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Interactive comment on "Some ice nucleation characteristics of Asian and Saharan desert dust" *by* P. R. Field et al.

Anonymous Referee #2

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General Comments:

This paper describes results from chamber studies of the ice nucleating ability of standard mineral dust particles. This is a companion paper to another paper submitted to ACP by Mohler et al. (2006). While the experiments described in this paper and the results obtained and worthwhile and carefully conducted, this paper suffers from a lack of detail in the discussion of results and their atmospheric implications. The introduction is also incomplete and more details regarding the methods and associated errors are needed. However, upon reading the companion paper by Mohler et al., many of these above issues are in fact discussed by that paper. Thus, Field et al. could strengthen this paper by more clearly outlining what relevant aspects of this study are

already described by Mohler et al. but are not described in this manuscript. The bulk of the major results from this study appear to be presented in Mohler et al., with Field et al. providing some additional insights. The introduction, methods, and discussion in Mohler et al. are also more comprehensive. Therefore, the best option may be to combine the Mohler et al. and Field et al. papers into one complete manuscript. This decision is up to the authors and editor. This manuscript will be acceptable for publication upon significant expansion of the discussion of results, when further discussion of the companion paper is added, and the following points are addressed.

Specific Comments:

Numerous terms need to be defined to make the results clear to the non-specialist. Some of these include: deposition nucleation, condensation nucleation, CVI residuals, heterogeneous ice nucleation, homogeneous freezing. I was particularly surprised and disappointed with the complete lack of discussion of the various types of ice nucleation mechanisms, the uncertainties surrounding them, their atmospheric relevance, and citations to related papers that discuss these mechanisms, e.g. [Cantrell and Heymsfield, 2005; Ren and Mackenzie, 2005].

Many of the following issues are addressed by Mohler et al. (2006) but that is not made clear in this paper, forcing the reader to go and "hunt" for the answers in the companion paper. As these are important issues for both studies, the following questions should either be dealt with separately by Field et al. here or clear reference given to the appropriate sections in Mohler et al. (2006).

There should be some discussion of the importance of ice nucleation in the atmosphere, particularly for climate forcing, and its large uncertainties. More references to the many studies of ice nucleation should be provided, for example [Archuleta et al., 2005; Cantrell and Heymsfield, 2005; Cziczo et al., 2004; DeMott et al., 2003; Jacobson, 2003; Levin et al., 2005; Lohmann and Feichter, 2005; Murray et al., 2005; Salam et al., 2006; Sassen et al., 2003; Tabazadeh et al., 2002; Twohy and Poellot, 2005; Yin

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et al., 2002].

The Zuberi et al. (2002) study used mineral dust immersed in (NH4)2SO4 to represent mineral dust that has undergone atmospheric processing. As many measurements of atmospheric dust show that the particles tend to have experienced some chemical processing during transport, this is a realistic approach. The dust samples used for this study have not undergone such aging. While this study of unprocessed dust is valuable there should be some discussion of the role that this atmospheric processing might play in the ice nucleating ability of mineral dust. Do you plan to study this effect in the future? Cziczo et al. (2004) found that the mineral dust particles that nucleated ice lacked secondary species caused by processing, suggesting that this aging of dust could reduce its ice nucleating ability. This point is raised again on pg. 1522 when you state "Therefore, it is unclear whether the secondary nucleation events (region III) represent a further deposition nucleation mode, or a condensation freezing mode facilitated by the presence of some SOLUBLE MATERIAL on the surface of the aerosol." (capitals added)

The Roberts and Hallett (1968) study you cite demonstrates the importance of pre-conditioning of the particle on its ice nucleating ability. Was this explored at all in your experiments? Did you perform cooling and heating cycles on the same dust sample? On pg. 1522 you state "We are unable to comment on this - from examination of the starting aerosol concentration (Table 1) it is likely that all of the ice and hence ice nuclei sediment out during the course of each experiment." Does this prevent you from performing such temperature cycles?

How was the dust sample re-dispersed? What pre-treatment did it undergo? What was its size distribution and is this relevant to dust aerosol undergoing ice nucleation in the atmosphere?

Who are the manufacturers of the SID probe and the CPI? Regarding the SID probe you state "Using a threshold can lead to some non-spherical particles being classed

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as spherical and vice-versa (see Field et al., 2004)." (pg. 1515). What fractions of misclassifications are typical here (upper limits)?

A wide variety of instrumentation was used to measure the size distribution of the dust aerosol and many of these have overlapping size ranges. Could you provide more detail about the role that each instrument played in these studies and also the typical measurements errors associated with each under these conditions. The APS and SMPS data doesn't seem to be used in the data presented here.

For the SID why was Af $>$ 12 chosen as a cut-off for non-spherical particles? (pg. 1517) Are there any previous studies that you could cite for this value? You later state that the SID underestimated the activated fraction by using Af$>$ 12 as the cut-off. Could this have been corrected for by using a different value for Af?

A more quantitative discussion of the relevant experiment errors should be provided.

Please demonstrate that the temperatures and cooling rates used here are relevant to ice nucleation by dust in the upper troposphere.

This statement on pg. 1519 requires further explanation, particularly in terms of the various potential mechanisms: "This suggests that the dominant ice nucleation mode in this case is an immersion or condensation mechanism and activated about 10% of the aerosol (panel f)."

The ability to separate the results out into three distinct nucleating modes is very nice but the discussion would benefit from an explanation of the various ice nucleating mechanisms that you observe (e.g. immersion, condensation, deposition).

Why does the water saturation in Figs. 2 & 4 vary so roughly, as opposed to the smooth transitions in Figs. 1 & 3?

Your finding of similar behavior for both the Saharan and Asian dust samples suggests that the chemical composition (of the mineral components at least) does not play a large role in the ice nucleating behavior of dust. Did you make any comparisons of the

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morphological characteristics of these two samples to compare how similar they are on a physical level?

The Figure Captions for Figs. 1-4 should state which dust sample the experiment was performed with. In Fig. 5 are the CPI images shown for both the Asian and Saharan dust samples or just one type? This might help address the above question regarding morphology.

In your conclusions you state, "The primary nucleation event is likely to be deposition nucleation, but the secondary event is either a further deposition mode exhibited by particles of different sizes or mineralogical/chemical composition or a condensation mode facilitated by the presence of soluble material on the desert dust." Do you have any insight into how to identify the mechanism of the secondary event in future studies?

Finally, and most importantly, what are the atmospheric implications of your findings? Your conclusions do not state what novel information has been presented here.

Technical Corrections:

There appears to be an error in this sentence: "This suggests that if the first runs are carried out at slower pumping speeds then the dual nucleation will become apparent for the first expansion also - not because cooling rate is expected to affect the cooling rate (Mohler et al., 2005), but simply because this allows a longer temporal seperation between the nucleation events." (pg. 1521)

The Caption for Fig. 6 should explain what the region numbers I-III indicate.

In Fig. 7 there is no dotted line shown as the caption states there is. In addition, I do not see any circle symbols that "represent experiments where only a single nucleation event was observed."

Additional References:

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