

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

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Final Response to referee # 1

Referee # 1 is positive about the manuscript, and accepts the conclusions, but also points out some missing details in the presentation theory, experiment and results, which he/she believes are crucial for publication in ACP. We acknowledge that inclusion of the suggestions greatly improves the manuscript. We mostly accept the suggestions, and they will all lead to changes in the text. Here we go through the comments of referee # 1.

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### **“(1) Theory: The authors do not cite and discuss previous work.”**

First of all we would like to thank the referee for pointing out literature that we were not aware of. Especially the article by Lapshin et al. (2002) in Russian J. Phys. Chem. We were actually aware of Cohen et al. (1987), but had chosen to omit that reference because we believe that the expression they derive for the reduction in partial pressure only include the Coulomb interaction and not the charge-dipole term which is by far the most important. This is also the case for the other references mentioned, except for Lapshin et al, who correctly derives the theory with both the Kelvin effect, Coulomb effect and charge-dipole interaction, and conclude that the charge-dipole interaction is dominating for all relevant aerosol sizes. Curiously Lapshin et al. end up with deriving the limit where  $k_B T \gg \sigma_q d / \epsilon_0$  where  $\ln e / e_\infty$  follows a  $r^{-4}$  law, while our experiment is in the opposite limit,  $k_B T \ll \sigma_q d / \epsilon_0$ , where the logarithmic partial pressure correction follows a  $r^{-2}$  law. We take the point of the referee and include a brief mentioning of all the literature in the manuscript, with emphasis on Lapshin et al. and with an explanation of why we are in the  $\ln e / e_\infty \propto r^{-2}$  regime. We will as referee # 1 suggests include the Kelvin term in figure 1.

### **“(2) Experimental setup: [...] experimental data of Fig. 3 do not show much fluctuation in radius”**

What is actually going on in the experiment is a slow temperature oscillation ( $\delta T < 0.1$  K) with a period of around 25 minutes caused by the temperature control device. On the timescale of one evaporation experiment this low frequency noise is seen as a slow drift added to a bias which unfortunately varies from experiment to experiment.

### **“How much could RH drift over the duration of the experiment which last a few**

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minutes?”

We include, as proposed by referee #1, a longer time interval in figure 3 of the manuscript (attached figure # 1 below), where it can be seen that during one single experiment the terminal area practically does not drift, which means that resulting RH does not drift either. But between experiments the temperature instability may cause an RH variation of around 1 %.

**“How do the RH values determined the way described compare to those from the temperature measurements of the ice surface?”**

The reason we chose not to calculate the RH from temperature, is that the uncertain temperature determination results in an estimated uncertainty of +/- 1 % on the RH which is a relatively large uncertainty in this context.

**“How accurate is the determination of radius from camera images?”**

The uncertainty on the droplet diameter has been estimated to be below 1.56  $\mu\text{m}$ . This number has been incorporated in the error bars of figure 4. We will make a note about that in figure 4.

**“How clean is "clean" water?”**

For these experiments we used NANOpure(tm) water, > 200nm filtrated, with a resistivity greater than 18.2 M $\Omega$  cm. Even when the concentration of possible ionic impurities is increased by a factor of 1000 during the evaporation process, their mole fraction is kept below  $10^{-4}$ . We will make a note about that in the paper also.

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**“(3) Results: The initial paragraph discussing the experiment of Fig. 3 is not sufficient in its present form”.**

It is a good point raised here by the referee. During our work we have become so used to the fact that this is actually happening, that we have simply forgot to illustrate it. We apologize for that. The doubling of the time span of figure 3 should convince the reader that the terminal radius is actually reached.

**“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)?”**

Taflin et al. (1988) look at fast evaporation into dry nitrogen where the water vapour pressure is practically zero, or if related to our terminology we could say that  $\ln(RH) \rightarrow -\infty$ , i.e. far “to the left” of our domain in figure 4. Here the surface charge density would diverge, or rather, the droplet is reaching the Rayleigh limit and simply breaks up. We will comment on this in the article. Referee #1 is hinting that our view could explain why some measurements in Fig. 8 of Taflin et al. (1988) diverges from their theory. That is tempting but it requires a little more insight into their experiment than we have, so we will leave that untouched.

**(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.”**

We will elaborate a on this in the paper. Generally we believe that if one measures a parameter which is strongly dependent on the relative humidity, like for instance a ice nucleation rate, one should take into account that  $e_{\infty}$  is smaller than the actual water

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vapour pressure experienced by a charged droplet.

All the minor comments of referee #1 are relevant and the errors will be corrected. Especially with respect to this remark:

**“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?”**

We have to admit that it is a mistake from our side. The rectangle just indicates the range of our experiments. The purple area is where the terminal radius is larger than the Rayleigh instability radius.

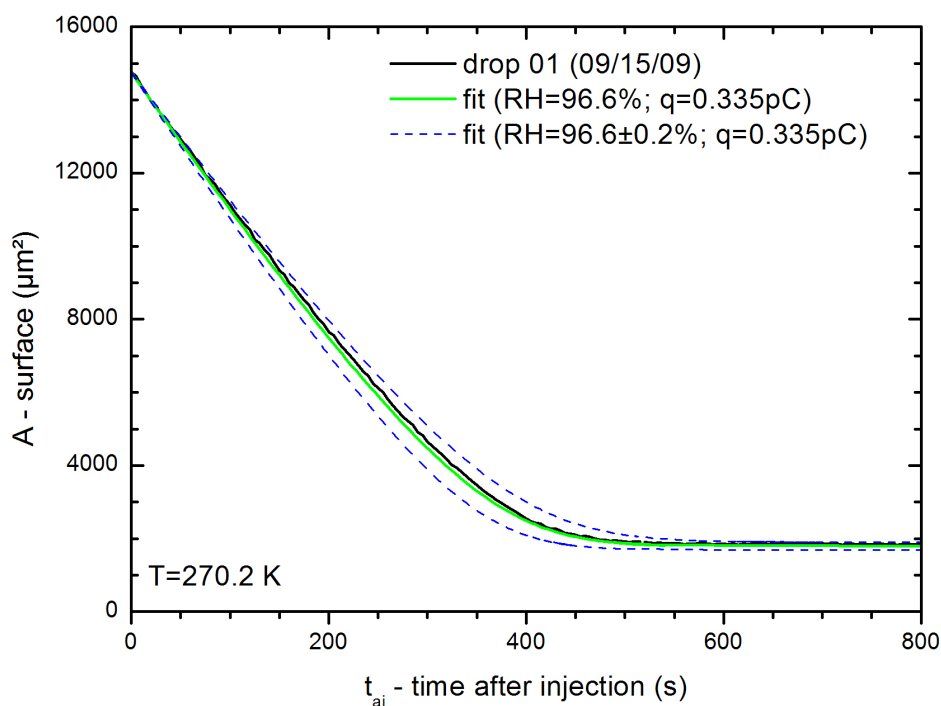
On behalf of all authors, Johannes K. Nielsen

## References

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**Fig. 1.** Example of evaporation curve with extended time axis.

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