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# ***Interactive comment on “Influence of the ambient humidity on the concentration of natural deposition ice nuclei” by M. L. López and E. E. Ávila***

**Anonymous Referee #3**

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This paper reports on an intriguing observed correlation between deposition ice nucleus concentration and the ambient relative humidity. By ambient the meaning is the environment from which the aerosol particles were sampled; the relative humidity at which ice nucleus concentration is measured is fixed at 15% with respect to ice at a temperature of -25 C. The finding is important for several reasons: the correlation itself adds understanding to some prior observations (e.g., Prenni et al. 2013), the results are demonstrated to be independent of air mass origin, decreasing the possibility that geographical source is confused with relative humidity, the measurements are made using a very direct method that allows for straightforward interpretation, and although

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not mentioned in the paper, the results also provide valuable measurements from the southern hemisphere where such data are sparse (the authors may wish to point this out). I recommend that the paper be accepted after the following comment, as well as some minor points are considered:

More details on the air/aerosol sampling need to be included, and specifically, more information on residence time of the particles are needed to help with interpretation. Is it correct that the air is drawn continuously into the chamber from outside? Where does the air/aerosol sampling occur, and where is that located relative to the relative humidity sensor? Is there any cutoff filter or other size selection, or at least can an upper aerosol size be estimated for the sampling lines? Most importantly, what is the residence time of the air/aerosol within the sampling tube and then within the controlled relative humidity chamber? Presumably there is at least one, perhaps more than one, time scale associated with the response of aerosol particles to new ambient conditions. It seems to me that if that time scale is much less than the residence time in the cloud chamber, then one would not expect to observe any effect of outdoor relative humidity. The measurements therefore suggest that the time scale is of the same order or greater than the residence time in the chamber. This may be helpful in evaluating possible mechanisms, and therefore is important to discuss.

Other suggestions and corrections:

Line 9, abstract: "ability" probably should be "effectiveness".

Line 10, abstract: I recommend deleting the sentence "These results are consistent with previous results." It is vague and may give some readers the impression that the measurements are repeating others already made. I do not think that is the case.

Line 25, page 16698: I would say "can serve as IN" instead of "serve as IN".

Line 21, page 16699: After "under thunderclouds" is a reference needed, or is this referring to the work cited on lines 24-25? Please clarify in the paper.

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Lines 28-29, page 16701: Is it verified that the instrument can measure at ice supersaturated conditions without artifacts?

Lines 7-9, page 16703: This statement seems unlikely to be correct, judging from the uncertainty bars on the data points. The points show scatter much greater than the bars. Can natural variability really be ruled out? If so, it needs to be shown.

References: I recently became aware of the following work by Wright et al. (Aerosol Sci Tech, 2014) that appears to be relevant to this paper. It should be cited and discussed. <http://www.tandfonline.com/doi/abs/10.1080/02786826.2014.968244#.VbgxlfIVhBc>

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Interactive comment on Atmos. Chem. Phys. Discuss., 15, 16697, 2015.

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