Atmos. Chem. Phys. Discuss., 8, S7831–S7833, 2008
www.atmos-chem-phys-discuss.net/8/S7831/2008/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Parameterizing ice nucleation rates for cloud modeling using contact angle and activation energy derived from laboratory data" *by* J.-P. Chen et al.

J.-P. Chen et al.

Received and published: 2 October 2008

We greatly appreciate reviewer #1 for his/her thorough and helpful review. Some of the comment are of technical nature, and have been incorporated into the revision. The following are responses to the more critical comments. Please note that we made some adjustment in selecting experimental data for deposition nucleation on mineral dust to exclude possible events of "condensation freezing" that was mentioned by the authors but we failed to notice. The resulting contact angles are somewhat smaller, but this does not affect our major conclusions. All relevant data and discussions have been updated.

Specific Comments:

1. Page 14421 line1: Different types of freezing modes are mentioned. However, later in the paper, the use of the term "freezing mode" or "freezing nucleation" is referred to as only immersion freezing mode, and do not include condensation and contact freezing. Somewhere it should be clearly stated that "freezing nucleation", is means "immersion freezing".

Reply: Thanks for the reminder. We have clearly stated in Sect. 2.2 that "freezing nucleation" refers to "immersion freezing" in most part of the paper, and parameterization of "condensation-freezing" will be discussed in Sect. 4.3.

2. Page 11421 line 12: References such as Möhler et al. [2006] and DeMott et al. [1998] could also be included here. Page 14421 line 13: It is mentioned that the "threshold temperature" formulations and deposition mode formulations are "....not strictly correct either", and base the statement on that theoretical nucleation theory does not support this notion. However, recently published paper by Vali [2008] shows strong support for the modified singular model of heterogeneous immersion freezing nucleation. Maybe you could add something like "....not strictly correct either, if the theoretical nucleation theory can sufficient explain heterogeneous ice nucleation"?

Reply: Thanks for the suggestion. We have incorporated the suggested changes in the revision. The issue of "singular model" versus statistical model is related to reviewer's next comment, so our additional response is combined into the discussions below.

3. Page 11412: Somewhere in the introduction, the work by Khvorostyanov and Curry [2004] could be mentioned as an example for a heterogeneous ice nucleation parameterization, based on classical nucleation theory, that can be used in models.

Reply: These suggestions have been incorporated into the manuscript.

4. Page 14424 line 1: Möhler et al. [2006] gives the nucleation rate in their paper for a given dS_i/dt. They note that the formation of new ice crystals quickly stops after reaching a maximum S_i (ice supersaturation), and new ice is not formed (even if a

ACPD

8, S7831–S7833, 2008

Interactive Comment

large fraction of the dust particles have not frozen yet). Thus even if enough time is given, more ice is not nucleated. How can this observation be incorporated into the classical nucleation theory where ice seem to still form, only given a specific length of time?

Reply: We believe the issue of statistical versus singular concepts is unsolved. The observation by Möhler et al. [2006] cannot be viewed as solid proof of singular nucleation because for which the time-independent feature (i.e., no new ice formation at a constant environment) would occur at all Si, not just above a threshold maximum. The existence of a threshold maximum is also quite peculiar to the singular concept. We also noted that in Figures 5 and 6 of Möhler et al. [2006], the fraction of nucleation reached 80% to 100% toward the end. It is possible those relatively minor fraction of dust that remain un-nucleated have different physical or even chemical properties than those have been nucleated. More detailed discussions on this have been added in Section 4.4. We welcome the reviewer to provide more comments on those discussions. We also put in notes in Sections 1 and 2 to stress that our analysis is based on the statistical model.

5. Tables: I suggest, for easy comparison, to keep the different types of IN in the same order as in table 2 and 4. Also, use the same order and naming in table 6 and 7 as in table 2 and 4.

Reply: We have modified all the tables according to this suggestion.

6. Table 5: First value for m is wrong. With a contact angle of 109.2, m = -0.328 (not -0.279)

Reply: Thanks for pointed out the error. It was a typo. The angle should be 106.2 instead of 109.2.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 14419, 2008.

ACPD

8, S7831–S7833, 2008

Interactive Comment