

Reply to Anonymous Referee #2

The answers are in red.

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.

The major goal of this work is the study of the factors that regulate the effectiveness of aerosols on deposition ice nucleation. It will be included in the revised manuscript.

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.”*

We found that the ambient temperature (T_a) do not influence the IN concentration (not mentioned in the original manuscript). The Figure below displays the IN concentration as a function of T_a . The temperature of the measurements varied between 5°C and 30°C and the results do not seem to indicate that there is any particular tendency of the IN concentration values with T_a variations.

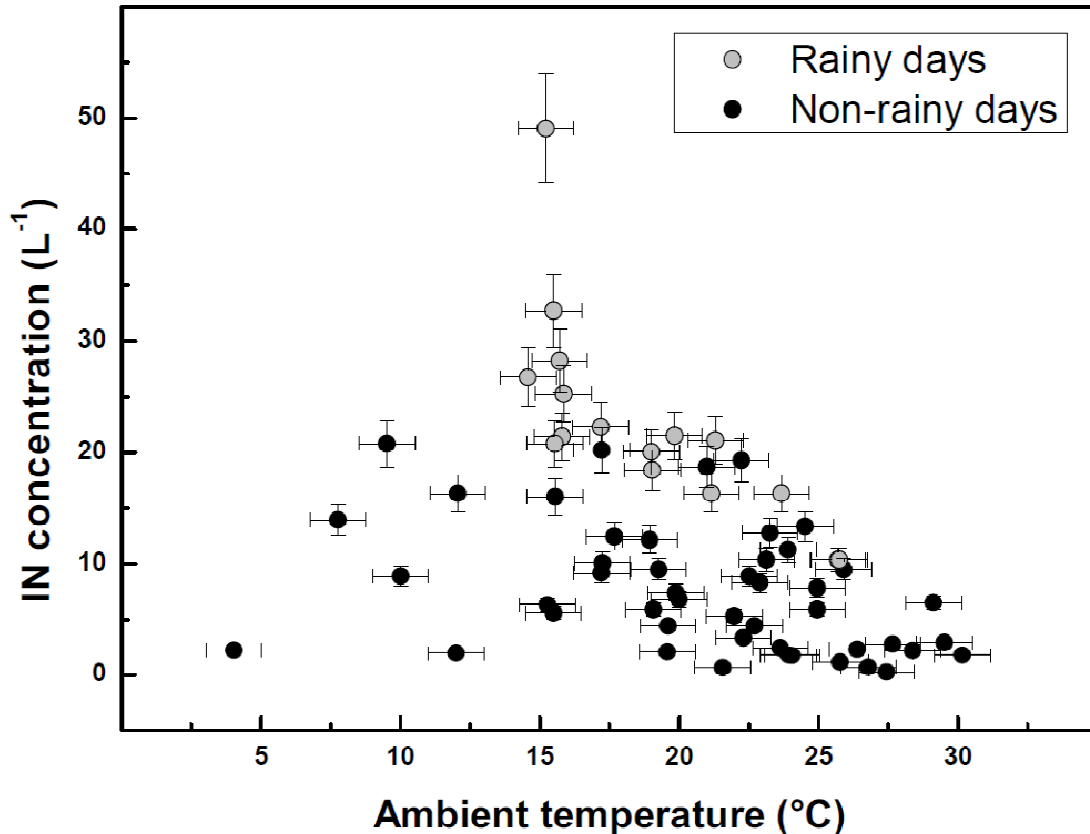


Figure: Data of IN concentration at different ambient temperatures.

- “Furthermore, the usage of relative humidity is ambiguous throughout the manuscript, therefore I suggest the use of RH_w and RH_i.”

Agreed. This change will be implemented.

- p. 16699 line 10: should read “ice-nuclei content of the atmosphere”

Agreed. This change will be implemented.

- 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.

This paragraph will be rewritten.

- 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.

It is assumed that the aerosol concentration of the air inside the chamber is the same (or quite similar) as the aerosol concentration outside the chamber (ambient). In fact, the air within the chamber was renewed at the beginning of every experiment.

- p. 16701 line 20: “reach a size *_large_ enough*”: how large is it? Can you please provide some numbers?

Figure 1 shows image of crystals once they grown on the sugar solution. It is possible to observe that the crystals reach mm sizes.

- 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?*

It was already mentioned in the Experimental section that the measurements were performed in deposition nucleation mode. We would like to recall the readers the experimental conditions here.

Yes the saturation conditions described here occur inside the chamber.

- p. 16702 line 23: *What was the reason that the cold chamber could not reach the expected temperature? The too high ambient temp?*

In general, the reason was that the cold chamber engine was not working properly.

- *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.*

We cannot assess that the RH is the most relevant parameter because the measurements of high IN concentration were obtained with high RH during rainy days. We did not measure IN concentration at high RH (> 70%) without rain; then, it is not possible to discard that both effects (rain and high RH) are required.

