

Interactive comment on “Charge induced stability of water droplets in subsaturated environment” by J. K. Nielsen et al.

Anonymous Referee #1

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The authors report about the influence of charge on the surface of water particles (either liquid or solid) on the partial pressure of water vapor close to the surface. They present the theory of vapor pressure of charged hydrometeors, i.e. the depression of saturation pressure above a charged surface as a function of surface charge density because of the attraction of polar gas molecules to a charged droplet. The theory predicts that sufficiently charged water droplets are in thermodynamic equilibrium in a slightly subsaturated environment. To prove the theory the authors performed experiments of charged water droplets in an electrodynamic balance in equilibrium with an ice surface at temperatures slightly below 273 K.

The manuscript raises an interesting question and presents an elegantly designed experiment to prove the validity of the derived theory. It is well written and appropriate

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figures are provided to support the argumentation. However I feel additional discussion of some aspects of the theory and experiments of the authors are needed as are in-depth discussion of theoretical work and experiments by other authors before it can be accepted for publication.

(1) Theory: The authors do not cite and discuss previous work. For the first time J. J. Thomson (1928) studied the influence of charge on the vapor pressure of a small drop. Later for example Rusanov (1978), Cohen et al. (1987) and Lapshin et al. (2002) addressed the problem again. The authors derive eq. (3) by neglecting the Kelvin term which may be justified for particles larger than 10 μm in radius. A much more thorough derivation is given by Lapshin et al. (2002) who derive the full Kelvin equation for charged polar droplets. It is not obvious - but easy to show for the authors - that their solution is a good approximation of the full solution for the sizes and surface charge densities considered in the manuscript. My suggestion is to include a line for the full solution in Fig. 1 of the manuscript. Especially for somebody interested in a possible effect for atmospheric particles (e.g. condensational growth on small particles) this is important to know.

(2) Experimental setup: The authors explain why the terminal radius becomes very sensitive to small temperature variations, which is absolutely correct. However, the experimental data of Fig. 3 do not show much fluctuation in radius even at the latest times of the experiment when it should become sensitive to temperature fluctuations. What is the reason for this?

The authors also use the initial slope of the evaporation curve to determine RH using their theoretical model. How much could RH drift over the duration of the experiment which last a few minutes? How do the RH values determined the way described compare to those from the temperature measurements of the ice surface?

How accurate is the determination of radius from camera images? Is it possible that

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here is a bias in radius determination over the course of the radii measured, namely going from 34 μm to 13 μm in Fig. 3?

How clean is "clean" water? The evaporation process concentrates trace impurities which will also lower the water activity of the droplet. For the example of Fig. 3 a reduction of water activity to 97 % means a mole fraction of 1.5 % for a simple dissociating salt assuming Raoult's law. That corresponds to an initial impurity of 0.08 % molar which I guess the authors have easily avoided. But some discussion of this issue would be helpful for the reader.

(3) Results: The initial paragraph discussing the experiment of Fig. 3 is not sufficient in its present form and needs to be extended considerably before publication. Here the authors waste an enormous potential of their elegant experiment. The reader immediately asks himself why the experiment stops at 450 s. Before, the authors convince us that there will be a terminal radius for the particle after initial evaporation (even though the experimental value might be fluctuating considerably with time due to fluctuations of the ice temperature and hence RH). But in the experimental proof of the effect they show only the part of the experiment where the first deviation from the linear relationship between surface area and time occurs. In my opinion the authors have either to show the data for a particle actually retaining its terminal radius for a time span about a factor of 2 larger than the evaporation period, or explain to the reader in detail why it was impossible to do such an experiment. I feel the second paragraph of section 4 and Fig.4 can not compensate for such a clear experimental proof.

In addition there are other experiments in the literature (especially those of Taflin et al., 1988) who are slightly different (evaporating of water droplets into dry nitrogen and evaporation of some organics in dry nitrogen). Based on their theoretical insight the authors should discuss why the effect of charge was not visible in the data of the organics why it may be even visible in the data for water (Fig. 8 of Taflin et al., 1988)?

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(5) Perspectives: Closely connected to my comment above: The authors should discuss in a bit more detail then their last sentence which type of EDB measurements they believe will be influenced to what degree by the charge effect.

Minor comments:

- line 5: so-called should be so-called.
- Fig. 2: I find the ellipsoidal symbol in the caption inside the figure confusing, just show a straight line. I also suggest to give the charge in elementary charges to be consistent with Fig. 1. In addition I doubt that the area in which the terminal radius is larger than the Rayleigh instability radius is actually rectangular, or I am I wrong?
- Table 1: you should also give the symbols for eq. (6) since it is actually part of eq. (5).
- Fig. 3: temperature information is missing.

References:

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