

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

Anonymous Referee #1

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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 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 tech-

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nique. 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.

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.

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.

Minor points: Page 2, line 19: “. . .is reasonable CCN material using supersaturated conditions” should be re-worded.

Page 4, line 23: Why “proposed”?

Page 5, line 11: What is a “large response”?

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.

Page 6, line 21: Use metric or change in to inch or to in. to clarify.

Page 7, line 11: “. . .behave as non-volatile” should be re-worded.

Page 7, line 12: Replace “extremely” with something like “very”.

Page 7, line 16: This final sentence repeats what was already explained.

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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.

Page 9, line 18: “. . .conventional thinking, which assumes” should be re-worded.

Page 9, line 24: Change to something like “Similar to the pattern observed with the activation diameter. . .”

Interactive comment on Atmos. Meas. Tech. Discuss., doi:10.5194/amt-2017-213, 2017.