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

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General comments

In this article, B. Ervens and G. Feingold investigate different parameterizations for immersion freezing, which are based on droplet freezing experiments by Lüönd et al. (2010). The five nucleation schemes are described clearly. As the parameterizations are all fitted to the data, they all do a reasonable job to reproduce them for the conditions under which the experiment was conducted. However, the authors show that they strongly diverge for different time scales, temperatures and particle sizes. Further-

more, the parameterizations have been implemented into an adiabatic parcel model, in which supersaturation varies during the ascent of the parcel and feeds back on ice nucleation.

This is a very timely study. Several different parameterization approaches of immersion freezing have been discussed in the recent literature, but so far, they have not been thoroughly compared with respect to their impact on ice formation in a ascending air parcel. I found the article very interesting and enlightening, but stumbled at a number of occasions. I hope that my comments, some of which are intended to provide more background on experimental findings on heterogeneous ice nucleation, help the authors to further improve this study. My main points are the following:

- In this paper, N_{IN} and the term "IN number concentration" refer to the total number of kaolinite particles, and N_{ice} to the number of ice particles created through heterogeneous ice nucleation. This is different from the common terminology in most experimental studies, which use N_{IN} as the number of **activated** ice nuclei at a given temperature and relative humidity, i.e. a (usually small) subset of the kaolinite/dust/etc particles. Although this is rather a semantic point, I think it is important to clarify this because it can lead to a lot of confusion. I strongly recommend to adopt the common terminology and **not** to term all dust particles "IN".

- My second point is actually related to the first one. For the parcel model studies, N_{IN} (i.e. the kaolinite concentration) is prescribed to 4 l^{-1} (and 1 l^{-1} in two sensitivity experiments). No reference is given for these values, but my impression is that this choice is guided by typical atmospheric IN concentrations, as measured in a CFDC. However, it is important to note that atmospheric dust number concentrations, while very variable in space and time, are typically orders of magnitude higher! Measurements of dust number concentrations are usually only available in dust plumes and dust source regions, but see e.g. Penner et al. (2009), Fig. 5,

or Hoose et al. (2010), Fig. 3, for simulated zonal average dust concentrations - even these are in the order of 1 cm^{-3}. In my understanding, only a small fraction of the total dust concentration activates as ice nuclei in clouds, and the 100% activation (or frozen) fraction as displayed in Fig. 2 is normally not reached in the atmosphere. Therefore, probably the most interesting regime is outside the temperature range of Fig. 2, at temperatures above 241 K, where a small fraction of the kaolinite particles are active. (For example, Pinti et al. (2012) find much higher freezing onset points for bulk samples containing numerous kaolinite particles.) In contrast, more than 50% activation are reached in most simulations presented here. I recommend to extend this study by using a higher dust number concentration as input to the adiabatic parcel model, and to focus on regimes (with a warmer start temperature for the parcel) where only a small fraction of the kaolinite particles activates to IN.

- The description of the deterministic scheme should be improved, some of the statements are inaccurate. See detailed comments below.
- It is stated in the abstract that laboratory experiments "often" report a time-dependent behaviour of ice nucleation. This is not true; on the contrary, of the few experimental setups which actually were able to investigate the time dependence of ice nucleation (DeMott, 1990; Niedermeier et al., 2010; Murray et al., 2010; Broadley et al., 2011), only a small number actually found an increase of the activated fraction with time (or at slower cooling rates).

Detailed comments

- Section 2.1.1: Please give more details on how the prefactor in eq. (1), ΔF_{act} and r_{germ} are parameterized (possibly in an appendix), because different formulations can lead to very different results.

- Section 2.1.3: It would be helpful to include information on in how far the "internally mixed soccer ball scheme" is equivalent to the "active site distribution" by Marcolli et al. (2007) and the "Nucleation Probability Dispersion Function" by Barahona (2012).
- Section 2.1.5: The deterministic scheme, as given by equation (6), is often called "active site density scheme" or "n_s-scheme", and these names should show up here. I find it confusing that Fletcher, Meyers and DeMott et al. (2010) are given as prominent examples - although they are of course deterministic scheme, they follow a completely different approach. Closer to what is actually implemented here are Connolly et al. (2009) and Niedermeier et al. (2010). These do not suffer from any missing constraints to the total IN (or dust) concentration. Neither does Phillips et al. (2008), by the way.
- Section 2.3 More information is needed on how condensation freezing is treated. Is the freezing point depression taken into account? How is this done? What happens to evaporating droplets? Are efflorescence and deflorescence treated explicitly?
- Section 3.3 and also in the figure captions: I don't think the variation of S_{ice} needs to be mentioned, as this is not an independent variable. S_{wat} is kept constant.
- page 7181, line 13: "since the supersaturation is sufficiently high": I would argue that it would be better to say that the temperature is suffciently low. For immersion freezing, J is primarily a function of T and only indirectly a function of S_{ice}. Similar formulations are also found in section 4.1.2.
- page 7186, line 5: "the lack of comprehensive parameter sets ... at 253K$<$ T $<$263K": This criticism should be formulated more carefully: Which parameters are required? After all, there are numerous laboratory studies which cover this

temperature range. Most recent examples are Pinti et al. (2012) and Niemand et al. (2012).

- page 7188, line 20: It is unclear to me why the size dependency should be stronger for the deterministic approach than for the other schemes. Aren't they all roughly proportional to surface area? Is the size dependency of $f(m, x)$ already relevant for diameters around 800 nm? Please explain.
- Fig.2: Please include Lüönd et al. (2010)'s data points as a reference.
- Fig.3: Why are the dotted curves not smooth?
- Fig.4: Why are there steps in the ice crystal number concentration, e.g. in the yellow curve in 4(b)? If this is due to the discretization of the contact angle spectrum, then more bins would be needed.
- Fig.4(f): For the deterministic model, why does N_{ice} still increase although all liquid droplets seem to have evaporated? Even though eq.(6) doesn't include any explicit dependency on S, it can only be applied as long as the kaolinite particles are immersed in water.
- Table 1: The choices with respect to number of nucleation sites/particle and number of different particles should be commented on in the text.

Technical comments

- page 7172, lines 1 and 5: please give the values in SI units.
- page 7176, line 14: $\phi -> \Phi$
- eq.9: $i -> j$

- Fig.4: It would be helpful to include a vertical temperature axis similar to Fig. 7.
- Table 1: b_1 seems to be missing in the column heading. I can't identify what footnote 2 refers to.

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