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Atmospheric Measurement Techniques Discussions

Interactive comment on "A DMA-train for precision measurement of sub 10-nm aerosol dynamics" *by* Dominik Stolzenburg et al.

Anonymous Referee #2

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This manuscript describes a system comprised of a set of six DMAs operated at ~fixed voltage and connected to independent CPCs. The potential advantage of this sort of configuration over a traditional instrument in which a single DMA/CPC is used with stepped or scanned voltage to span a desired particle size range is the improved time resolution that may permit characterization of rapidly evolving aerosols. Careful consideration was given to the component selection and construction of the system. And considerable effort was invested in calibrating and characterizing the DMAs and CPCs. Just that description of the component calibrations alone might justify publication.

The writing would have to be cleaned up a bit before publication, but the paper is certainly readable and understandable in its current form.

My primary concern with the manuscript is simply that for atmospheric research the

time resolution that can be gained from this approach will very rarely justify the expense. Perhaps there are applications in industry for which rapid < 10 nm size distribution changes are common and important. But they aren't identified in the manuscript and the provided justification that this is needed to characterize nucleation mode particles just doesn't seem to be supported by the presented data. The primary example given is of a rapidly evolving size distribution of the aerosol emitted during warm-up of a tungsten oxide generator. But even then the presentation in the paper seems biased to suggest a greater loss of information without parallel measurements. Below I summarize some of my specific concerns. These would need to be addressed before I could recommend publication.

Section 4.1: The manuscript describes measurement of the aerosol emitted during warm up of a tungsten oxide generator. Towards the end of the section the authors state "No details of the warm-up procedure could therefore be resolved with the classical system." Even with the datasets used in the comparison this is an overstatement. Linear interpolation between the size distribution measurements wouldn't capture all of the evolution, but it would certainly do better than provide "no details". Furthermore, the comparison seems designed to provide a large contrast between the two. Specifically, i) a DMPS system was used, rather than a faster SMPS system, and ii) the size range spanned by the DMPS extended out to ~15 nm while that by the DMA-train extended to only 7 nm. If the argument is that only with a DMA-train can these dynamics be captured then the authors should compare with an SMPS scanning up to 7 nm and with the scan time minimized to the extent possible.

Section 4.2: Unlike the rapid evolution of the generated aerosol, the nucleation mode aerosol sampled in the CLOUD chamber grows rather slowly. The manuscript again argues that an SMPS can't provide the needed time resolution, but this needs to be defended with data. From the text description and Figure 10 it seems the particle growth rate for this example was around 10 nm/hr, which is quite fast. But even then, that is just 0.17 nm/min or about 0.25 nm/DMPS measurement. That sort of change

between measurements should be easily resolved and quantified. I appreciate that low count rate may limit the DMPS stepping or SMPS scanning rate. But this needs to be defended using data and not just asserted to be significant.

Minor issues and comments

Abstract, line 5: Change A 10 to A10

Page 1, line 17: Add "," after (Albrecht, 1989)

Page 2, line 1: Replace "get lost to bigger pre-existing aerosol" with "coagulate with bigger pre-existing particles"

Page 2, line 21: "sufficiently fast" is arbitrary without some reference to the measurement or conditions.

Page 2, line 23: re-word "allows to exploit"

Page 3, line 8: "allows" suggests an option and not necessarily that it was done.

Page 3, line 10: Why does the approach follow an SMPS and not either a DMPS or SMPS?

Page 3, line 29: "state-of-the-art" is subjective. Replace with "modern".

Page 4, line 1: Contrasting the butanol- and water-based CPCs to infer composition is mentioned but never demonstrated (also in the conclusions).

Page 8, line 6: Related to the above comment, the statement that the inferred particle concentrations should not be too dependent on size-dependent counting efficiency also implies that the dependence on working fluid (water or butanol) is also likely pretty modest.

Page 9, line 4: Is the influence of multiply charged particles really relevant for this size range? To keep this statement it should be supported by a back-of-the-envelope estimate of the error introduced by not having an instrument to measure the >10 nm

particles.

Page 9, line 5: The presence of >100 nm particles is irrelevant because they would not show up in the <10 nm size range even if they were multiply charged (with reason). And the bipolar charging probability of <10 nm particles is irrelevant because they are not the particles that would contribute to the signal only because of multiple charges.

Page 11, line 8: "Perfectly adjusted" is subjective.

Page 11, line 10: What does "using the full counting statistics" mean?

Page 11, line 11: Lowering the sensitivity is not a good thing as implied here.

Figure 7: The < 1.0 plateau for the PSM is mentioned in the text, but not explained.

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