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Atmospheric Measurement Techniques Discussions

Interactive comment on "A technique for quantifying heterogeneous ice nucleation in microlitre supercooled water droplets" *by* T. F. Whale et al.

Anonymous Referee #1

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In their manuscript, the authors present an experimental setup, which has been used for the quantification of heteorogeneous ice nucleation of droplets with a size of about 1 mm. The setup, which is called mL-NIPI, is the combination of an enclosured cold stage with an hydrophobic surface and a digital camera. Generally, the manuscript is well written and structured. After describing the setup, the authors also present calibration data, discuss potential artefacts and show several experimental results. Overall, the manuscript gives a complete and round picture.

However, the idea of cold stage experiments is quite old. Cold stages have been used for decades (e.g., there is a very old paper from Roberts and Hallett, 1967).

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And even if the cold stages differ from each other in several details, they are always based on the same fundamental idea. What I missed here is a discussion what's really new. Furthermore, experimental data measured with this instrument are already presented in previous papers (as also stated in the text there is a very high ranking level paper presenting K-feldspar data). Except the detailed discussion of the setup (and maybe several Snowmax and AgI data) there is nothing new in the manuscript. I was wondering what was the reason to write this separate paper. The authors should motivate this much more.

Apart from that major point, I see no argument against a publication in AMT. Several specific comments, suggestions and technical corrections for additional minor revisions are listed in the following:

Specific comments and suggestions:

abstract: I would recommend to shorten at least the first part of the abstract. I think it would be enough to give most of the information (radiative properties of clouds, ...) in the introduction section.

introduction: The authors try to highly motivate their experimental concept. Generally, this is totally okay. However, my feeling is that they overdid it. By reading the introduction one can get the impression that experiments with cloud sized droplets are completely senseless and not needed. But this is absolutely not true. Droplet freezing experiments with microlitre sized droplets are a very good complement to those with cloud sized droplets, but cannot replace them. On the one hand, using bigger droplets allows the determination of much smaller ns values. But on the other hand, (overlapping) experiments with cloud sized droplets are still needed to show the connection to atmospherically relevant droplet sizes (i.e. relevant amounts of dispersed material). This has to be done because in terms of nucleant surface area the experiments with big droplets (as done at mL-NIPI) are not atmosperically relevant.

p. 9513, l. 19: The authors claim that experiments "can be performed with excellent

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reproducibility". Hovever, this has not been proven in the paper. It is shown that there is a spread of 2-3K for the pure water samples. Furthermore, there is the information included that melting points are average values of 5 measurements. But that's it. I guess, the data shown in the figs. are single experiments. If not, standard deviations should be included. The authors should discuss the topic of reproducibility a bit more.

p. 9514, l. 8: "40 to 50 droplets". This seems to be not true. All experiments shown in the manuscript were done with less than 40 droplets.

p. 9517-9518, section 3.1: The authors discuss the formation of Frost and the resulting artefacts. They also present a solution how Frost growth can be avoided. However, I'm not really satisfied with the phrase "largely eliminated". What does this statement mean? Is there still Frost growth or not? Furthermore, can "largely" transformed into "completely" by using an even higher flow of N2? In other words, what is the motivation to use a N2 flow of 0.2 l/min (increasing droplet evaporation at higher flows?, cooling/heating by the dry flow? Is the N2 flow temperature controlled?)? I was also wondering, why there was no Frost growth problem in the experiments discussed in section 3.2. In 3.1. it was shown that Frost growth can be observed even at -5°C. By turning the dry flow off there should be also Frost growth in the experiments shown in Fig. 5, right?

p. 9520, l. 15: The other substances (Snomax, AgI) should be included in the discussion of Fig. 7.

p. 9521, l. 7-19: Here, the authors discuss the influence of "impurities". I don't get the argument why this is particularly important for large droplet experiments. Small amounts of high efficient nucleants would also dominate experiments with small droplets. The advantage of experiments using small droplets is that they usually observe much more droplets, which makes it statistically more easier to separate the impact of several nucleants. This section should be rewritten being more precisely what is really meant here.

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p. 9531-9536: The quality of the figs. is okay but can be further improved by being a bit more consistent in terms of scales, labels, symbols and colors. For example, I suppose that the K-feldspar data in Fig. 5 and Fig. 6 are the same. Why not using equal colors and symbols?

p. 9535, Fig. 7: The uncertainty in the surface area is estimated by +/-15 percent. The authors should give some information where this number comes from. The authors give a number for the uncertainty of the droplet volume (p. 9514, l. 12), but there seems to be no connection to the estimated uncertainty of the surface area.

Technical corrections:

p. 9536, caption of Fig. 8: The last sentence is not complete.

p. 9536, Fig. 8: it should be "fraction" instead of "freaction"

Interactive comment on Atmos. Meas. Tech. Discuss., 7, 9509, 2014.