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Atmospheric Measurement Techniques Discussions

Interactive comment on "The micro-orifice uniform deposit impactor-droplet freezing technique (MOUDI-DFT) for measuring concentrations of ice nucleating particles as a function of size: improvements and initial validation" *by* R. H. Mason et al.

K. Kandler (Referee)

kzk@gmx.de

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The manuscript of Mason et al. deals with optimization and validation of a combined aerosol particle collection and freezing technique to determine atmospheric ice nucleating particles (INP), which is relatively new in its current state (2013). As actual INP measurement decades still differ by an order of magnitude in their concentration readings, such work is crucial for the progress of INP research. The authors come up at

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the end of their experiments with correction parameters for the MOUDI-DFT technique, which are in the range of 0.7 to 2.0, in average correcting for an underprediction of INP by this technique. Given the comparatively large differences between MOUDI-DFT and their reference technique CFDC (factors of 1.1 and 3.8), however, obviously there are more biases to be regarded in future (not necessarily in the MOUDI-DFT technique only). The paper is concisely written and the results are adequately presented. Appropriate reference to previous work is given. Therefore, I recommend publication after some minor corrections.

General remarks

2233/13-16: 3 or 4 samples were used for calculation the correction parameters. It didn't become clear to me from the manuscript, however, whether the aerosol density data and the confidence intervals were calculated from all samples as a single dataset or for each sampled glass slip separately. Basic question: is there a difference from sample to sample in the corrections derived from the data displayed in Figs. 4 to 6 and the resulting functions shown in Fig. 9, or are they identical or at least very similar for each sample? I would suggest demonstrating this at least in one graph.

There are some forward references ('see below', etc.), which make the manuscript at some points not so easy to read. Please give this a critical read and revise, if possible.

Minor remarks / corrections

2227/14-15 and 2228/1: Why were these previous methods limited to > -25°C and the current one not?

2228/24-2229/16: This reads rather like a summary of a part of the paper than an introduction. I suggest revising and shortening to a few sentences just outlining the approach.

2230/6: Having as first Fig. reference a no. 9 might raise objections from the technical editor.

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2230/7: Consider a new paragraph before ‘To determine. . .’

2233/2: It appears to me that Eq. 2 is only valid if the aerosol density on the substrate is low, i.e. no particle is deposited touching another. Could you comment on that, and if the case, include the restriction?

2234/27 and Fig. 3: This figure is practically useless in current state due to poor resolution. However, I think it makes sense in the manuscript to present it, so I suggest a) taking care that it comes with sufficient resolution in online / print version (let it span the whole page) and b) additionally somehow illustrating the changes in particle density, e.g., by drawing a graph showing the particle density per area as function of the distance of the deposit center in parallel above the image.

2243/22: The following section is rather a summary than conclusions.

Interactive comment on Atmos. Meas. Tech. Discuss., 8, 2223, 2015.