Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-258

Anonymous Referee #2

Referee comment on "Cloud condensation nuclei (CCN) activity analysis of low-hygroscopicity aerosols using the aerodynamic aerosol classifier (AAC)" by Kanishk Gohil and Akua A. Asa-Awuku, Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-258-RC1, 2021

Review of Gohil and Asa-Awuku, AMT 2021

GENERAL COMMENTS

This study describes the use of a relatively new system to size-select particles known as the Aerodynamic Aerosol Classifier (AAC) manufactured by Cambustion to measure cloud condensation nuclei (CCN) activity of aerosols. This combination of AAC + CCN counter has not yet been characterized in the literature and the study uses a single model compound, sucrose, to verify the theoretical calculations and uncertainties calculated from the transfer function of the AAC. This work offers a nice extension of the work by Moore et al. for the differential mobility analyzer + CCN counter system and should provide a useful tool for future users as the system becomes more popular. As part of the study, a Python package is available through GitHub for the activation diameter and uncertainty calculations. Although I checked that it was available, I did not run the package myself and cannot comment on its capabilities. In addition, I was only able to follow the equations at a high level and have some specific comments listed below.

I see four minor issues associated with this study. The foremost is the use of the aerodynamic diameter to initially calculate the hygroscopicity parameter. The main reason that the diameter is in the original Kohler equation is to determine the number of soluble moles in the particle. It would therefore make sense to present the calculations using the volume-equivalent diameter first, especially since the point of this study is to demonstrate the usefulness of the AAC and to present the results in the best possible light. I could see keeping the discussion about the use of aerodynamic diameter when the particle density and shape factor are unknown since this will be the case for some applications. I realize that this point is entirely stylistic and I leave it to the editor and the authors to determine whether this suggestion should be implemented. Related to this point, the tone of the article would be improved if lines 328-330 of the conclusions were reframed to say that it is important to use the volume-equivalent diameter when possible to calculate kappa,

since that is more representative of the terms in the Kohler equation, instead of framing it around the aerodynamic diameter.

A more important issue is the relative uncertainties presented in equations 12, 15 and 16. Errors are never subtracted, otherwise a large uncertainty in the terms in the denominator could reduce the overall relative uncertainties, which is unreasonable. There should be an absolute value around the terms, leading to the relative errors being summed (i.e. all the subtraction signs should be changed to addition). Can the authors explain why they are adding the relative errors instead of adding them in quadrature? Once corrected, the new uncertainties should be propagated through the rest of the manuscript in the revised version.

Another concern is that the uncertainty in the calibrated supersaturation of the CCN counter is not included in the overall uncertainty calculations. The authors should include a discussion of the uncertainties in the diameters measured by the DMA used in the calibrations, the uncertainty of the fitted critical diameter (as shown in the Supplement), and their effect on the calibrated supersaturation. This should then be included in the uncertainty in kappa.

A final concern is that the final uncertainty for the measured kappa value presented on line 290 is 0.006. Was this calculated from Equation 16? This value is comparable to the standard deviation of the kappas presented in Table 1 (0.007) and suggests that the repeatability of the measurement, over a range of supersaturations, is worse than the instrumental uncertainties and all the analysis presented. Was this true of repeated measurements at the same supersaturation? Please provide some perspective on this.

SPECIFIC COMMENTS

Equation 13 - Can the authors provide some background about the origins of this equation?

Line 226 - Dp50 in Equation 16 refers specifically to the volume equivalent diameter. The other diameters should not be used.

TECHNICAL COMMENTS

Fig. 4 caption – “The measurements WERE performed…”