

Interactive comment on “Heterogeneous ice nucleation on atmospheric aerosols: a review of results from laboratory experiments” by C. Hoose and O. Möhler

Anonymous Referee #2

Received and published: 29 June 2012

I rate this manuscript as excellent on all accounts, and in need of the following minor revisions prior to acceptance in ACP.

This is a serious undertaking and a carefully constructed summary of many ice nucleation measurements in the atmospheric. The manuscript represents a major and useful contribution to be used by many other researchers. The only serious omission I see is a discussion of contact freezing. I am surprised by this omission, since the lead author has stated in previous work that contact freezing is in fact, the least understood, mechanism in heterogeneous freezing at this time.

A second omission is that surface area vs. volume based nucleation rates occupied a

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lot of literature discussion and should also be included, at least, in brief.

Third, aerosol aging processing which are not represented by coating should be at least mentioned. While these are almost non-existent in the literature, this type of experiment needs to be undertaken in a serious way in future experiments. It would be nice to that mentioned briefly.

Minor comments follow:

Pg. 12. The manuscript states that - According to Murray, commercially available minerals may have undergone acid washing or other chemical processing. It should also be mentioned that commercial processing can include physical processing, such as milling which may change particle size, morphology, and surface roughness.

Pg. 14. In the discussion of impacts of coatings added to aerosols in laboratory, it would be useful to mention that methods of coating have been poorly constrained in most studies and that the resulting coating vary in thickness and also may vary in the percentage of the aerosol population which is coated vs. uncoated. This makes comparison of results from various experiments difficult, and also makes appropriate use and interpretation of these for atmospheric modeling difficult.

Pg. 14. Line 9 The manuscript defines a norm, referencing only 2 publications and then states that 3 manuscripts are exceptions to this norm. This needs revision.

Pg. 15. Line 11. Word choice: ..." (in reality always" is out of place.

Pg. 17 line 17 "High nucleation onset temperatures are consistent with most results at low T" This does not make sense as written.

Pg. 21-22. Section on time dependence. This fairly lengthy section really only discusses the work of Murray 2011. A literature survey would reveal that there are quite a large number of other published works addressing the issue of time dependence. Omitted them and focusing exhaustively on Murray is inappropriate.

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