The authors have addressed most of my comments and the manuscript has improved. However, a few major concerns remain.

General comments

1. The authors responded to my comment.
   For Part B, I suggest that this response regarding to what extent the artifacts investigated here can explain the discrepancies in the kappa in the literature should be also incorporated into the revised manuscript.
2. Regarding my formed general comment 2:
   a) I think that the assumption of the "volume-weighted approach accepted as a standard convention of kappa theory" is that different components are internally mixed on particles! Particles at different sizes are apparently not internally mixed. Taking an extreme example, if an aerosol population consists of black carbon particles for all particles <100 nm and $(NH_4)_2SO_4$ particles for all particle >100 nm. If one measure D_{50} at 0.1% one would get a D_{50} of ~140 nm since the D_{50} of $(NH_4)_2SO_4$ is ~140 nm. And from that D_{50} vs. SS, one would get a kappa of ~0.6 (the kappa value of $(NH_4)_2SO_4$). Of course, the D_{50} vs. SS data sets obtained in this case will not fall on the lines of constant kappa.
   b) This again leads to my concern about the approach used to derive artifacts from DMA in this study because it is different from the real CCN measurement. To clarify my comment, in the real CCN measurement, at first activated fractions of particles are obtained, either activated fraction vs. supersaturation(SS) for particles of a given size or activated fraction vs. particle size at a given SS. The activation curve is then fitted to derive a D_{50} or SS_{50} and from D_{50} vs. SS or D vs. SS_{50}, kappa is obtained. In order to investigate the uncertainties of kappa due to instrument artifacts, one would need simulate the data acquisition process of CCN activation measurement by simulating the number of particles and number of activated particles in each size bin (in the case of D_{50} vs. SS) and then activation fraction and D_{50}. I am not sure whether the artifacts in this study can reflect the real uncertainties in CCN measurement and is useful to get an idea of the uncertainties in CCN measurement.
3. Regarding my formed general comment 3:
   I think one would like to see a more quantitative analysis of the uncertainties of kappa after taking the influence of instrument calibration by a standard compound into account. This is most relevant to real CCN measurement and is most interesting to those who do the measurement and who use these data.
   I also suggest that the discussion should be somewhat included in the conclusion because if after the calibration using $(NH_4)_2SO_4$ or NaCl, the discrepancy caused by instrument artifacts would be much smaller than values shown in the manuscript.

Technical comments

1. In figure 3a, in the lognormal distribution it is dN/dlogDp that follows the shape of the curve rather than N (number concentration, y-axis) or dN/dDp.