Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-192-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Ice nucleating particles in the Saharan Air Layer" *by* Yvonne Boose et al.

Anonymous Referee #1

Received and published: 28 April 2016

General comments

A comprehensive set of data from 2 months of field measurements at Izana, Tenerife is analyzed. The authors have applied advanced experimental methods to measure atmospheric ice nucleating particles (INP), mineral dust, and bioparticles. The subject clearly meets the scope of the journal. These new data that are of special interest, because they characterize relatively young Saharan dust plumes, whereas most data published so far on the ice nucleating properties of mineral dust concern particles that were transported over long distances. From a comparison to the latter the authors plausibly infer that aging and mixture of the dust with pollutants likely will enhance the nucleating properties of mineral dust particles. Bioparticles were found to be slightly enriched among the INP as compared to the total ambient aerosol. In summary, the article is a valuable contribution to our current understanding of the role of mineral dust and bioparticles as INP, on the Saharan dust source, and on the effects of aging. The

Printer-friendly version

Discussion paper

manuscript is overall organized and crafted very well, and it deserves publication in ACP with minor revisions only.

Specific comments

Page (P.) 2, line (L.) 16: The reference Klein et al. (2010) may be omitted, because of serious experimental errors in these data, as stated in a recent paper by this laboratory (Schrod et al., Atmos. Meas. Tech., 9, 1313–1324, 2016).

P.2, L. 20: Huang et al., JGR 115, 2010 give a nice climatology of dust AOD over the subtropical atlantic from MODIS, you might add it as a reference here.

P.3, L.20: Conen et al. (2015) measured immersion freezing nuclei, this could be stated.

P.4, L. 19: Uptake of water, deliquescence and growth begin below water saturation. Assuming 100% as a threshold is reasonable for practical reasons, as we usually have nothing better for a given situation, but it is not the truth.

P.7, L.5: I recommend to replace “dried” by “evaporated” or “sublimated”. I presume that all ice is completely evaporated once a particle enters WIBS ?

P.7, Chapter 2.5: I wonder if a flow scheme would help a reader who is not familiar with your setup, but I realize that it can be looked up in your JAS paper.

P.8,L.6: Do you have more details or a reference on how gains and losses were accounted for ?

P.10, 3.3, Fig.4 and various other places: when R2 or R are compared, the number N of observations is often useful.

P. 10, L. 5-10: To my knowledge the first one to publish a correlation of INP to the number concentration of “large” particles (0.1-1μm dp) were Georgii and Kleinjung (Journ. de Recherches Atmosphérique, 145-156, 1967). This reference may be added.

Printer-friendly version

Discussion paper

P.13, L.1-2: since you excluded the (blue) squares from the Atlantic sector in your analysis of Fig. 9c you should write "... collected in the SAL under Saharan influence ..." . In the same sense you could add, that in Fig. 9d there is no correlation for the red symbols (for red and blue together one might get the impression that there is a correlation).

P.13, last sentence: It is not plausible to use Fig. 9f as an example for the large scattering and variability, because this is the plot that is least scattered of all the 6 plots in Fig.9, less than others that you use for interpretation.

P. 14: I suggest "Potential sampling bias" as a header of chapter 3.7

P. 15, L. 22-23: The parameters that are displayed in Fig.'s 12b and d could be described more clearly . "observed " is misleading, because it is more "what would have been observed, if there were no gains and losses". Also in the sentence "Figure 12b and d show ambient concentrations" the term "ambient" might be understood in this way, one could add, that it is derived from measurements.

P. 16, L29, conclusions: you mention the good correlation of INP to bulk dust mass, but not the much higher correlation of R=0.95 to the total particle number Ntot (Fig. 9a), why ?

P.17, L.5-8, conclusions: you could make a more forceful point of your finding, that ammonium sulfate at the surface of dust increases nucleating properties, by comparing it to the traditional wisdom that insolubility is required for an INP. Pruppacher and Klett (1980) have a whole little chapter 9.2.3.1 named "Insolubility requirement" on that.

Technical corrections

P.11, L.34: Fig. 6e must be 7e.

Fig. 10, caption, last sentence: change to "Each ambient PSD curve was measured ...", because the red PINC PSD curves were calculated, as stated in chapt. 3.7.

P.15, L.19/20 and Fig. 12: Fi. 12 has red symbols, which are described neither in the text nor in captions of Fig. 12 or Fig.2. What is it ?

Fig. 5: the dashed vertical lines indicating water saturation do appear in my printout only for 240 K, but not for 233K and 248K. Maybe the whole Figure can be enlarged ?

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-192, 2016.