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Atmospheric Measurement Techniques Discussions

Interactive comment on "Analysis of cloud condensation nuclei composition and growth kinetics using a pumped counterflow virtual impactor and aerosol mass spectrometer" *by* J. G. Slowik et al.

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This paper describes a novel new approach that combines three tools to study the dependence of cloud particle activation on particle size and chemical composition. A flow of ambient particles are passed through a thermal-gradient diffusion chamber (TGDC), within which some particles (CCN) become activated into cloud particles. Cloud particles larger than a few microns are then separated from the flow by a pumped counterflow virtual impactor (PCVI), dried in the stream of nitrogen, and chemically resolved by an Aerodyne time-of-flight mass spectrometer (AMS). The paper mainly presents

and discusses the first results obtained in urban and rural settings. It is well written and will be an important contribution to the scientific literature. I have several relatively minor, but important (and related) points that I would like the authors to address in a revised paper.

First, as the authors know, the extent to which the AMS undersamples particles depends on the state (e.g., phase, size, composition) of the particles, and can be as much as 50% (some have reported even lower for some particles). A number of recent studies have examined the issue of the collection efficiency (CE, or 'bounce') and, while there is still much to understand, clear trends are beginning to emerge. It is important for this paper, the first to describe the use of the AMS in a new application that involves the processing of dissolvable materials, to describe how the observations might be affected by changes in CE due to (1) differences in the relative humidity conditions between the two sample modes ("CCN-active" and "polydisperse") and (2) potential alteration in the physicochemical state of the particles. This is an issue that cannot easily be resolved in one study, but being that this is the first of its kind with the promise of many more to follow, it would be very helpful for the authors to include a new section before the conclusions that discusses the potential complications and limitations of the method that relies on the AMS. With respect to (1), it is important to note whether or not the airstream was dried prior to sampling with the AMS in polydisperse mode. If not, it is useful here to speculate on the potential introduction of biases in the results. With respect to (2), it would be useful to discuss how a particle formed by deliquescence=>activation=>evaporation=>efflorescence may be in a different physical and chemical state, and thus have a different CE, than that of the original particle. Finally, this section would provide a good opportunity to speculate on how ancillary measurements (e.g., SMPS, UHSAS, etc.) might help to constrain this issue. Alternatively, if the authors have other information that they bring to bear on this issue (e.g., SMPS measurements) it would be good to present those here.

Second, it would be very useful to try to place some quantifiable bounds on systematic

errors that may arise from potential variations in CE noted above. It appears as though the uncertainties in Tables 1 and 2 are standard deviations only. Would you expect systematic errors to be larger, smaller, or similar to these errors? Can they be neglected, or are they potentially larger than the variations you observed in the data?

Minor technical issues:

Does it make sense to provide a reference to work by K. Noone on the CVI technique?

Page 289, line 24. "...the atmosphere..."

Page 291. Can you describe in a bit more detail how you know that transmission is only 50%?

Page 291, line 22. "...particles were produced by atomization..."

Page 291, lines 22-23. This was meant to be ammonium sulfate, correct? Not (NH4)2-SO4 or NH4NO3.

Page 292, line 6. "4" should be a subscript.

Page 292, lines 9-10. Assuming the flows are at STP, shouldn't the counterflow be in slight excess over the sum of the pump and sample flows? Otherwise, you will be sampling some ambient air?

Figures 2, 5, and 6. The traces would be easier to see (and colors easier to distinguish) if they were similar in thickness to those in Figures 3 and 4.

Page 293, lines 22-25, Page 294 lines 2-6. A style comment - you can probably condense this or delete some text given that you talk about specific figures in the individual sections on the case studies that immediately follow, and you don't necessarily need to note that the figures are coming here.

Page 294. You may want to comment somewhere here about the lack of time for smaller particles to be scavenged by the cloud particles.

Page 295, line 7. "...the detection limit..."

Page 295 and 297. It would be good to report the uncertainties in the quantities listed in the text so that the reader doesn't have to keep referring back to the table.

Page 296, line 14. "Washington"...more specifically? Nearby town, part of state, etc. would be helpful.

Page 299, line 14. "...to activate..."

Page 302, line 18. "...composition..."

IPCC and Solomon references are redundant.

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 285, 2011.