

Interactive
Comment

***Interactive comment on* “The influence of chemical composition and mixing state of Los Angeles urban aerosol on CCN number and cloud properties” by M. J. Cubison et al.**

Anonymous Referee #2

Received and published: 8 May 2008

The authors examine roles of chemical composition and mixing state to CCN number concentrations in urban Los Angeles. They compare measured CCN concentrations with modeled concentrations with different complexity of the mixing state and hygroscopicity in the model. Their main result is that fresh particle emissions should be considered explicitly as non-activating. In addition, the relative number concentration of the particles from fresh emission sources, inorganics and oxygenated organics with limited hygroscopicity determine the CCN number concentrations. Furthermore, the authors explore the effects of mixing state to predicted CCN number concentrations and cloud properties with an adiabatic cloud model.

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The paper is well written and tells an nice story how adding complexity into models improve the agreement with the measured quantities. The modeled concentrations, however, are still typically larger than the measured ones. This article is a good addition to a growing set of CCN literature and in my opinion should be published in ACP after the authors have considered the comments below.

General comments:

The authors should use a term closure more cautiously. In the abstract a closure between the modeled and measured CCN number concentration is attempted, but the authors however, state in pp. 5641 that the aim is not to form a perfect closure but rather compare the measured and modeled CCN concentrations.

How sensitive are the modeled concentrations to particle composition? As the composition data is averaged to a typical diurnal cycle, some of the variability is bound to be lost. Does this affect the conclusion that the chemical composition is only of a minor importance to the predicted CCN concentrations? By adding complexity to the model, slope is approaching one, but the correlation coefficient is still less than 0.46. Does the averaging cause this?

As presented in Figure 10, during rush hour there is a lot of CN available, but the measured CCN remains quite stable. What is a diurnal cycle of e.g. $D_p > 100$ nm particle concentration? Placing the CN concentration in its own panel would give the CCN concentrations more space and make the figure more readable. How large are the errorbars in this figure?

The most discrepancies take place during morning rush hour when a lot of the freshly emitted non-activating appear in the spectrum. How accurate is the estimated HOA concentration and how sensitive are the results to this parameter?

Currently the cloud droplet model is slightly unconnected with the rest of the manuscript. I would suggest bridging the section 6 more closely with the excellent

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discussion in the previous section.

Specific comments:

pp. 5653 lines 12-14 Please give reasoning the selected values.

Figures 5-9 $S = 0.1$ is not discussed in the text due to insufficient time to grow in the CCNC. I see no point showing the figures either.

Figure 1 and 10 trim the legend boxes. Figure 13 add unit for the updraft velocities. Figure 15 case 2 does not have a symbol.

Different model runs should be consistently marked as C1-C5 or M1-M5. pp. 5644 line 10, M1 pp. 5645 line 2, M1 pp. 5651 line 6, M5

Correlation coefficient should be consistently capital R or lower case r throughout the paper.

pp. 5640 line 4, chemical? pp. 5648 line 5 ammonium sulphate pp. 5653 line 21 S_{sc} ?

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5629, 2008.

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