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## ***Interactive comment on “CCN activity of organic aerosols observed downwind of urban emissions during CARES” by F. Mei et al.***

**Anonymous Referee #2**

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### General Comments

This paper presents an analysis of the CCN activity of organic aerosols during the CARES field study in 2010. The paper is not ground-breaking, but it does include some interesting findings concerning the relative value of f<sub>44</sub> versus O:C ratio as a predictor of the hygroscopicity of the organic aerosol component (section 5.4). This core finding is worthy of publication, but in its current form the manuscript has major issues that must be addressed before publication.

One major concern with this present manuscript is its striking similarity with a recently-published paper in JGR-Atmospheres involving some of the same authors (Mei et al., 2013). There are several paragraph-length passages that are virtually identical be-

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tween the two papers, including (but not limited to) the first paragraph of section 1, all of sections 2.2 and 2.3, all of section 4, and parts of section 5.3. Figure 1 in this paper is identical to Figure 1 in the JGR paper, and Figure 3 here is functionally identical to Figure 4 in the JGR paper (i.e., identical ideas are communicated with different data). Figures 2, 4, and 5 in the ACPD paper also have very close analogues in the earlier JGR paper. The supplemental information is also virtually identical to material in the JGR paper and its supplement. It should be noted that some but not all of the authors are common to the two papers, and that the copyright to the JGR paper is held by the American Geophysical Union.

Some similarity between the ACPD and JGR papers might be expected, since they present similar analyses with similar instrumentation from two field studies closely spaced in time. In this case though, the documents overlap to an unacceptable degree. The separate scientific contributions of the two papers must be distinguished more clearly to merit separate publication. Given that the JGR paper has already passed peer review and is available to the public, sections 2 and 4 of this paper should be greatly reduced and readers should instead be referred to the appropriate sections of the existing paper. The supplemental material should be removed completely, as well as Figures 1 and 3. Section 1 should probably be rewritten to reduce redundancy and avoid copyright complications.

In addition to the above major critique, it also seems that the analysis in Section 3 (including Figure 2) has relatively limited value to the overall goals of the paper. The motivation to include the section seems to be to provide a foundation for the fuller data analysis later in the paper- an overview, as the section title suggests. If that is the primary intended goal then there should also be some discussion of the day-to-day variability in addition to the diurnal variation. Either time series plots or error bars on the mass concentration and size distribution plots would be good additions.

Specific Comments

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[Interactive  
Comment](#)

Page 9357, Lines 20-23: ‘Essentially identical’ is too broad here. Presumably the authors of the later studies saw their work as improvements over what existed previously—are these improvements trivial? If you are making that assertion, then it must be supported.

Page 9358, Lines 21-22: This line asserts that measurements of  $\kappa_{org}$  for ambient organics are scarce, and no previous studies are cited. There are several previous studies looked at this same parameter for ambient conditions, and these should be mentioned. A very quick search revealed recent papers by Latham et al. (2013) and Moore et al. (2011). The lack of citations combined with the phrasing of the sentence may lead to the impression that the present analysis is more isolated than it is.

Page 9361, Lines 6-16. The text suggests that the CCN counter was stepped through the six temperature gradients in order, so that each supersaturation was measured every four hours. However, Figure S1(b) suggests a sawtooth pattern. Under the pattern suggested by Figure S1, the 4.5 C temperature gradient would only be measured once every eight hours (approximately). Please clarify this, and if the latter, please add text discussing the implications of this approach in your analysis.

Page 9364, Lines 13-17: Isn’t this an estimation rather than a derived solution? Why would a derived property be valid only for  $\kappa > 0.1$ ? What is the uncertainty associated with using equation (2) rather than the analytical solution?

Page 9365, Lines 16-21: What advantage is gained by using two different methods to fit the curve describing the characteristic critical supersaturation? Neither this paper nor the earlier paper in JGR indicates why the second lognormal fit is sometimes superior, or whether the difference is sufficient to justify the added effort.

Page 9365, Lines 24-27: The derivations of  $\sigma_{\kappa}$  and  $\bar{\kappa}$  are not provided in the Supplement to the ACPD paper. The readers should be referred here to the JGR paper.

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Page 9372, Lines 7-9: What is difference between a  $\sim 100\%$  increase and a factor of 2 increase?

Page 9372, Lines 25-29: This is an interesting point.

Page 9373, Lines 5-14: Are these periods evenly distributed through the study period. Are they distributed evenly diurnally? What fraction of the total data set do they represent? Knowing this would better place the results that follow in the context of the overall conditions at the site.

Page 9374, Line 21 – Page 9375, Line 7: The approach for relating size-resolved kappa<sub>org</sub> to size-resolved O:C ratio seems somewhat faulty. Size-resolved O:C as described here is not a measured value, but rather a parameterized value based on size-resolved f<sub>44</sub> and the bulk f<sub>44</sub>/O:C ratio. This approach implies an assumption the f<sub>44</sub>/O:C ratio is size-independent. This seems to be an ambitious assumption given that the point of the analysis is to evaluate how f<sub>44</sub> and O:C ratio might vary relative to each other.

#### Minor Revisions

Page 9357, Line 2: 'Also' should be deleted.

Page 9357, Line 12: This sentence should be attached either to the preceding paragraph or the following one.

Page 9358, Line 1: 'Is' should read 'can be'. This sentence is also slightly awkwardly worded.

Page 9358, Lines 16-17: Why no references after the clause about SOA?

Page 9358, Line 24: Needs a space after the first kappa.

Page 9359, Line 21: 'Site' should be plural.

Page 9360, Line 17: 'Spectrum' does not need to be capitalized here.

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Interactive Discussion

Discussion Paper



Page 9361, Line 5: 'CPC3771' should just read 'CPC'.

Page 9364, Line 16: Delete 'and was'.

Page 9367, Line 18: 'Combing' should be 'combining', probably.

Figure 5 caption: 'Expect for the outlines' should read 'except for the outliers'.

Supplement, Figure S1: The resolution on this figure is poor. Can it be improved?

Supplement, Page 1, Line 9: This is listed as Mei et al., 2013 in the main paper.

## References

Lathem, T. L., Beyersdorf, A. J., Thornhill, K. L., Winstead, E. L., Cubison, M. J., Hecobian, A., Jimenez, J. L., Weber, R. J., Anderson, B. E. and Nenes, A.: Analysis of CCN activity of Arctic aerosol and Canadian biomass burning during summer 2008, *Atmospheric Chem. Phys.*, 13(5), 2735–2756, doi:10.5194/acp-13-2735-2013, 2013.

Mei, F., Hayes, P. L., Ortega, A., Taylor, J. W., Allan, J. D., Gilman, J., Kuster, W., de Gouw, J., Jimenez, J. L. and Wang, J.: Droplet activation properties of organic aerosols observed at an urban site during CalNex-LA, *J. Geophys. Res. Atmospheres*, 118(7), 2903–2917, doi:10.1002/jgrd.50285, 2013.

Moore, R. H., Bahreini, R., Brock, C. A., Froyd, K. D., Cozic, J., Holloway, J. S., Middlebrook, A. M., Murphy, D. M. and Nenes, A.: Hygroscopicity and composition of Alaskan Arctic CCN during April 2008, *Atmospheric Chem. Phys.*, 11(22), 11807–11825, doi:10.5194/acp-11-11807-2011, 2011.

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