each bin) is the same?

p. 16412, l. 25: Which experiments? The ones by Hoffmann and Kiselev?

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p. 16413, l. 2ff and Figure 3: Why can a linear behavior of freezing temperature and particle size be assumed? What is the physical basis for this assumption?

p. 16415, l. 13ff: I am not fond of calling unactivated particles 'dry'. I don't think there is any dry particle in the atmosphere since likely all particles contain some water upon hygroscopic growth. 'Unactivated' might be more appropriate.

p. 16417, l. 22: I do not understand this. The activated fraction should dependent on the temperature. Please, clarify.

p. 16423, l. 2: I do not see color differences in the tables. Or do you refer here to Figures? (which ones?) p. 16425: A key reference on bacteria concentrations in clouds is missing here (Delort et al., 2010)

p. 16428, l. 25: Do you mean the number of ice particles formed on feldspar and montmorillonite or the total number concentrations of dust particles?

p. 16429, 1st paragraph: Does this discussion refer to results in a figure?

p. 16431, l. 27: Thermodynamically, most of the water should be in vapor form. You could simply replace 'total water content' by 'total amount of water in condensed form' (or sth similar)

p. 16433, l. 16: I do not understand this discussion. It is true that the concentration of unactivated particles might be larger near cloud edges; however, the (super)saturation is much lower there so that ice nucleation seems less likely.

Table 6: This table seems unnecessary given that there are only a few cells with numbers > 0.01. I think these results could be easily mentioned in the text.

Figure 1: Why does the extrapolation of the Snomax result in a plateau?

Figure 12: This figure is very busy and not clear. What are the droplets and what are the ice particles?

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Technical comments

- At several places in the manuscript 'as function of' should be replaced by 'as a function of'

p. 16412, l. 14: independently

p. 16416, l. 21: drop –> drops

p. 16417, l. 3: base –> basis

p. 16421, l. 18: less but large drops –> fewer but larger drops

p. 16428, l. 14: 'the spectra broaden' or 'the spectrum broadens'

p. 16428, l. 18: still –> even?

p. 16428, l. 22: enhances –> extends?

p. 16430, l. 21: in the order –> on the order

p. 16430, l. 24/25: Add units

p. 16431, l. 27: relation –> ratio

Tables 2-5: Please, check carefully the equation numbers you refer to here. They seem wrong. Figure 5 and 6: References to Ardon-Dryer and Levin (2012) and Danielczok and Bingmenr (2014) are missing in the reference list.

Figure 8: 'Concentration' misspelled on y-axis

Figure 11: Clarify the units: Figures say 'per L', caption says 'per cm3'

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