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Atmospheric Measurement Techniques Discussions

Interactive comment on "A technique for the measurement of organic aerosol hygroscopicity, oxidation level, and volatility distributions" *by* Kerrigan P. Cain and Spyros N. Pandis

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(1) *This manuscript describes an instrument configuration used to measure the volatility dependent composition and CCN activity of an aerosol. The experimental approach was used to measure a well-characterized inorganic aerosol and a well-studied secondary organic aerosol produced from ozonolysis of alpha-pinene. The measurements are useful and the description would be of interest for some readers of this journal. But while there is nothing wrong with the manuscript, it describes a rather straightforward configuration of components used by many researchers and, thus, is not especially novel. To me, the preliminary results are the most interesting element of the*

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Discussion paper

manuscript. But I wonder whether it would make more sense to present them in a separate paper focused on the interpretation of the data and not the experimental technique. Nevertheless, the manuscript could be suitable for publication after the points below are addressed. The text is easily understood but would benefit from additional editing.

We thank the referee for the helpful comments and suggestions. While we do agree that the experimental setup combines techniques that have been used in the past, we are proposing a new way to analyze the corresponding data and then synthesize them following the 2D-VBS framework. Our hope is that the same experimental method combined with the new analysis technique will provide valuable insights into these properties and their relationships. We do agree that the first results are quite interesting and this is the reason that we prefer to combine the presentation of the experimental technique, the data analysis method, and the results of this pilot study to demonstrate the utility of the proposed approach.

(2) *The observation that O:C and hygroscopicity decreased for the least volatile particles is certainly interesting. The authors provide a plausible explanation for the unexpected pattern. But further explanation of the experimental technique is required. Specifically, the time required for the full measurement sequence and the order of the TD temperatures should be provided. The rationale for this is that it seems possible that the aerosol continued to evolve during the measurements such that what is sampled when the TD is set at 100 C differs from that when it is at 150 C.*

We have added the requested details about the experimental technique. In short, the aerosol is passed through the TD for 7-10 SMPS scans (16-23 minutes). Then, the aerosol is sent through the by-pass line until the TD is at the next set point (usually 7-10 SMPS scans). Once the TD is at the desired new temperature, aerosol is sent through the TD for the same number of SMPS scans and this process is repeated until measurements in all TD temperatures have been performed. Depending on the

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number of temperature set points, a full sequence can take anywhere from 2.5-3.5 hours. In our experiments the temperature has been increasing during the TD scan. The potential evolution of the SOA during these measurements can be checked by comparing the AMS spectra to the original one in the beginning of the scan. The measurements can also be repeated by performing a second TD scan and collecting a second thermogram. In our experiment the change of the SOA spectra during the experiment was minimal (less than 5 degrees) in all cases. This information has been added to the revised paper.

(3) *Page 6, line 22: Rather than just stating that the loss rate was determined it should be reported. A large correction for a high loss rate could significantly increase uncertainty in the measured MFR.*

This is a good point. We have included the corresponding information about the TD loss rate constant as a function of temperature and particle size in the Supplement.

Minor points

(4) *Page 2, line 19: ". . .is reasonable CCN material using supersaturated conditions" should be re-worded.*

We have removed "using supersaturated conditions" from this sentence to avoid confusion. In the following sentence we added that all of the studies cited there performed measurements in supersaturated conditions.

(5) *Page 4, line 23: Why "proposed"?*

We included the word proposed because we are proposing a method to measure and relate these three properties. To avoid confusion, we have removed this word.

(6) *Page 5, line 11: What is a "large response"?*

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We have removed this relative term and rewrote the sentence to state that we aligned the size distributions using the minimum that occurs between the DMA upscan and downscan.

(7) *Page 5, line 13: If this level of detail about the analysis of the data is going to be provided then the approach to inverting the SMPS-CPC and SMPS-CCNC distributions should be included.*

We have rewritten this section to include more detail regarding the inversion technique and included a reference to the study that developed this method.

(8) *Page 6, line 21: Use metric or change in to inch or to in. to clarify.*

Done.

(9) *Page 7, line 11: ". . .behave as non-volatile" should be re-worded.*

We have rewritten this sentence.

(10) *Page 7, line 12: Replace "extremely" with something like "very".*

Done.

(11) *Page 7, line 16: This final sentence repeats what was already explained.*

We have removed this sentence to avoid repetition.

(12) *Page 8, line 24: I appreciate what you are trying to explain here, but as written the first and second parts of the sentence seem contradictory.*

We have removed the beginning of this sentence and added the rest to the end of the previous sentence to avoid repetition and confusion.

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(13) *Page 9, line 18: ". . .conventional thinking, which assumes" should be re-worded.*

Done.

(14) *Page 9, line 24: Change to something like "Similar to the pattern observed with the activation diameter. . ."*

We have made the recommended change.

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