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.

We do not follow what the Reviewer points out in this comment. We think that in several parts of the manuscript (Abstract, Introduction, Experimental, Results and discussion

sections) it was emphasized that we measured the IN concentration activated at -25°C and 15% ice supersaturation of ground level air.

- 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.

In the experiments, the thermodynamic conditions occurring in real clouds cannot be exactly reproduced; however, the measurements on the lab are relevant because in general the thermodynamic variables can be controlled or determined so that they allow for straightforward interpretation of the results. The lab experiments can add to understand the factors that govern the ice nucleation mechanisms. For instance, the findings of the current work would not be obtained from measurements inside clouds.

- 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).

The back trajectories Fig (see below) was changed as required by the Reviewer. The new Fig displays in more detail from which part of the continent the air masses are coming. Unfortunately, we do not have the information of the IN characteristic (biological, mineral, dust, etc) from the different trajectories. All we can say is that the enhancement of the IN concentration measured in lab is not linked to the origin of the air masses.

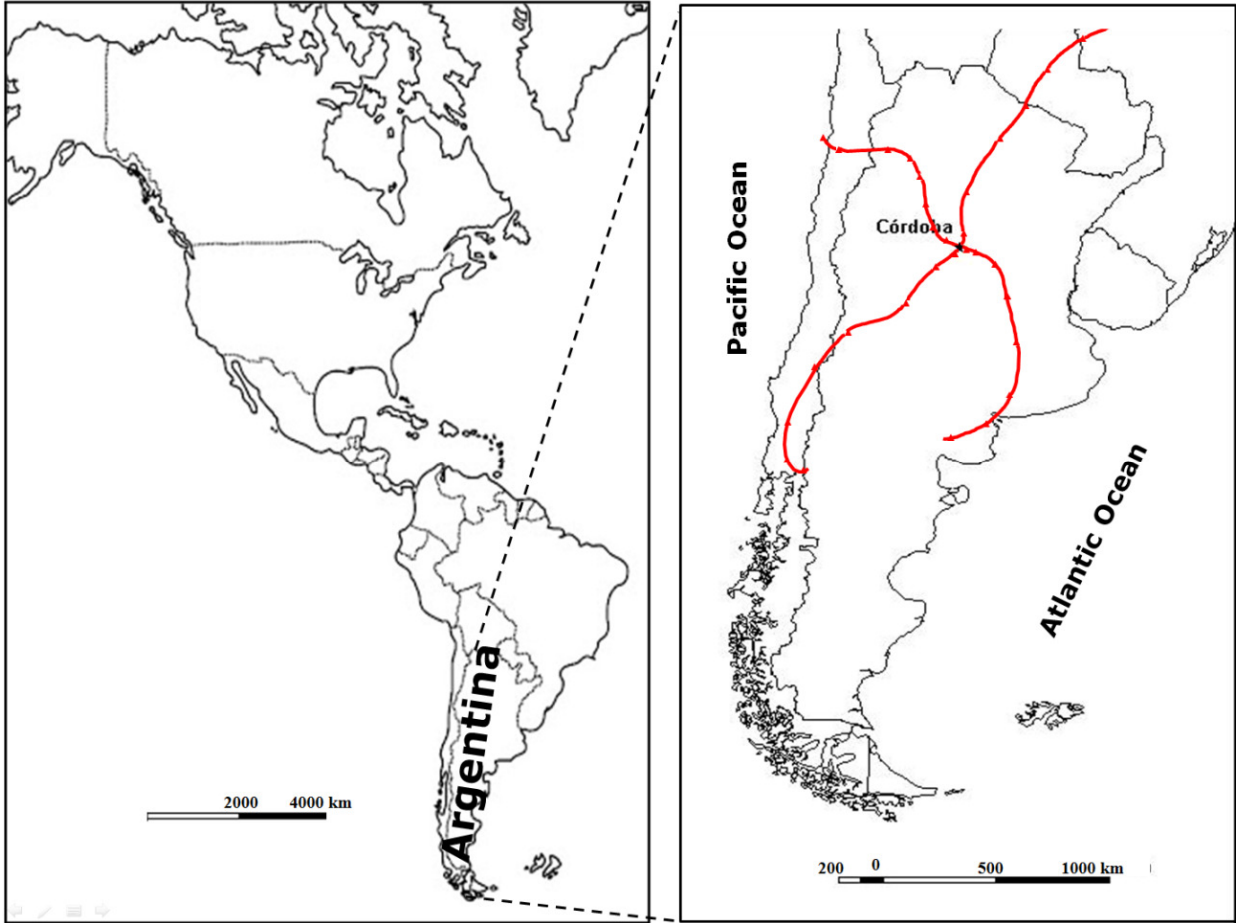


Figure: Air mass pathways of different categories. In a larger scale is showed the map of Argentina.