Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2022-188

Anonymous Referee #3

Referee comment on "A multiple-charging correction algorithm for a broad-supersaturation scanning cloud condensation nuclei (BS2-CCN) system" by Najin Kim et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2022-188-RC2, 2022

To support the formatting of equations, Specific Comments & Technical Corrections are available in the supplied PDF.

General Comments

This article presents a new algorithm for processing measurements from a Broad Supersaturation Scanning Cloud Condensation Nuclei (BS2) system. The outcome of this algorithm is to improve retrieval of aerosol hygroscopicity parameters, namely the particle hygroscopicity, κ. The article claims that this algorithm provides a unique solution to a known problem in determining κ: namely, that multiply-charged particles that pass through the differential mobility analyzer (DMA) in a BS2 system result in misshapen particle activation curves which degrade the retrieval of κ. Despite the claim of novelty, the algorithm bears a rather close resemblance to the proposed methodology of Moore et al (2010).

In general, the Methods section is missing sufficient detail for their method to be utilized and reproduced by other researchers. Some issues are purely technical: the authors need to re-work the notation of the Methods section. There are several instances where the notation is not appropriate, misleading, or definitions are missing altogether. Other issues are pragmatic: further descriptions of their BS2 system should be included (rather than referenced) such as impactor size, DMA size detection range, etc. This is, essentially, a methods paper and the Methods section is perhaps the weakest point of the current manuscript. It should be spelled out to the letter what a researcher needs to do to implement this method.

The results demonstrate a fulfillment of the original promise. The appearance of multiply charged particles in the activation curve have disappeared. However, the assessment of this methodology is fairly qualitative. The case study approach is not sufficient enough argument for researchers to understand when this correction needs to be applied to their measurements. It is clear, for example, that the correction algorithm need not be applied to calibration experiments. A revision of this manuscript should include a more quantitative laboratory-based study with ammonium sulfate rather than the qualitative field-based study that is currently used. The revision should also include a full uncertainty analysis to determine confidence intervals on derived hygroscopicity. This would allow other researchers to better understand when they should apply this correction and the magnitude of the effect on hygroscopicity retrieval (so that they can troubleshoot their implementation). The authors should also make it more apparent in the abstract and conclusions that the proposed algorithm assumes that the particle size distribution is monomodal. The algorithm has not been tested for more complicated PSDs.

Finally, the authors should support their claim that their methodology is a necessary improvement on previous approaches. A revision of this manuscript should also include a side-by-side comparison of this methodology to previously proposed methods in the literature, e.g. Moore, Nenes & Medina (2010) to which the proposed method bears an uncanny resemblance.

Please also note the supplement to this comment:
https://amt.copernicus.org/preprints/amt-2022-188/amt-2022-188-RC2-supplement.pdf