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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Efficiency of immersion mode ice nucleation on surrogates of mineral dust" *by* C. Marcolli et al.

C. Marcolli et al.

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We thank reviewer 2 for his/her thoughtful comments.

Detailed response to reviewer:

Specific comments:

1) What is the influence of the lanolin surfactant, if any? Lanolin is added to the oil phase in order to stabilise the water-oil interface. Is it possible that the lanoline also goes to the solid-aqueous interface (i.e. the surface of the immersed ATD) and therefore alters the ice nucleation efficiency of the mineral particles? The authors quote a few papers where heterogeneous nucleation in the immersion mode has been studied in oil emulsions as a justification for this approach. However, in these previous studies (Zuberi et al and Zobrist et al) the solid particles were generated within the aqueous

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droplets only after the emulsion had been formed. Hence, it is difficult to see how the surfactant could get to the solid-aqueous interface. The present study employs a different approach. Here the authors mixed an aqueous suspension of ATD with oil using a high speed mixer, i.e. they mechanically break the solution down into small droplets. I am concerned that during this vigorous mixing process ATD particles could come into contact with oil and pick up some lanolin at their surface. Maybe the authors have thought about if lanoline would energetically be allowed to sit at the solid-liquid interface. If the authors are happy that the surfactant is not an issue, then they should indicate why in the manuscript.

Response: In the revised manuscript, we discuss the possibility of lanolin interacting with the mineral dust by adding the following sentences to section 4.2, page 9701, line 8, after ...nucleation. From this, we conclude that mineral dust partitions to the water droplets and is not covered by the surfactant. To our knowledge, interactions of lanolin or substances consisting of the same functional groups as lanolin with montmorillonite or kaolinite (main components of mineral dusts) have not been reported in the literature. Nevertheless, minor adsorption of lanolin to the ATD surface can not be excluded on the basis of the available evidence.

2) P 9700 (lines 17-21). This seems to be repeated from earlier in the paper.

Response: We avoid the repetition in the revised manuscript by deleting these lines.

3) P9701 (lines 19-21). Here the authors discuss the relevance of their results to Knopf and Koops ice nucleation experiments. The authors have made a distinction between immersion and condensation freezing. My understanding is that condensation freezing is where one starts with a dry solid particle and it takes up water and some remaining solid then nucleates ice within the droplet. Whereas immersion freezing is where a solid particle immersed in liquid then nucleates ice. Surely, in the condensation mode once a solid particles is immersed in water then it becomes the immersion mode. The only distinction is the history of the droplet. Hence, the authors explanation does not

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seem to explain why there is a difference between your and Knopfs experiments. An alternative explanation might be the differences in time spent at one temperature. I think Knopf and Koop spend longer at 260 K than the present authors and therefore nucleation should be more probably.

Response: In their nucleation experiments, Knopf and Koop reach supersaturation by increasing relative humidity at a rate of ca. 2 - 20% min-1 corresponding to an increase of the ice saturation ratio at a rate of also ca. 2 - 20 % min-1. A cooling rate of 1 K/min as applied in our experiments is equivalent to an increase of the ice saturation ratio by ca. 1 % min-1. The rates at which supersaturation is reached are therefore similar in the Knopf and Koop and the present study and even a bit lower in our experiments. Different timescales can therefore not explain the discrepancies between the two studies. We agree that our explanation of the different freezing temperatures is not altogether satisfying but based on the experimental evidence at hand further conclusions are difficult.

4) P 9702 (lines 10-13) I do not understand these lines. Maybe delete lines 11-13 and leave a revised form of line 10?

Response: We agree that this sentence may be difficult to understand and change it in the revised manuscript as suggested by the reviewer.

5) p9704 and 9705. How were the parameters in eq 8 derived. Is this some sort of best fit to the data? Did you perform a least squares fitting procedure? Is this a unique solution? Please clarify.

Response: We tested different functions and varied the parameters to best fit the measurements. The function given in eq 8 can not be considered as a unique solution. We clarify this in the revised manuscript.

Technical comments: 1) P9700 (line 15). Modeled should be Modelled. I am using a UK dictionary not a US one (not sure what the ACP rules are?). Response: We used

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American spelling throughout the whole manuscript.

2) P9699 (line 5). Full stop after balance and start a new sentence.

3) P9699 (line 5 and 6). I suggest you revise this to the following: In the AIDA chamber Benz et al observed homo.

4) P9705 (line 19) Replace Neither the contact angle nor the active site distribution allows to fully explain the tail to higher freezing.. with Neither the contact angle nor the active site distribution fully explain the tail to higher freezing ..

Response: We changed the revised manuscript according to technical comments 2 - 4.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 9687, 2007.