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Interactive comment on “Measured and modelled Cloud Condensation Nuclei (CCN) concentration in São Paulo, Brazil: the importance of aerosol size-resolved chemical composition on CCN concentration prediction” by G. P. Almeida et al.

Anonymous Referee #1

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Major

This manuscript, as I see it, sets out to discuss two points; one of which is done better than the other. Firstly, the manuscript addresses measured and modelled CCN concentrations in Sao Paulo, Brazil. Secondly, the manuscript (as titled) stresses the importance of size-resolved chemical composition on the prediction of CCN concentrations. The first aspect is addressed quite nicely, and it no doubt of value to

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ACP and the scientific community at large. The second aspect is demonstrated neatly through the use of size-resolved chemical composition data, but I feel that a slightly different approach would have been more valuable, for the following reasons.

As stated in the manuscript a particle's CCN affinity is dependent on its size and composition. It is therefore a shame that the CCN measurements were not performed downstream of a DMA. It can be argued that size-resolved measurements are the best way of trying to measure the impact of a changing aerosol size distribution on CCN activity. Indeed, the authors acknowledge that size-resolved measurements of particle constituent matter via AMS measurements are beneficial in improving CCN closure studies. Though more complex, size resolved measurements of CCN activity will reveal the activated fraction for a given dry diameter - paired with size-resolved chemical composition; a valuable tool. Though it is widely accepted that chemical composition plays a larger role than particle size, this is only true within certain limits. These limits are best probed with size-resolved measurements of CCN activity. It is my hope that the authors consider this in future works. Some discussion related to this issue would be useful. Larger assumptions and consequently, uncertainties, are associated with the type of bulk measurements presented in this study (though no instrumental errors are propagated nor discussed in detail).

Though size-resolved CCN measurements should possibly have been used, the data seems to be of high quality, and it reasonably presented.

The English throughout the paper needs to be carefully checked, as the numerous mistakes disrupted the flow from the otherwise nicely set-out paper. I have noted a few (not all) below in the "Misc" section.

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Minor

Page 32356, Line 26; What is “relative accuracy” in this context? It would be useful to include examples of studies where CCN concentrations were *not* predicted to within instrumental errors, or “relative accuracy”, to give the reader some perspective on this difficult measurement and assumptions therein.

Page 32359, Line 25; It’s confusing to use S and then SS for supersaturation in consecutive sentences. Being as it is always the supersaturation with respect to water vapour that is considered, I recommend choosing one and then remain consistent in the text.

Page 32360, Line 1; Not all particles are activated within the column, and not all will grow to supermicron sizes. This is even confirmed in the following sentence, whereby it is stated that the CCN counter identifies particles larger than $0.75\mu\text{m}$ in diameter as being CCN. Consider revising this sentence for consistency.

Page 32360, Line 5; ratio should be written as 10:1

Page 32360, Line 6; I do not fully understand what “Data reported here include only under supersaturation steady state” exactly means. Furthermore, after changing supersaturation (e.g. from 1

Page 32360, Line7; I have concerns regarding “the factory calibration. . . was used”. How was the instrument verified to be working correctly? In general, it is good scientific practice to perform one’s own calibrations, especially in the field under varying conditions. A calibration using inorganics before and after the project would

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have been optimal, and would have verified the instrument performance.

Page 32360, Line 20; In what sense were the DMPS data “corrected”? Is this “corrected” data used for the subsequent integrated number calculations? (the latter is not clear)

Page 32364, Line 23; Only “Köhler, 1936” should be used here. The authors later present the simplified Köhler equation (eq. 1), which was described by McFiggans et al., 2006.

Page 32365, Line 6; It should be stated that this is the surface tension of water.

Misc

Page 32355, Line 9; “internal mixture” should be “internal mixtures” or “internal mixing”

Page 32355, Line 13; do the authors mean “with a dominant nucleation mode”?

Page 32355, Line 21; I think it would be helpful to say in what regard (i.e. compared to what) the CCN results were overpredicted (e.g. from Köhler model)

Page 32356, Line 2; “on a site” should be “at a site”

Page 32356, Line 5; “enables” should be “enable”

Page 32356, Line 8; delete “distribution”

Page 32356, Line 13; “its sizes” should be “their size”

Page 32357, Line 9; “an internally” should be “internally”

Page 32357, Line 10; “the identical” should be “an identical”

Page 32357, Line 21; there should be a comma after “processing”

Page 32359, Line 19; following section? following text?

Page 32366, Line 13; “Initially, the κ size independent solubility value” would read

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better as “Initially, the size-independent solubility value, κ ,”
Page 32367, Line 9; “for the assumptions of size averaged” is not clear
Page 32370, Line 8; “chemistry composition” should be “chemical composition”

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 32353, 2013.

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