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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of common assumptions regarding aerosol composition and mixing state on predicted CCN concentration" *by* Manasi Mahish et al.

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Response to first anonymous reviewer

We appreciate the opportunity to respond to the reviewer's criticism of this manuscript. The review focuses on the important question of whether the manuscript adds enough to what is already known about the link between aerosol characteristics and CCN concentration to warrant publication. It does not offer specific recommendations for improvement. Thus, this response will be rather brief.

Reviewer's Comment:

In its current state the manuscript is of little relevance and utility for the atmospheric and CCN community as it does not present anything new, and the conclusions it does present are not well-explained or justified. The effect of mixing state on aerosol CCN activity has been examined in numerous previous publications, and the authors present a multitude of relevant references on the second page of the manuscript. The authors do not provide any information on how their study is different from the published ones, do not clearly state their objectives taking into account the already existing knowledge, and, therefore, fail to convince me that the presented study is new or important. It has long been known that aerosol mixing state plays a minor role in determining the ambient CCN and, even more so, cloud droplet number concentration CDNC, especially so in non-pristine regions (Moore et al., 2013). The effects of the total particle number and the size distribution are of much higher importance than the particle hygroscopicity or the mixing state (e.g. Conant et al., 2004; Dusek et al., 2006).

Answer:

We don't disagree with the reviewer's statement about the importance of the size distribution. Even so, the results summarized in Table 4 for the 3 scenarios for which particle hygroscopicity was assumed to be constant over time (20%, 50%, and 100% ammonium sulfate) show potentially large bias (4 – 81% for fixed composition vs. 1.4% for the best performing approach) and variability (average r2 of 0.70 for fixed composition vs. 0.86 for the best performing approach). Our goal was not to conclude that such differences were or weren't excessive, but rather to simply highlight the tradeoffs.

Reviewer's Comment:

On page 2, lines 21-24, the authors reference previous studies that have shown that NCCN is most sensitive to the particle size distribution and that assuming an internally-mixed aerosol is sufficient for an accurate NCCN prediction. The main conclusion of the study by Mahish et al. is mostly identical to this abovementioned statement, demonstrating the absence of any novel aspects in the study and deeming it a mere

repetition of the work that has already been done before.

Answer:

We believe that consistency of the findings presented in this manuscript with what has been presented in other publications should not preclude publication. Aerosol characteristics, sampling instrumentation, and analytical techniques vary widely among the publications that we cite and (undoubtedly) among others we did not. There are two noteworthy differences between the dataset we worked with and those used for the 11 publications on page 2, lines 21 – 24, the reviewer argues make this work duplicative.

1) The use of size-resolved growth factor distributions as the basis for the descriptions of mixing state and hygroscopicity. More so than for the datasets on which those other publications were based, we had a description of the actual size-resolved mixing state (at least a description of the mixing state that matters for this sort of analysis). The focus of the manuscript was a comparison of the CCN spectra determined when directly using the hygroscopicity distributions with those when the distributions were in some way averaged. And though a comparison with directly measured CCN spectra was included, it was only meant to show consistency among the measurements and was not the basis for conclusions about the most suitable description of the aerosol or about the error introduced as the full details contained in the measurements were simplified and averaged in different ways. So even if we arrived at the same conclusion as some of the referenced publications, we reached it following a rather different approach, which we feel makes this complementary of other analyses and not redundant.

2) The approximate mean and range of the duration of the datasets on which those publications are based are 22 days and 2 weeks - 1.5 months, respectively. Here we used an almost continuous 4-year dataset from a site at which there is considerable variability in aerosol properties and concentration over multiple timescales. In fact, the dataset used in this analysis is longer than those of the noted 11 publications combined. Of course we realize that more data doesn't necessarily mean better results,

but at least for SGP or sites like it the measures of bias and variability we report are more representative than if we had instead used just a month or so of data as with most similar studies. If given the opportunity to revise the manuscript we will more clearly articulate how our dataset and analyses differ from those used for other publications.

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