

Interactive comment on “Seasonal variability of Saharan desert dust and ice nucleating particles over Europe” by L. B. Hande et al.

Anonymous Referee #2

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Review of: Seasonal variability of Saharan desert dust and ice nucleating particles over Europe (Hande et al., 2015)

This paper describes a regional model study of Saharan desert dust and ice nucleating particles over Europe. The underlying dust emission and transport model has been previously evaluated in other studies. The present study applies experimentally-derived parameterizations of the ice-nucleating activity of dust particles to the model-simulated spatiotemporal dust distributions in order to calculate the distribution of “potential INP”. It is shown that the mean values of dust INP are much higher than the median, suggesting that the mean is dominated by a small number of strong events. It is also shown that the interplay between declines in dust concentration with altitude and declines in temperature with altitude leads to a peak in the vertical distribution of “potential INP”

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at altitudes around 6000 – 8000 m for immersion freezing, and around 7000 - 1000 m for deposition freezing. The seasonal profiles are shown to differ between summer and winter as a result of seasonal differences in both dust concentrations and temperature profile.

This is a valuable contribution in that it begins to elucidate how the climatological variability of dust concentrations in the atmosphere will impact the variability of INP concentrations. My main comments are related to the presentation of comparisons with observations in the paper. I would recommend this manuscript for publication in ACP after the following comments have been considered and addressed.

Overall comments: 1. Would it be possible to show a model-obs comparison on an x-y plot using the data presented in Fig 4 (particularly for immersion freezing)? I recognize that the Niemand et al. (2012) parameterization is only valid over a specific temperature range and these observations are mostly at warmer temperatures, but it would be interesting to know whether extrapolating to the warmer temperature would produce good agreement with the observations. From Fig. 4, it appears that it likely would agree well. 2. Figure 4 would have a more intuitive interpretation (to my mind) if it were normalized such that the integral of each temperature bin were unity. On p. 32081, l. 3, it is stated that “From Fig. 4, most immersion INPs are occurring at temperatures warmer than 250 K”, but doesn't the figure simply show that the temperature in the model domain is almost always warmer than 250 K? It would be more useful to know, for a fixed temperature, the probability of different values of potential INP occurring. 3. The direct comparison with the DeMott (2010) parameterization in Fig. 4 could be misleading. First, the histogram convolves the temperature dependence of IN activation with the distribution of dust particle in the atmosphere. Both particle concentrations and temperatures will be higher at lower altitudes, and both impact the histogram. The DeMott et al. parameterization is also a function of particle concentration, but in this figure the particle concentration has been set to a constant value (I assume), so it is not straightforward to compare these two variables. The compari-

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son would be more straightforward if the histogram were normalized as suggested in the previous comment. Furthermore, the DeMott et al. (2010) parameterization is not composition-specific, while the parameterization applied in this paper is applicable only to dust aerosol. Tobo et al. (2013) and DeMott et al. (2015) have published coefficients for this parameterization that are specific to dust, which would be more appropriate for this comparison. The dust-specific comparison should be used either in addition to, or in lieu of, the DeMott et al. (2010) parameterization, and it should be mentioned in the figure caption that a fixed particle concentration was used for the parameterization. 4. The same issue also affects the comparison with the observed INP concentrations. I believe the temperatures of all of these observations are instrument temperatures, not ambient temperatures (which should be pointed out to the reader). But, here they are being compared with a histogram of INP concentrations that is based on the number of INP active at ambient temperatures, which is not an apples-to-apples comparison, for the reasons mentioned above.

Specific comments:

1. p. 32075, l. 9: “this parameterization is active a[t] colder temperatures . . .” → does this mean that the parameterization is only valid in the range given, or that particles are IN-active at those temperatures? Or that this parameterization is applied in the model when the conditions are within the specified range? 2. p. 32075, l. 17-24: does “potential immersion INP concentration” here mean the ambient temperature in each model grid box was used to calculate the number of immersion INP active at that temperature? (and the same question applies for the “potential deposition INP concentration”)

Typographical/ technical comments: p. 32072, l. 4: “it’s” -> “its”, omit comma after “concentrations” p. 32074, l. 20: “Modell” -> “Model” p. 32075, l. 9: “active a” -> “active at” p. 32077, l. 25: “northern most” -> “northernmost” p. 32082, l. 22: typo in “parameterisations”

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