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Interactive comment on "A new temperature and humidity dependent surface site density approach for deposition ice nucleation" *by* I. Steinke et al.

Anonymous Referee #1

Received and published: 13 August 2014

I support publication of the paper. The experiments are well described and the data analysis is comprehensive. I have a few comments that the authors may wish to consider.

Deposition nucleation as homogeneous or immersion nucleation in pores and cavities. The recent paper by Marcolli [ACP, 14, 2071-2104, 2014] presents a pretty compelling case that what is typically interpreted as deposition nucleation is, in fact, condensation of water into cavities or pores, followed by freezing in the immersion mode or homogeneous freezing, if the temperature is low enough. Lines 1 through 5 on pg. 18511 of this paper are consistent with that hypothesis. Higher RH with respect to ice is required at higher temperatures while freezing begins at low RH for the colder

runs. (See "Conclusions and Outlook" in Marcolli's paper.)

I am not suggesting that the authors re-analyze their data in light of that paper, but I think a discussion of the mechanism in light of these results is worthwhile.

Why is a parameterization for Arizona Test Dust in the deposition mode necessary? The authors advertise this in the abstract as a surrogate for mineral dusts, yet there are a lot of natural dusts out there that do not resemble Arizona Test Dust. Why is this parameterization a valuable addition to the literature, especially considering that measurements of ATD in the deposition mode have already been made.

I think the authors could have made the case for these measurements and this analysis a bit more forcefully.

Minor point pg. 18512, lines 24-25: "...T represents the numerical value of the prevalent temperature..." What is a prevalent temperature? Is that the average temperature in the chamber? This is a bit confusing to me.

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