

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

C. Chou et al.

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Comment) 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

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

Reply) We thank Reviewer 1 for all the helpful suggestions and comments. Below are detailed answers to the reviewer comments with the locations of the incorporated changes in the revised manuscript.

Specific Comments.

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

Reply) We have now modified the sentence to highlight that the study was specifically for the tropics. (page 2 line 27-28)

Comment) 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]).

Reply) The above references have now been included in the manuscript (page 2, line 41)

Comment) p. 3, line 72. Change ‘the University of Colorado’ to ‘Colorado State University.’

Reply) The change has been made. (page 3 line 77)

Comment) 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?

Reply) The impactor was cleaned before the beginning of the campaign, but was not tested at the field site. The impaction efficiency could have degraded during the field study, however our laboratory tests show that the impactor only started to degrade in efficiency when mineral dust concentrations of the order of $\sim 10^3$ cm⁻³ were sampled through the impactor for about 10 days (7-8 hours a day). Given the lower concentrations of dust at the field site, we believe that the impaction efficiency would not have

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been significantly influenced during the field study.

Comment) 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).

Reply) We acknowledge this relevant remark and a column has been added in Table 2, as well as more relevant value points for conversion, i.e. 0.91, 0.3, 0.5, 0.6 and 0.8 micron as suggested by the reviewer.

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

Reply) With PINC, quantitative measurements can only be performed in the deposition nucleation mode, as above water saturation there is a competition between condensation and deposition, which cannot be clearly differentiated with the current version of PINC.

Comment) 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.

Reply) The sentence in question has been removed.

Comment) 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

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diameter mode

Reply) The full size distribution of the SDE of June 15th has now been added in Figure 8 (or in attachment).

Comments) 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. 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.

Replies) We used this method as it is the highest resolution (hourly resolution) at the measurement site and also because it provides us with real-time measurements and indications of possible dust events at the Jungfraujoch. The criterion of a negative SSA exponent for 4 hours has been used by Coen et al., 2004 for the same measurement site and they have shown with this methodology that SDE lasted between few hours up to several days and that in 92% of the cases the negative exponent correlated with calculated backtrajectories, filter measurements, satellite observations or a combination of these methods.

In order to clarify the periods influenced by dust with negative SSA and periods influenced by dust with positive SSA, the two sections has been merged into one, and read as "high IN number concentration events" to avoid confusion. (page 8 section 3.1.2)

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

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Reply) This sentence refers to June 15 and 16. This has been clarified in the new version of the manuscript (page 9, section 3.1.2)

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

Reply) We meant higher IN number concentration. This has been clarified in the new version of the manuscript (page 9, section 3.1.2).

Comment) 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.

Reply) The figure has been changed according to the above suggestion.

Comment) p. 10, line 305 and line 320. Is it correlation coefficient or R^2 ?

Reply) We meant R^2 . This has been clarified in the new version of the manuscript (page 11, line 351 and line 367)

Comment) 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.

Reply) The above reference has been added and discussed in order to give more impact to our results. (page 11, lines 354-358)

Comment) 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 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.

Reply) The suggestion of the reviewer has been taken into account and included in the revised manuscript (page 11 line 351). Changes to Figure 11 have been made

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accordingly. (Figure 11)

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

Reply) The sentence has been modified to read: "... at constant thermodynamic conditions of -31°C ..." (page 12, line 375)

Comment) 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 0.05 IN cm^{-3} (within a factor of 2) when there is 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 0.25 IN cm^{-3} when there are 5 dust particles cm^{-3} . In both cases, 5% of the dust particles activate as IN, suggesting that they have similar properties.

Reply) The discussion on the above point has been clarified in the revised version of the manuscript and the intensity and dust load have been quoted to be the main parameter in the different IN concentrations observed. (page 12, lines 377-379)

Comment) Equation 2. Clearly define all variables in the text.

Reply) The variables have now all been defined (page 7, section 2.3)

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

Reply) Figure 2 has been changed according to the above suggestions.

Comment) Figure 4 is unnecessary.

Reply) The Figure has been removed as suggested by the reviewer.

References:

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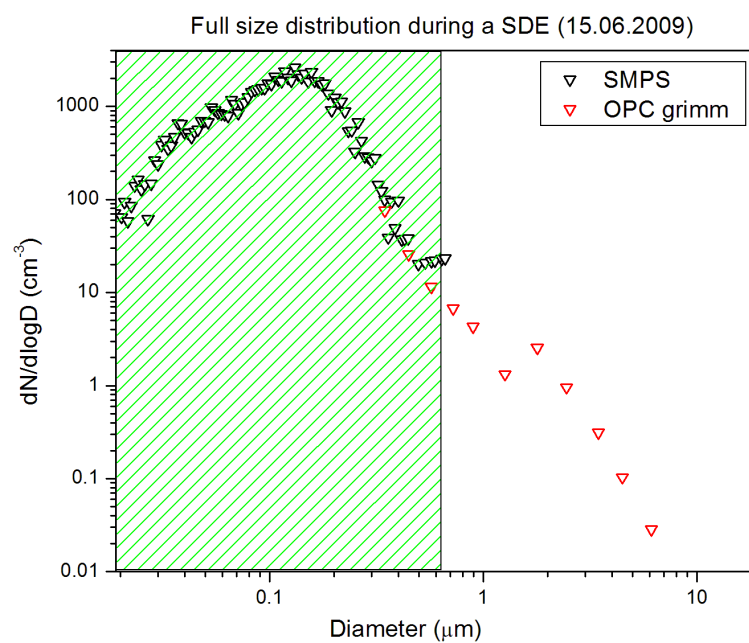


Fig. 1. Full size distribution during a Saharan Dust Event on June 15, 2009. The green shaded area corresponds to the fraction of particles sampled through PINC within a SDE.

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