Atmos. Meas. Tech. Discuss., 7, C4233–C4239, 2014
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Atmospheric Measurement Techniques Discussions

AMTD

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Interactive Comment

Interactive comment on "A new electrodynamic balance design for low temperature studies" *by* H.-J. Tong et al.

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Received and published: 15 December 2014

Response to the comments of Referee Thomas Leisner

The manuscript describes an electrodynamic balance (EDB) designed for low temperature studies and demonstrates its capabilities by reporting water droplet evaporation rates and immersion freezing rates at temperatures between -35 âŮęC and 0 âŮęC. The EDB consists of coaxial cylindrical segments in a design introduced by Heinisch et al. 2006, which facilitates the exposure of the levitated particle to and core/sheath gas ïňĆow. The novel aspect is the utilization of the gas ïňĆow for particle cooling, while the trap electrodes and housing are not actively cooled. Some of the characteristics of the apparatus are detailed by describing measurements of droplet evaporation rates

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and immersion freezing rates performed with this setup. General comments: I feel that in its current form, the manuscript is not suitable for publication in AMT. Even though the idea of cooling a levitated particle purely by gas ïňĆow is interesting and worth exploring, this manuscript does neither assess the merits and disadvantages of such a design nor does it give the necessary information for this.

Response: We thank the referee for reviewing the paper and for providing very pertinent comments. We respond to these comments in detail as follows. We have highlighted the changes to the text (in yellow) and have attached the altered text as a supplement.

Referee comments 1: Below follow some important points, which should have been addressed in much more detail: There is too little detailed information on the overall experimental setup (sizes, materials, measures) apart from the cursory ïňĄgure 1.

Response: We now provide much more detail on the electrodes, CEDB chamber, and liquid nitrogen Dewar. We also describe more specifically the sizing procedures. (line 29-30 of Page 4, line 1-19 and 24 line of page 5, line 11-22 of page 6, line 24 and 26, 27 of page 7, line 3 of page 8, line 5-21 and 24-25 of page 9)

Referee comments 2: There is too little experimental characterization on the EDB performance: No information is given on the stability of particle trapping under the various experimental conditions, or on the admissible range of gas ïňĆow, and how it affects the performance of the EDB.

Response: We now provide more detail about the temperature within the CEDB by measuring the homogenous freezing temperature of the supercooled water droplets (line 9-13 of page 10 and Figure 5 (b)). In addition, we have added more details on the trapping stability of the CEDB system under three gas flow conditions (line 1-21 of Page 7).

Referee comments 3: The axial and radial temperature proïňĄle inside the EDB is

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qualitatively discussed but no measured data (ideally at various set temperatures) are given.

Response: We performed additional measurements and determined the temperature profile around the null point of the trap (this is now line 5-21 of page 9 and table 1 on page 27). We now provide more information about the measured temperature gradients within the cell (see Table 1 within the revised paper).

Referee comments 4: The main drawback of the chosen design is the use of an integrated liquid nitrogen cold trap for the precooling of the gas ïňĆow, which inevitably results in an extremely dry gas ïňĆow around the droplet. This excludes experiments at environmentally relevant conditions and leads to a rapid evaporation of the droplet even at low temperatures. This effect is aggravated by the fact that the droplet evaporates not into a stagnant atmosphere, but into a gas ïňĆow. The authors mention this fact brieïňĆy but do not discuss its implications or possible remedies.

Response: The first version of the instrument presented in this publication was only tested for dry conditions using liquid nitrogen as coolant. We mention now the possibility to perform experiments at higher RH in future studies (p. 9, line 27-32). However, it should be noted that one benefit of using the liquid N2 and the resultant low RH is that it removes an experimental uncertainty that is often present in ice nucleation studies what is the environmental RH. Within the CEDB using liquid nitrogen it is clear that the RH is very dry.

Referee comments 5: A rapidly evaporating droplet will assume a temperature that is lower than its surrounding. This effect is neither discussed in the manuscript nor is it considered in the following experiments on evaporation and immersion freezing.

Response: We now address the effect of evaporation on the temperature of the droplets. (26-30 lines of page 10, 1-6 line of page 11).

Referee comments 6: Even though I understand that the experiments described are

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to illustrate the capabilities of the EDB, I am missing important information, which the authors should provide: Evaporation rates of water droplets: How do the measured evaporation curves compare to theoretical expectations?

Response: To model the evaporation rates one needs a value for the accommodation coefficient (alpha) of water vapour into liquid water. This value is highly controversial within the literature with alpha values reported within the last decade varying between ∼0.1-1 (Eames et al., 1997, Li et al., 2001, Winkler et al., 2004, Davies et al., 2014). This factor of ten presents a large uncertainty for the modelling of cloud water. Whilst it is possible for us to use the theoretical model of Davies et al. (2014) to calculate alpha from our measurements (and we have) we intend to perform more experiments over a wider range of conditions before we publish this result, hence the mention of the forthcoming paper. We believe that publication of this value before these further experiments have been performed would be premature and could further muddy the water in this controversial arena. We further justify this approach by pointing out that we choose AMT for this initial paper because it was primarily to highlight our new CEDB approach not to provide new data.

