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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Mixing state and compositional effects on CCN activity and droplet growth kinetics of size-resolved CCN in an urban environment" *by* L. T. Padró et al.

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General Comments:

This paper presents analysis of an Atlanta-area field study regarding the effects of mixing state and composition on a CCN closure study. The authors show that both mixing state and chemical composition were necessary to achieve the best CCN closure. Several predictive scenarios were compared against the actual data to confirm the relative closure obtained. A further conclusion is that growth kinetics do not appear to be a concern for this particular study (most samples studied experienced growth similar to ammonium sulfate). The paper

is well-written and concise however details on how chemical composition was incorporated into the model scenarios appear to be left out or are unclear.

We thank the reviewer for the positive and thorough feedback. Our responses to the issues raised are provided below in italics.

Specific Comments:

The discussion of "Period A" and "Period B" is unclear and appears disconnected to the data presented in the Figures. Do a) and b) of Figure 1 correspond to A and B Periods in Figure 2? The conclusions observed on page 32729 lines 2-11 do not appear to match the data presented in Figure 2 as claimed.

We will ensure that Figure 1 will agree with text and other figures which reference the different air masses.

The section on Chemical Composition is very short. It appears to take into consideration the various ions detected but the only connection with chemistry and CCN activity mentioned is solely related to WSOC. To what degree of size-dependency was chemical composition measured and exactly how were those measurements incorporated into the size-dependent model scenario (just water solubility or were actual ions somehow incorporated)? This should be clarified in the final paper before publication.

The compositional data (inorganic ions and WSOC) collected for this study was an average over all particles sizes. The inorganic salts present in the dry aerosol were determined using the sulfate molar ratio (discussed in Section 3.3, page 32737, lines 2 -7). We have expanded the discussion to clarify these points.

Equations (12) and (13) appear to differ by the absolute value but this is unclear due to lack of parenthesis in equation (13) – is it the (sum of Pi) minus Oi or the sum of (Pi-Oi)? This is further confusing in Tables 1 and 2 where the difference between mean error and mean bias is only an issue of sign (in which case it

seems unnecessary to have both columns) for over half of the data but the other half of the data is very slightly different between Error and Bias. The authors should check these numbers and clarify the issue and equations as needed.

Equation (13) is indeed missing some parenthesis. This is now corrected in the final manuscript. This typo does not affect the numbers in both tables.

The conclusion reached on P32739 L15-17 indicates INT-SALTS achieved best closure while this seems to be the opposite of what the paper is trying to conclude. The entire section from P32739 Lines 5-26 and onto the next page is confusing and possibly contains errors (or misleading statements like the one above) which should be clarified before final publication.

The discussion was referring to calculation of d_{pc}*, where INT-SALTS provides the best agreement. However, our overall best CCN closure was achieved for the size-resolved external mixture scenario (S = 1.10 in Table 2) which is consistent with what we conclude.*

Technical Corrections:

P32756 L6 – please define "lattice effects"

Done.

P32729 L14 – introduce (PILS) after defining it the first time

Done

P32739 L10 – this sentence needs to be corrected

The sentence will be corrected. In the final manuscript the sentence will be written as follows:

"For all scenarios considered, theory predicts that a smaller activation diameter, $d_{p,c,}$*is needed than the one observed."*

Figure 1 – This figure is in error (possibly in multiple ways). The arrows on the trajectories appear to be going in the wrong direction (away from Atlanta instead of towards it). Also, is (a) and (b) supposed to represent Period A and Period B? If so, clarify this, if not, they should; but the main concern with the figure is that (b) does not represent what is stated in the caption, instead it represents the second type of Period A (coming from the ocean instead of the Gulf of as shown in (a)). Period B is not depicted anywhere.

We will ensure that Figure 1 will agree with text and other figures which reference the different air masses.

Figure 2 – see the first comment in "Specific Comments" If the intent is to show a difference between Period A and Period B as claimed, please highlight or otherwise make this point clear. An additional Figure or statistical analysis comparing Period A and Period B seems needed here.

Indeed our intent is to contrast the total condensation nuclei (CN) and total cloud condensation nuclei (CCN) between air masses. This is highlighted and mentioned in page 32729 lines 9-11. The mean concentrations of CN and CCN in both periods are statistically different (p < 0.001 and F » 1).

Figure 7 – the red dots in (a) seem too few and are perhaps hidden by the other dots. Consider making the dots smaller or otherwise more visible for all data points.

Good point! We will augment the graph to make things clearer

Figure 11 – there appears to be no difference in data between Period A and Period B as I infer from the captions.

Indeed, there is little difference between the fit parameters across different air masses, but the total concentration changes. This is discussed further in Section 4.2. The figure is shown to show the range of the different fit parameters during the study period.

Figure 13 – the caption claims 10th-90th percentiles capture 90% of the data when it is actually 80%.

Thank you for pointing this out! This will be corrected in the final manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 32723, 2011.