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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Cloud droplet activation of black carbon particles coated with organic compounds of varying solubility" *by* Maryam Dalirian et al.

Anonymous Referee #1

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General comments

The topic discussed in this paper is very important, and of interest to the community and the ACP readership. I found the paper to be very clear and well written, and the study to be carefully conducted in their experiments as well as in the analysis and interpretation of the results. Therefore, I suggest publication with only minor suggestions. I have a couple of general comments:

1. The BC particles were produced from a liquid suspension using an atomizer and a dryer before coating, this changed the morphological structure of the BC to a compact BC structure, as the authors discuss. It would help mentioning that the atmospheric

BC can have different degrees of compaction (there are a few studies showing this for laboratory, but also for atmospheric particles), and discussing briefly how the presence of more open-structured BC particles in the atmosphere might affect the conclusions of this study, and the models applied. For example, that might be relevant for fresh vs. aged BC particles. Related to this topic, a lot of work published in the literature on BC morphology, compaction, and coating is completely neglected here (in particular several electron microscopy, SP2, and optical studies from different groups around the world). I think that mentioning a few of these studies would improve the paper.

2. The BC particles were size selected with a DMA before being coated. This maintains the core constant while the coating thickness is increased. This approach is fine for the most part and produces interesting results. However, to untangle the effect of the coating hygroscopicity from that of size, it would have been interesting to size-select before and after the coating stage as well, to maintain the overall particle size constant, while changing the coating thickness; in this way, isolating the effect of the overall particle size (this would be particularly interesting for the case of the oleic acid). I am not suggesting the authors should conduct such experiments for this manuscript, as I think the results of the current study are very interesting on their own, but they could briefly discuss this possibility for future studies.

Specific comments

- Page 2 line 31, there are several studies, including some recent, that analyze and quantify the effect of coating, mixing and compaction for BC with the detail and unambiguity of electron microscopy, as well. Studies are available for both laboratory, as well as, ambient BC particles. It might be worth discussing some here.

- Page 3, line7, "of uncoated" seems not to belong here considering they are talking about multilayer models.

- Page 3, line 27. Please provide a sentence or two on why regal back and how does that represent (how well it acts as) a surrogate for atmospheric BC. This in addition to

the compaction issue mentioned in the general comments section.

- Figure 5: the theoretical calculation seems to perform less well for the larger core diameter; in fact, the range of the gray band does not seem to intersect with the experimental data even considering their uncertainties. Is there any reason for that? A short discussion would be interesting.

- Figure 6, maybe this was mentioned and I missed it, but why are the theoretical calculations so much narrower here than in figure 5? I guess the spread reflects directly the k range for the two coating materials, narrower for levoglucosan than for glutaric acid. Can the authors comment on that?

Technical corrections

- Page 3, line 13, consider adding the article “the” in front of Soot Particle Aerosol Mass Spectrometer

- Page 10, line 7, “procedure”, should be “procedure”, probably.

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