

Title: Influence of the ambient humidity on the concentration of natural deposition ice nuclei
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This paper presents experimental results on a very important topic of atmospheric research, the ability of aerosols on deposition ice nucleation. Among the ice nucleation modes probably deposition ice nucleation is of least understood. The authors try to find correlation between IN concentration and relative humidity of air. In general the paper is clearly written, well organized and scientifically sounds, however a clear statement of the major goal of the study is missing.

I recommend the paper for publication in the Atmospheric Chemistry and Physics.

Nevertheless, I list some comments and questions that can be taken into account for a revision before publication:

- The experiments are clearly described, however it would probably be interesting to know also the ambient temperature.
- Furthermore, the usage of relative humidity is ambiguous throughout the manuscript, therefore I suggest the use of RH_w and RH_i .
- p. 16699 line 10: should read “ice-nuclei content of the atmosphere”
- p. 16700 line 26 to p. 16701 line 5: What in the present and in the previous work was done is not really clear, this part should be reorganized.
- p. 16701 line 9: If the air is exchanged inside the chamber, I do not see why the number of aerosols should stay constant. It depends on the ambient aerosol concentration, isn't it? Thus, it can vary.
- p. 16701 line 20: “reach a size large enough”: how large is it? Can you please provide some numbers?
- p. 16702 line 15 to 18: This part should come to Experimental. Furthermore, the saturation conditions described here are meant inside the chamber, aren't they?

- p. 16702 line 23: What was the reason that the cold chamber could not reach the expected temperature? The too high ambient temp?
- Figure 3 and its discussion are the most important parts of the study. The clear trend of IN concentration vs. RH indicates for me that the RH is the most relevant factor, not the rain itself. Therefore the statement near line 18 on p. 16703 can be revised. Actually, I do not see why a linear trend should be sought in the plot. Furthermore, I do not understand, why the IN concentration should depend on the RH. The discussion regarding pores and cavities provided by the authors seems to be plausible, but not regarding the IN concentration. I think one should ask himself/herself, what one exactly measures. I would say it is not the IN concentration which was measured, but the concentration of IN which were activated at -25°C and 15% ice supersaturation (i.e. the formulation in the paper for me is too loose). In this case the discussion provided by the authors is acceptable, since the nuclei had already been “prepared” (i.e. pre-processed) by the RH of the air. Actually this is somehow mentioned by the authors near line 19 on p. 16704.
- If the former issue holds, the concentration of effective IN is only relevant at the place of the measurement. So why is it important to make measurements on the ground? It should be measured inside the clouds, which would give some information about the generation of ice particles.
- The back trajectories provided in the manuscript should be discussed in more detail. It would be desirable to show a common map of the trajectories. I am not familiar with the geography of South-America so I could not really figure out from which part the air masses are coming. What more important could be is to identify the areas from which the air masses are coming. It could give us some clues about the origin of the possible IN (e.g., whether they can be biological, mineral, etc).