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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nuclei properties within a Saharan Dust Event at the Jungfraujoch" *by* C. Chou et al.

Anonymous Referee #1

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Overview Comments.

This manuscript presents ice nuclei measurements collected at the high alpine research station Jungfraujoch. The measurements were focused on deposition nucleation and were conducted at a time of year when contributions from Saharan dust were expected. Relatively high concentrations of deposition IN were observed during periods of Saharan dust influence, and IN showed a reasonably strong correlation with dust concentrations. In contrast, IN were not shown to be correlated with black carbon. The paper adds to the existing dataset of ambient IN measurements and should be published after addressing the following comments. My only major suggestion is that the authors consider another method for determining the presence of dust, so that the relation between dust and IN is better established. As it is, there seems to be a lot of

handwaving in the paper regarding when the site was actually in a dust plume.

Specific Comments.

p. 2, line 27. The Lau and Wu [2003] study is specifically for the tropics, and should be stated as such.

p. 2, lines 36-38. It has been known for quite some time that certain bacteria are very effective ice nuclei (e.g. [Schnell and Vali, 1976; Vali et al., 1976]).

p. 3, line 72. Change 'the University of Colorado' to 'Colorado State University.'

p. 4, lines 125-128. Was the impactor cleaned during the study? If not, for the high mass loadings during the SDEs, the impaction efficiency likely degraded during the measurement periods. Was this ever tested at the field site?

p. 5, line 134 and Table 1. Although I understand the reasoning behind using 1.5 g/cm3 for the average density, it would be useful to include a column in the Table using the density of dust, which is on the order of 2.3 g/cm3. Using this value, the cutsize for dust is < 0.6 microns, based on your D50 of 0.91 micron aerodynamic diameter. This will even be slightly smaller when you consider the effect of lower pressure at the high altitude research site, compared to the laboratory tests. This is significant, given that most of the dust is probably larger than this. I also would suggest using more relevant values for Table 1: the d50 cutoff (0.91 aerodynamic diameter) and the reference points for comparison to the OPC data later in the paper (0.3, 0.5, and 0.8 microns).

p. 5, line 136. What do instrumental limitations have to do with focusing on deposition nucleation?

p. 7, lines 222-225. With no data to support the statement, it is meaningless to speculate that the increase in IN is related to biological particles. This sentence should be removed.

Section 3.1.2 It would be interesting to see the full size distribution during a SDE to get a feel for what fraction of the dust falls in the <0.91 micron aerodynamic diameter mode.

p. 9, Section 3.1.3. As I read it, the authors are suggesting that there are periods that are influenced by Saharan dust, but are not Saharan Dust Events, based on their definition of a SDE. This suggests that the method that they are using to define a Saharan Dust Event (SSA exponent is negative for 4 consecutive hours) may not be appropriate for this field site, where local meteorology plays an important role. Why not use the same criterion for a period which is less than 4 hours?

p. 9, line 270. Is this first sentence referring to June 15 and 16, or June 11 and 14? It is not clear.

p. 9, lines 270-272. Higher concentrations of what? IN? Large aerosol?

p. 9, Section 3.1.3 and Figure 8. It is a little confusing trying to follow the text and compare it to the figure. It would be helpful to shade Figure 8 for regions which are SDEs or which are at least influenced by dust.

p. 10, line 305 and line 320. Is it correlation coefficient or Rˆ2?

p. 10, lines 307-308. DeMott et al. [2010] demonstrate the relationship between IN and particles >0.5 microns in a recent PNAS paper, although for the DeMott paper, the relationship is for measurements above water saturation.

p. 10, lines 303-308. As mentioned earlier, for an aerodynamic cutsize of 0.91 microns, and for a realistic density, the IN data are limited to diameters of less than $\sim$0.6 microns optical diameter. This obviously is not consistent with the 0.5<D<0.8 micron size bins that the authors use for comparison to the IN. I would suggest using either 0.5<D<0.6 microns, or everything >0.5 microns, as an indicator of dust concentrations.

p. 10, lines 318-320. The authors have already stated that there may be dust influences on non-SDE days, and so limiting the comparison to non-SDE days may not be sufficient to remove contributions from dust. This again suggests using a different

criterion for determining time periods when dust is present.

p. 10, lines 330-332. If you always processed at -31 $^\circ$C and 91% RHw, you were not at ambient thermodynamic conditions.

p. 10, lines 333-335. I think that you are primarily observing differences in intensity of the dust event, and not ‘that their properties might play an important role in the ice nucleation efficiency of the particles’. My comment is based on Figure 11, which shows that for a small dust event, there are $\sim$0.05 IN cm-3 (within a factor of 2) when there is $\sim$1 dust particle cm-3(within a factor of 2), when using particles 0.5<D<0.8 microns as a proxy for dust. Likewise, for a larger dust event, there are $\sim$0.25 IN cm-3 when there are$\sim$ 5 dust particles cm-3. In both cases, $\sim$5% of the dust particles activate as IN, suggesting that they have similar properties.

Equation 2. Clearly define all variables in the text.

Figure 2. It’s not very useful to have the scale starting at 80% RH, when all of the data are >100%.

Figure 4 is unnecessary.

References.

DeMott, P. J., et al. (2010), Predicting global atmospheric ice nuclei distributions and their impacts on climate, Proc. Natl. Acad. Sci. U. S. A., 107(25), 11217-11222. Lau, K. M., and H. T. Wu (2003), Warm rain processes over tropical oceans and climate implications, Geophys. Res. Lett., 30(24), 5. Schnell, R. C., and G. Vali (1976), Biogenic ice nuclei, Pt. 1, Terrestrial and marine sources, J. Atmos. Sci., 33, 1554-1564. Vali, G., et al. (1976), Biogenic ice nuclei. Part II: Bacterial sources, J. Atmos. Sci., 33(8), 1565-1570.

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