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Atmospheric Chemistry and Physics Discussions

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

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The paper adds to the increasing but still incomplete knowledge about the role of mineral dust as atmospheric ice nuclei. In this study, the case is being made by looking at an event of Saharan dust incursion. In common with the major part of the existing literature on the subject, the focus is on ice nucleation at temperatures at and colder than -18°C.

Listed below are a number of small and some more substantial questions. Most importantly, while it does sound plausible, the claim that the evidence presented shows dust as a 'dominant constituent' of ice nuclei isn't fully justified. In my view, the assumptions made in constructing the proof (see last comment below) need to be stated, or evidence given for their validity. Also, the findings apply only to specific temperature/superstauration regimes and that too limits their generality.

Specific comments (reference to page/line):

14994/25: As Referee #1 also mentioned, ”good ice nuclei’ a poor choice of words. In addition, it is also non-specific, as quality could refer to the temperature of activation, the abundance, the time lag, the probability of nucleation, per unit surface area at a given temperature, or other definitions. Most important is to distinguish between activity in large numbers and activity at lesser supercooling.

14995/5-6: Did Sassen (2002) really mean altucumulus evolving into cirrus? Seems unusual.

14995/14: What are ’cloud forming trajectories’?

14995/19: Might want to add this paper: Hobbs, P. V., G. C. Bluhm, and T. Ohtake, 1971: Transport of ice nuclei over the North Pacific Ocean. Tellus, 23, 28

14997/1: The many references to ’major’ dust events should be scrutinized since no definition is given of the term. If they occur once a month, they are not unusual. So, what is being described as a ’major’ versus a ’minor’ dust event?

14997/12 and throughout the paper: Wording like "ice nuclei observations" is a bit odd because of the use of the plural. It would be to replace it with " ice nucleus observations". The analogy for this is that "cloud observations" is better than "clouds observations".

14997/20: Was the loss of small particles by diffusion to the tube walls considered?

14998/11: The sampling rate of the deposited particles is not given. That rate, and the number of detected IN are factors in assessing the sampling error bounds, which should be stated.

14998/12-13: Was sampling onto the silicon wafers compared with with other methods of IN detection? Is there, perhaps, a need here for a caveat that relative values of the measured IN concentrations are more definitive than the absolute values? Is linearity of

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the actual to detected IN concentrations assured, i.e. is the absence of any saturation or depletion effect

14999/7-8: The model data are "..related to .." or are actually what is being said? Also, the designation, PM10dust is confusing to me, because it does not give a rapid distinction between measurement and model data. Perhaps the superscript could be changed from 'dust' to 'model'.

15001/12 - Fig 6: Since the dust has been broadly distributed by the time it arrived in central Europe, why are there five distinct pulses? From Fig. 6 it appears that a diurnal patterns was superimposed on the signal with low values at night and high during the day. How can this be explained? The peaks appear (reading from the plot) just before noon each day, so neither greater atmospheric stability, nor mixing offer simple explanations. Does the model indicate pulses of arriving material, or changes in vertical structure? Is the daily updating of dust concentration (14999/16) a possible source of this?

15001/22: The local effect argument is not very convincing unless there were distinct differences between the first and second half of the event. Is it certain that only reductions in IN could be ascribed to these sub-scale phenomena? There are numerous reports in the literature about large upward anomalies of IN counts accompanying thunderstorms.

15002/2 - Fig.7: Is the time scale on the abscissa correct? 28.05, 29.05 .. or 28.5, 29.5? Also, it should be clarified which IN points are included in the insets (it seems that 7 points) vs the those in the time series (13 overlapping the model data) vs 15 given in Table 2.

15002/17: Does 'significantly' refer to a statistical test, or just a subjective assessment?

15002/27-28: How were the +/- limits determined?

15003/13: Does the total number include all bins or just 1-7?

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15003/22: The figure number should be 9, not 8.

15003/29: How can the deviation from the 1:1 line be stated to be insignificant? Only three of the points have error bars crossing the 1:1 line.

15004/2-3: The analysis here given appears to be, basically, a comparison of the slopes of the IN supersaturation spectra during the dust event and at other times during 2008/9. For this conclusion to be accepted, it has to be demonstrated that most, if not all other materials that may be potential IN exhibit appreciably different slopes. It would also have to be demonstrated that changes in size distribution have negligible effect on the slope. This is a specially difficult point to establish when mixtures of material may constitute the IN, as is likely to be the most frequent situation.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 14993, 2010.