However, we are mindful that the paper will benefit from more useful data that the community can utilize. Towards this aim we have much extended the amount of data presented on the freezing of droplets.

Referee comments 7: Why is the time for evaporation proportional to the radius?

Response:

According to the Maxwell equation the radius change rate of a SWD is proportional to the radius reciprocal of it. Under the same environment, larger SWD will have smaller radius decay rate. In other words, the life time of larger droplet will be longer. This is now explicitly mentioned on p. 11, line 25-27.

Referee comments 8: What does this tell us about the ïňĆow characteristics?

Response: As explained in response to the above comment the evaporation time depends on the initial particle size. Flow conditions were constant over all experiments reported here as described in section 2.1

Referee comments 9: How do the measured evaporation rates compare to droplet evaporation into a stagnant atmosphere of zero RH?

Response: We did not investigate the evaporation into a stagnant atmosphere as described on p.7, line 2-10.

Referee comments 10: What are the expectations based on the literature on evaporation into an laminar gas ïňĆow?

Response: The corrections due to Stefan flow are negligible, we now state this in the manuscript.

Referee comments 11: Immersion freezing of birch pollen washing water: How do the evaporative cooling and the continuous volume reduction affect the freezing rate?

Response: The reviewer correctly points out that evaporation will affect the droplet temperature. However, in PWW droplets where freezing was observed, this happened within 0.3 seconds after injecting the PWW droplets into the trap. In this short time the droplets do not evaporate significantly, see Figure 4a. In a very small number of PWW droplets (< 5%) freezing occurred after 0.3 seconds but these droplets were not considered in the data analysis for Figure 7 and 8 (this is now explicitly mentioned on p. 14. lines of 9-15).

Referee comments 12: How were the ice fractions in Fig. 7 determined?

Response: 50-158 droplets were analysed at each temperature. The number ratio between frozen to total (frozen + unfrozen) PWW droplets was calculated as ice fraction. At very high and low temperatures, less PWW droplets were analysed. (more details are given now at 2-7 lines of page 14).

Referee comments 13: Why are there no vertical error bars?

Response: The fraction of particle freezing at a particular temperature (i.e. the y-axis in Figure 7) was calculated as mention above (equation3, p. 13). Thus it is not possible to determine an error bar for ice fraction.

Referee comments 14: How many droplets have been investigated at each temperature?

Response: 50-158 droplets will be analysed at each temperature. The numbers of PWW droplets we used here are: 158 for -15.2 ($\pm$0.7)C, 82 for 18.8 ($\pm$0.85)C, 151 for -20.5 ($\pm$0.65)C, 104 for -22.5 ($\pm$0.65)C, 125 for -24.2 ($\pm$0.65)C, 100 for 27.5 ($\pm$0.5)C, 50 for -30.5 ($\pm$0.5)C, and 60 for -32.2 ($\pm$0.5)C (these numbers are now given in Figure 7 and on page 14, lines of 2-7).

Referee comments 15: When comparing to published data, the authors have to take into account the sample volume, not just the concentration of active material.

Response: We have discussed more about the influence of sample volume on the freezing fraction in our manuscript. (9-15 lines of page 14)

Additionally, we have now performed more experiments to characterise the ice nuclei (IN) activity of PWW:

1) We found the IN activity of 47 mg/ml PWW is higher than the 5 mg/ml PWW.

2) We compare the IN activity of PWW before and after the selective removal of the protein component of the PWW. We find that the IN activity of 5 mg/ml PWW is higher after removing the protein component for the same given weight of extract (this is now described in Figure 8 and on p. 16, line 16-32, P. 17, line 1-15).

This additional work has led to two more co-authors.

Referee comments 16: Presentation quality: I enjoyed the introduction. The rest of the manuscript concentrates too much on minor issues but misses many important points.

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Response: We believe that the detailed answers provided above and the additional information added to the revised manuscript provide sufficient information on all the points raised by this thorough review.

References:

Davies, J. F., Miles, R. E. H., Haddrell, A. E., and Reid, J. P.: Temperature dependence of the vapor pressure and evaporation coefficient of supercooled water. J. Geophys. Res. Atmos., 119, 10931-10940, 2014. Eames, I. W., Marr, N. J., and Sabir, H: The evaporation coefficient of water: a review. Int. J. Heat Mass Transfer. 40, 29639-2973, 1997. Koop, T., Luo, B., Tsias, A., and Peter T.: Water activity as the determinant for homogeneous ice nucleation in aqueous solutions. Nature, 406, 611-614, 2000. Li, Y. Q., Davidovits, P., Shi, Q., Jayne, J. T., Kolb, C. E., and Worsnop, D. R.: Mass and thermal accommodation coefficients of H2O(g) on liquid water as a function of temperature. J. Phys. Chem. A, 105, 10627-10634, 2001. Winkler, P., Vrtala, A., Wagner, P., Kulmala, M., Lehtinen, K., and Vesala, T.: Mass and thermal accommodation during gas-liquid condensation of water. Phys. Rev. Lett., 93, 075701, 2004.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/7/C4233/2014/amtd-7-C4233-2014-supplement.pdf

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Interactive comment on Atmos. Meas. Tech. Discuss., 7, 7671, 2014.

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