

Interactive comment on “Ice nucleation efficiency of clay minerals in the immersion mode” by V. Pinti et al.

Anonymous Referee #1

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V. Pinti, C. Marcolli, B. Zobrist, C. R. Hoyle, and T. Peter have undertaken a differential scanning calorimetry study of a number of often-used mineral dusts and one which I have not seen specifically used from a mountain region in the Sahara. Heterogeneous nucleation behavior is observed and a bi-modal structure is taken to be a small population of very good ice nuclei and a more common population that nucleates at a somewhat lower temperature. Overall this is a solid study of ice nucleation that largely agrees with the previous literature but also modestly expands upon it. It is therefore my opinion it is suitable for publication in ACP with some minor revisions.

The use of the terms “special” and “best” for the aforementioned nucleation peaks is rather awkward. Specifically, it isn’t clear what “special” or “best” really refers to and neither is a descriptive scientific term; my suggestion would be use of a more

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descriptive term such as “highest temperature”, etc. (this for both the peak and the active sites). To be clear, I’m not so much concerned with what the final choice in words is but just that something descriptive and not subjective is used.

I suggest that the sentence “We suggest that apparently contradictory results obtained by different groups with different setups can indeed be brought into good agreement when only clay minerals of the same type and amount per droplet are compared.” should be reworded. Specifically, there are many experiments that only look at a single ice nucleus whereas these authors use a technique that can include more. Since a single ice nucleus is the atmospherically relevant condition what the authors should be suggesting is that their data need to be interpreted in the context of a single ice nucleus, not that more atmospherically relevant experiments be changed (the appropriate rewording is to suggest that those experiments that use multiple IN need to be interpreted as such; as it stands now it reads the other way around).

Reference on dust loading from IPCC 2001 should be updated to a more recent reference (i.e., IPCC 2007 or other).

While the DSC apparatus has a history for these types of studies there needs to be a line in the Experimental section that notes that the emulsion material is not what one would find in the atmosphere surrounding a particle but that it is believed to not effect the results. This is done but not until the results section on page 12 which is counterintuitive. A reference to this effect would also be warranted (I assume this issue was not in question until this manuscript?).

The “Clays” section is somewhat too long. There is extensive literature on the dusts and much of this information is best left as a reference. The Hoggar section is novel and should be left as is.

It is a bit strange that a new name (Ahaggar) is presented for the Hoggar samples on page 8 and then again on 11. Much as for an acronym this should be moved to the first call of the name and then use one or the other throughout.

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BET needs to be defined at first use (page 13)

The second paragraph of the Discussion section is rather awkward and needs a rewrite. The authors quickly go through BET, classical nucleation theory, contact angles, and singular theory without really explaining any concept. Please clearly define the different attempts to interpret the data and then show if the fit is good or not. The paragraph too quickly goes through multiple concepts known only to the expert in this area. More detail can then be given in the next paragraphs.

Despite these minor issues this is a well written paper of interest to the ACP community and one worth publishing.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 3213, 2012.

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