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# *Interactive comment on* "Saharan dust and ice nuclei over Central Europe" *by* H. Klein et al.

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This paper presents measurements of ice nucleation collected at a ground-based central European mountain site. Measurements were continuously collected before, during, and after a regional dust event. Transport of the dust is modeled using the Eulerian aerosol DREAM model. Comparisons are made between concentrations of ice nuclei (IN) and measured aerosol properties. This work is appropriate for publication in ACP, particularly because few measurements of ice nucleation are available in this local. Also, the duration of measurements is commendable, though the range of operating conditions (temperature and relative humidity) is limited here. However, some of the interpretation of the measurements needs modification prior to modification, since the stated conclusions do not follow directly from the measurements. These are specified below:

In the abstraction and elsewhere. One major conclusion, "..dust is always a dominant constituent of ice nucleating aerosol in Central Europe." does not follow from the previous sentence, "The ice nucleating characteristics of the aerosol with respect to temperature and supersaturation are similar during the dust episode [to characteristics observed throughout] the course of the year. Given that there are many unexplored aerosol types which may be effective IN (and thus nucleate at warm temperatures and low supersaturations), this conclusion is weak.

On a related note, it is not clear from the text what range of conditions each sample was exposed to in the FRIDGE ice chamber. What I glean from the methods section (14997) is that each sample was cooled to a single temperature (in the range of -8 to -18 C) and was then observed at a range of subsequently higher supersaturations. If so, mineral dust, biomass and definitely some biogenic aerosols may all produce the same results. Thus, based on these measurements alone, it should not be stated that the contributing IN throughout the year is dust.

In the same section on page 14998 referred to above, the text states that deposition and condensation freezing IN were observed and counted. Please clarify, can the FRIDGE differentiate between these two heterogeneous mechanisms or is a single IN count reported? Page 15: Regarding two other major conclusions in the manuscript, which appear to be in conflict with one another, as currently written. It is noted that spikes in IN concentration are correlated with spikes in the PM10 dust, not the total mass of PM10. This is an important result since it provides strong evidence that dust is acting as the IN in those episodes. Please explain how PM10 dust is measured/determined.

The second result is that IN number is better correlated with aerosol surface area than with aerosol volume. If I understand correctly, here the authors are referring to surface area of the total aerosol, not the dust. The previous conclusion, that IN is related to dust, not total aerosol, is based on mass (which roughly equates to volume). If IN is in reality connected to volume of dust aerosol, not total aerosol, what evidence is there that a better relationship with surface area than with volume is anything more than



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coincidence? Better justified is needed here, or this conclusion about surface area must be removed.

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